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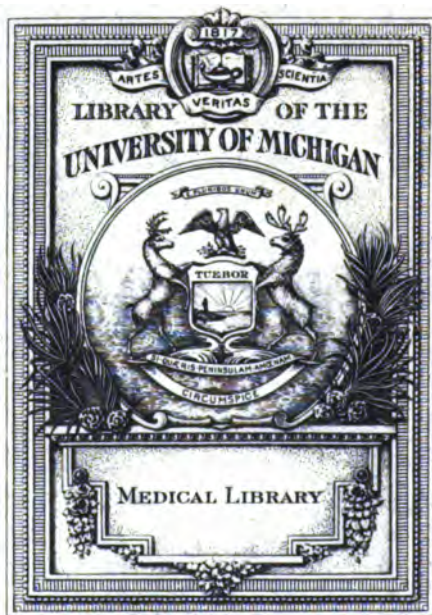
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LECTURES

ON INFLAMMATION,

(Delivered in the Theatre of the Royal College of Surgeons of England).

BY JAMES PAGET,

Professor of Anatomy and Surgery to the College.

LECTURE IV.

Degenerations and diseases of the lymph-products of inflammation.—General account of degenerations, as normal changes to which all living parts are prone; their relations to the other modes of natural nutrition, and their purposes; their difference from diseases.—Varieties of normal degenerations.

Characters of these degenerations when they affect inflammatory products in their several stages of development.—Degenerations of fibrine: its withering, liquefaction, disintegration, fatty, calcareous, and pigmental degenerations.—Degenerations of lymph-corpuscles; their withering, degeneration into granule-cells, and disintegration; their calcareous and pigmental degenerations: formation of various pus-cells, and their subsequent degenerations, as observed in the contents of chronic abscesses.—Degenerations of adhesions, and other examples of fully organised lymph.—Diseases of the products of inflammation.

In the last lecture a general history was given of the chief developments of the lymph exuded in the inflammatory process. I propose now to tell a corresponding history of its degenerations; and herein to describe what appear to be the transitions from the ordinary forms of lymph in its primary state,—its fibrine and its corpuscles,—to those many lower forms enumerated as molecular and granular matter, as pus-cells, granule-cells, inflammatory globules, and the rest. I said that, for the development of lymph produced in inflammation, it is requisite that the inflammation shall have ceased, and the conditions of healthy nutrition be restored. In the failure of this event, if the inflammation continue, or the due conditions of nutrition be in any way suspended, then, instead of development, degeneration may ensue. And this may happen in any of the stages

of formation which I described in the last lecture: it may happen alike to the rudimentary fibrine, or to the earliest lymph-cell, or to either, in any part of its progress to complete development.

Allow me first to illustrate generally what is to be understood by degeneration, as distinguished from disease, and to point out some of the larger principles which the very active inquiries of late years made enable us to enunciate concerning degenerations.

The usually admitted modes of normal operation of the nutritive processes are three—namely, development, growth, and maintenance or assimilation.* The accepted meanings of these terms are known; and I need only say concerning them that the maintenance of a part by assimilation must not imply the maintenance of an unchanged state, but a series of minor progressive changes slowly worked in the part, and consistent with that exercise of its functions which is most appropriate to the successive periods of its existence.

But to these three we should add, as another normal method of nutrition, the process of natural degeneration, such as occurs in advanced life. To degenerate and die is as normal as to be developed and live: the expansion of growth, and the full strength of manhood, are not more natural than the decay and feebleness of a timely old age,—not more natural, because not more in accordance with constant laws, as observed in ordinary conditions. As the development of the whole being, and of every element of its tissues, is according to certain laws, so is the whole process regulated, by which all that has life will, as of its own workings, cease to live. The definition of life that Richat gave is, in this view, as untrue as it is illogical. Life is so far from being “the sum of the functions that resist death,” that it is a constant part of the history of life that its exercise leads naturally to decay, and through decay to death.

Of the manner in which this decay or degeneration of organisms ensues we know but little. Till within the last few years the subject of degenerations was scarcely pursued: and, even of late, the inquiries, which ought to range over the whole field of living nature, have been almost exclusively limited to the human body. The study of development has always had precedence in the choice of all the best workers in physiological science. They who have devoted many years of laborious

* Lectures on Nutrition, in MEDICAL GAZETTE for 1847.

thought and observation to the study of the changes by which the living being is developed from rudiment to perfection, have given fewer hours to the investigation of those by which, from that perfection, it naturally descends into decay and death. Almost the only essays at a general illustration of the subject have issued in the ridiculous notion that, as the body grows old, so it retrogrades into a lower station in the scale of animal creation. The flattened cornea is supposed to degrade the old man to the level of the fish; while the *arcus senilis*, by a fancied correspondence with an osseous sclerotic ring, maintains him in the eminence of a bird; his dry thick cuticle makes him like the pachydermata; and his shrivelled spleen approximates him to the humility of the mollusk. One can only commend such day-dreams to the modern supporters of the doctrine of transmutation of species; and they might, indeed, form an appropriate supplement to their scheme, if they would maintain that, in these latter days, our species is destined to degenerate into lower and yet lower forms, descending through the grades by which, in by-gone times, it ascended towards its climax in humanity.

One cannot but wonder at the comparative neglect with which wiser men than these philosophers have treated a study so full at once of interest and of importance as this of the natural degeneration of the body. It could not be without interest to watch the changes of the body as life naturally ebbs,—changes by which all is undone that the creative force in development had achieved,—by which all that had been gathered from the inorganic world, impressed with life, and fashioned to organic form, is restored to the masses of dead matter,—to trace how life gives back to death the elements on which it had subsisted,—the progress of that decay through which, as by a common path, the brutes pass to their annihilation, and man to immortality. Without a knowledge of these things our science of life is very partial, very incomplete. And the study of them would not lack that peculiar interest which appertains to inquiries into final causes; for all the changes of natural decay, or degeneration, in living beings, indicate this purpose and design—that, being gradual approximations to the inorganic state of matter, they lead to conditions in which the elements of the body, instead of being on a sudden and with violence dispersed, may be collected into those lower combinations in which they may best rejoin the inorganic world; they are such, that each creature may be said to die through that series of changes which may best suit it, after death, to discharge its share in the

economy of the world, either by supplying nutriment to other organisms, or by taking its right part in the adjustment of the balance always held between the organic and the inorganic masses.

Nor would the student of the design of these degenerations do well to omit all thought of their adaptation, in our own case, to the highest purposes of our existence. When, in the progress of the “calm decay” of age, the outward senses, and all the faculties to which they minister, grow dim and faint, it may be on purpose that the Spirit may be invigorated and undisturbed in the contemplation of the brightening future; that, with daily renewed strength, it may free itself from the encumbrance of all sensuous things, or retain only those fragments of thought or intellectual knowledge which, though gathered here on earth, yet bear the marks of truth, and, being Truth, may mingle with the truth from Heaven, and form part of those things in which Spirits of infinite purity and knowledge may be exercised.

Moreover, the changes of natural degeneration in advanced life have a direct importance in all pathology, because the right understanding of them may guide us to the interpretation of many anomalies which, while they occur in earlier life, we are apt to call diseases, but which are only premature degenerations, and are to be considered, therefore, as defects, rather than as perversions, of the nutritive process, or as diseases only in consideration of the time of their occurrence.*

In this view, it will be a great help to our knowledge of disease if we can determine and arrange all the changes that mark the progress of natural decay or degeneration, i. e. the decay of old age, or of naturally abrogated functions. They seem to be these—1. Wasting or withering; the latter term may imply the usually coincident wasting and drying of a tissue. 2. Fatty degeneration, including many of what have been called granular degenerations. 3. Earthy degeneration, or calcification. 4. Pigmental degeneration. 5. Thickening of primary membranes.

Of each of these let me cite one or two examples.

Of *withering*, or wasting and drying, which is perhaps the commonest form of

* One can here have in view only the cases in which the degeneration affects the whole, or some considerable part of an organ; for it is very probable that some of the degenerations which we see *en masse* in the organs of the old, or in the seats of premature defect of nutrition, are the same as occur naturally in the elementary structures of parts, previous to their being absorbed and replaced, as it were by one particle at a time, in the regular process of nutrition.

atrophy, we have abundant instances in the emaciation of old age; in which, while some parts are removed by complete absorption, others are only decreased in size, and lose the succulency of earlier age.

Of the *fatty degeneration* in senility, we have the best proofs, as to its general occurrence, in the increasing obesity which some present at the onset of old age, and in the general fact that there is more fatty matter in all the tissues, and most evidently in the bones, than there is in earlier life; while, for local senile fatty degeneration, we find the *arcus senilis*, or fatty degeneration of the cornea, according to the interesting discovery by Mr. Canton,* and the accumulating fatty or atheromatous degenerations of arteries.

The *calcareous degeneration* is, in old age, displayed in the gradually increasing proportion of earthy matter in the bones; in the extension of ossification to cartilages, which, in all the period of vigour, had retained their embryonic state; and in the increasing tendency to earthy deposits in the arteries, and other parts.

The *pigmental degeneration* has its best instances in the gradually accumulating black pigment spotting and streaking the lungs; in the slate or ash-colour which is commonly seen in the thin mucous membranes of the stomach and intestines of old persons;† and in the black spotting of the arteries of some animals, in which pigment seems to hold the place of the fatty degenerations so usual in our own arteries.‡

Of the *thickening of primary membranes* we have indications in the usual thickening of the tubules of the testes, and, I think, of some other glands, as their function diminishes in old age; in the opaque white thickening of the primary or inner membrane of nearly all bloodvessels; and in the thickening of the walls of cartilage-cells in some forms of ossification. To this, also, we have a strong analogy in the thickening of the cell-walls of the heart-wood of plants.

Such are the changes which, singly or in various combinations, constitute the most evident degenerations of old age in man. Their combinations give rise to numerous varieties in their appearance, such as, *e. g.*, the increase of both fatty and earthy matter in old bones; the fatty degeneration and wasting of the pulmonary membrane in the

emphysematous lung; the dry, withered, and darkly tinged epidermis; the coincident fatty and calcareous deposits in old arteries. But I need not dwell on these, nor on the conditions which determine the occurrence of one rather than another mode of degeneration; for these I cannot tell. Let me rather observe that, taken singly, all the changes I have enumerated display certain characters, which should be present in every process that may be justly called a degeneration, and distinguished from disease. In all true degenerations these, or most of these, following characters should concur:—

First, and before all others, it should be a change naturally and usually occurring, in one or more parts of the body, at the approach of the natural termination of life, or, if not then beginning, yet then regularly increasing.

2. It should be a change in which the new material is of lower chemical composition, *i. e.*, is less remote from inorganic matter than that of which it takes the place. Thus, *e. g.*, fat is lower than any nitrogenous organic compound, and gelatine lower than albumen, and earthy matter lower than all these.

3. In structure, the form should be less developed than that of which it takes the place: it should be either more like inorganic matter, or less advanced beyond the form of the mere granule or the simplest cell. Thus the approach to crystalline form in the earthy matter of bones, and the crystals in certain old vegetable cells, are characteristic of degeneration; and so are the granules of pigment and of many granular degenerations, and the globules of oil that may replace muscular fibres or the contents of gland-cells.

4. In function, the part should have less power in its degenerate than in its natural state.

5. In its nutrition, it should be the seat of less frequent and less active change, and without capacity of growth, or of development.

Such are the characters of degenerations; and there are many things which show that the assumption of these characters is to be ascribed to a defect, not to a perversion, of the vital force, or of the conditions of nutrition.

Thus (a) these are all apt to occur in a part of which the functions are abrogated: a motionless limb wastes or becomes fatty as surely as an old one does. (b) They often occur, too, in parts that fail to attain the development for which they seemed to be intended. Thus, *e. g.*, fatty degeneration usually ensues in the cells of unfruitful Graafian vesicles.* (c) They bear also a certain

* Lancet, April 1850.

† It is remarkable that this accumulation of pigment should be occurring in any set of organs while pigment is disappearing from other parts; such as the hair.

‡ I have seen a similar black pigmental degeneration of the minute arteries of the human brain.

general analogy to the changes that ensue in some of the materials that are habitually excreted from the body; in the construction or composition of which materials one seems to have an instance of the gradual superposition of the ordinary or imitable processes of chemistry. (d) Again, they display living parts tolerating the presence and incorporation of inorganic or dead matter; fat being commonly infiltrated about degenerate cells and membranes, and earthy matter with it, even in the crystalline form. (e) And, lastly, and perhaps most clearly, the origin of degenerations from defective, rather than from perverted, conditions of nutrition, appears in the fact that one, at least, of them may be produced artificially. The fact observed by Dr. Quein,* that the fatty degeneration of muscular fibre is closely imitated when healthy dead muscle is placed in dilute acid, suggests, as the formation of adipocere did to Dr. Williams,† that, during life, the change of the protein-compound, which leaves the residuum of fatty matter, is one of common, that is, of imitable chemistry. Now we may be sure that where this chemistry prevails, there the forces or conditions of life are in defect.

From the whole, we may safely hold that, as the changes to which the several tissues are naturally prone in old age are the results of defect, not of perversion, of the nutritive process, so are the corresponding changes when they happen in earlier life; although, through their appearing prematurely, they may in this case bear the features of disease. Therefore, we should be prepared to separate from the catalogue of diseases all those changes in which we find the marks of degeneration alone; for the distinction of degeneration from diseases must be an essential, though often it may be an obscure, one. The one is natural, though it may be premature; the other is always unnatural: the one has its origin within, the other without, the body: the one is constant, the other as various as the external conditions in which it may arise: finally, to the one we are prone, to the other only liable.

I will now proceed to describe what, in accordance with the foregoing observations, appear to be the chief degenerations of inflammatory products. As I have said,

the degeneration may begin in any period of the formation of the lymph, as well in its rudimental as in its fully organised state; and that which determines for degeneration rather than development seems always to be the continued suspension of the conditions of nutrition.

The following appear to be the chief degenerations of the fibrine of lymph, or of the materials derived from its earliest stages of development:—

1. It may wither—wasting, and becoming firmer and drier, passing into a state which Rokitsansky* has designated *Aorwy*. One sees the best examples of this change of fibrine in the vegetations on the valves of the heart, or in the large arteries, when they become yellow, stiff, elastic, and nearly transparent.† The fibrine may, in this state, show no marks of development into tissue, but may have all the simplicity of structure of ordinary fibrine, being only drier and more compact. A similar character is nearly acquired when lymph is deposited over a lung which is extremely compressed in empyema, or in hydrothorax.‡ The tough dry lymph that here forms the greyish layer over all the lung is not always developed, though it may adhere firmly: it is withered, wasted, and dried (as the lung itself may be) in consequence of the compression.

2. Fibrine is subject to a degeneration which we may compare with fatty degeneration. In the coagula, or solid parts of effusions, that are found in the lower forms of inflammation, or in very unhealthy persons, the fibrine is usually not clear and uniform, and filamentous, but rather opaque or turbid, nebulous or dotted, presenting just such an appearance as marks the earliest stages of fatty degenerations in the muscular fibrils. In such coagula as these, also, one sees, not unfrequently, minute, shining, black-edged particles, which we may know to be drops of oil; while some general alteration in the composition of the fibrine is shown by its not being made transparent with acetic acid. In all such cases as these the fibrine is also very soft, and easily broken: it is devoid of all that toughness and elasticity which is the peculiar characteristic of well-formed fibrine; and by breaking it up, one may see the meaning of what one so often finds in the lowest forms of inflammatory exudation, such as occur in erysipelas and typhus,—namely, films and fragments of molecular and dotted substance, floating in fluid that is made turbid by them, and by abundant

* Lancet, Report of Medico-Chir. Soc., April, 1860.

† Principles of Medicine. Rokitsansky has always held that the fatty and earthy matters of degenerations are residues of the transformations which the higher compounds of the tissues have undergone; part of the products of the transformation being absorbed, and part (at least in many cases) remaining in the place. See, also, the Lectures on Nutrition, p. 40.

* Vol. i. p. 229.

† As in a case in the Museum of St. Bartholomew's, ser. xii. 55.

‡ As in the College Museum, No. 1756 A, and 1757.

minute molecules and granules and particles of oily matter. These represent the disintegration of fibrine that has degenerated after clotting, or has thus solidified in an imperfect coagulation. Of such changes, also, an excellent instance is presented in the softening and disintegration of the clots within the heart, which Mr. Gulliver* has described, and which might be studied for the illustration of the corresponding changes in inflammatory lymph.

We have examples of numerous varieties of this degeneration and disintegration of fibrine formed in inflammation. It is a principal constituent of most of what has been called "aplastic lymph," in inflammation of the serous membranes. Similar fibrine occurs, mingled with mucus, in the severer inflammations of the mucous membranes. And to the same source we may trace most of that molecular and granular matter which is usually mingled with pus formed by the suppuration of inflammatory indurations—with the variously changed corpuscles of "scrofulous matter"—or with the granule-cells, and other corpuscles of pneumonia, and the like inflammations. At least, this disintegration of fibrine is probably a frequent origin of such molecular matter; while the quantity of fatty matter present in pus and the products of pneumonia, and its gradual increase,† while pus is retained in an abscess, confirm the view, that the changes here described are of the nature of fatty degeneration.

When we see how a large mass of inflamed hard substance will become fluid, as it suppurates, and this with scarcely any, if any, increase of bulk, we may believe that another change of which fibrine is capable in degeneration is its liquefaction. In such a swelling as a hard bubo, or a hard chronic abscess, we may be nearly sure there is coagulated fibrine, both from the general circumstances of the inflammation, and because neither corpuscles alone, nor fibrine in the liquid state, would give such hardness. The suppuration, therefore, of such a swelling, if without increase of bulk, can hardly be explained, except on condition of the fibrine, which had coagulated, becoming again liquid. The occasional liquefaction of clots out of the body‡ makes this more probable; but I am not sure that it can be proved by any more direct facts.

I may here refer to a point of some practical importance connected with these

forms of degeneration of lymph, whether affecting fibrine or corpuscles. When the fibrine has withered and become dry, it is probably put out of the capacity of being further developed, and is rendered passive for further harm or good, except by its mechanical effects. But the molecular and fatty degenerations are yet more beneficial, in that they bring the lymph into a state favourable to its absorption, and, therefore, favourable to that which is termed the "resolution" of an inflammation in which lymph has been already formed. I suppose it may be considered as a general truth, that the elements of a tissue cannot be absorbed so long as they retain their healthy state. There is no power of any absorbent vessels that can disintegrate or decompose a healthy portion of the body: for absorption, there must be not only an absorbing power, but also a previous or concurrent change, as it were a consent, in the part to be absorbed; so that it may be reduced (or, rather, may reduce itself) into minutest particles, or may be dissolved. And this change is probably one of degeneration, not death, in the part; for dead matter is rather discharged from the body than absorbed.

Now there are some facts which indicate the probability that the fatty degeneration is that which commonly precedes the natural absorption of many normal parts; or, rather, that, in the change which they undergo before absorption, fatty matter is one of the products, and that the principal evident difference between the atrophy of a part which is manifested by its wasting, and the atrophy which is manifested by fatty degeneration, is, that the fatty matter, which is absorbed in the former case, is retained in the latter. However this may be, it is certain that the disintegration and fatty degeneration of the fibrine-products of inflammation bring them into a state most favourable for absorption; indeed, one may see in fibrine thus changed many things which, in regard to the fitness for absorption, make it parallel with chyle.* Of such absorption of fibrine we may find many instances. In rheumatic iritis we may believe the lymph to be fibrinous; but we see its complete absorption taking place; and the recent observations of Dr. Kirkes on the rarity of adhesions of the

* See also the ingenious contrast of the progress of chyle and the regress of pus drawn by Gerber, in his *Allgem. Anatomie*, p. 48.

One assumes that the absorption of visible particles is possible here, as it is commonly assumed to be in chyle; but the passage of such particles through the membranous walls of blood-vessels is only imaginable. Can it be that they pass into the vessels by being incorporated in the tissue adjacent to them, and then by being gradually shifted into them in the mutations of particles that are effected in nutrition?

† *Medico-Chirurgical Transactions*, vol. xxii.

‡ Gerber, *Allgem. Anatomie*, 48.

§ As in cases by Nasse and De la Harpe, quoted by Henle, in his *Zeitschrift*, B. ii. p. 169. See also Virchow on the same subject, in the *Zeitschrift*, B. iv. Henle refers to this same liquefaction the changes that ensue in emitted semen.

pericardium, in comparison with the frequency of pericarditis,* may be in the same manner explained. In rheumatic pericarditis we may be sure fibrine is effused; and the observed friction-sound has, in at least one case, proved its coagulation; yet in this case, when death occurred four months afterwards, scarce a trace of fibrine was found in the pericardium: it had been absorbed, and the degeneration I have been describing was probably the preparation for its absorption.

3. I am not aware of any direct proof of the calcareous degeneration ensuing in the fibrinous part of an inflammatory exudation; but we have the strongest evidence from analogy for believing that this change may be a frequent one. For there are numerous instances of calcifications of fibrine within the vessels—as, *e.g.* in the ordinary formation of phleboliths from clots of blood, in the branching and irregular pieces of bone-like substance found in obliterated veins, and in the lumps and grains of substance like mortar imbedded in fibrinous deposits in the heart's valves. We can, therefore, hardly doubt that the fibrine, even before development, may take part in formations of earthy matter in inflammatory products; but the calcareous degeneration is much more frequent in purulent fluids, and in the later developments of lymph.

4. Lastly, we have examples of the pigmental degenerations of fibrinous lymph in the various shades of grey and black which often pervade the lymph formed in peritonitis, and which are produced, not by staining or discoloration of the blood by intestinal gases, but, according to Rokitsky, by the incorporation of free pigment-granules.

Such appear to be the degenerations of the fibrinous lymph of inflammation: such at least are the changes in it which we may refer to defects in its power or conditions of nutrition, because they correspond with changes that may be traced in the gradual degenerations of old age. I need hardly say, that it is only by such correspondence that we can interpret them; for when we find them, it is altogether beyond our power to tell, by direct observation, whether, or in what way, the conditions of nutrition were defective.

The corpuscular constituents of lymph, in any of their stages of development, may retrograde, and then present degenerations corresponding, and often concurrent, with those which I have just described.

a. Their withering is well seen in some forms of what is called scrofulous matter,

such as occur in chronic and nearly stationary scrofulous enlargements of lymphatic glands. In the dull ochre-yellow-coloured and half dry material imbedded in such glands, may be found abundant cells, collapsed, shrivelled, wrinkled, glietening, and altogether irregular in size and form. One might suppose them to be the remnants of pus dried-up, or the corpuscles of chronic tuberculous matter, if it were not that among them are some with nuclei shrivelled like themselves, and some elongated and attenuated, which are evidently such as withered after they had been developed into the form of fibro-cells; into which form it is certain that neither pus-cells nor tubercle-cells are ever changed.

These are the best examples of withered lymph-corpuscles; but they may be also found in the pus of chronic abscesses, especially, of course, in that of such abscesses as ensue by suppuration of lymph-deposits like those just referred to. It may be hard sometimes to say whether corpuscles in these cases may not be pus-corpuscles shrivelled up: but, on the whole, I am inclined to believe that the shrivelled corpuscles of the pus of chronic abscesses are usually derived from the lymph, in which, having withered, they had become incapable of further change.

b. The fatty degeneration of lymph-cells is shown in their transition into granule cells.* We owe the first demonstration of this to the excellent observations of Reinhardt,† who has also shown how, by similar degenerations, corresponding forms of granule-cells may be derived from the primary cells of almost all other, both normal and abnormal, structures.

This method of degeneration appears peculiarly apt to occur in the inflammations of certain organs—as, especially, the lungs,‡ brain, and spinal cord; but it may be found occasionally prevalent in the lymph of nearly all other parts, and in the granulations forming the walls of abscesses or of fistule. It may occur alike in the early forms of lymph-cells, and after they have already elongated and attenuated themselves, as for the formation of filaments, and after they have degenerated into pus-cells. The changes of transition are briefly these:—The lymph-cells, which may have at first quite normal characters, such as I have assigned to “pri-

* The exudation-cells of Dr. Hughes Bennett; the inflammatory globules of Gluge.

† Traube's Beiträge, B. ii. 217, *e. s.*

Observations similar to part of those of Reinhardt were made independently by Dr. Andrew Clark (see MEDICAL GAZETTE, vols. xiii. xliii.)

‡ Dr. Gairdner describes also the formation of granule-cells from epithelium-cells in pneumonia (Contributions to the Pathology of the Kidney, p. 20).

mordial cells," present a gradual increase of shining black-edged particles, like minute oil-drops, which accumulate in the cell-cavity, and increase in number, and sometimes in size also, till they nearly fill it. The fatty nature of these particles is proved by their solubility in ether: and their accumulation is attended with a gradual enlargement of the cell, which also usually assumes a more oval form. Moreover, while the fatty matter accumulates, the rest of the contents of the cell becomes very clear, so that all the interspaces between the particles are quite transparent; and, coincidentally with all these changes, the nucleus, if any had been formed, gradually fades and disappears.

I need hardly say, that in these particulars the changes of the lymph-cells, (which may also occur when they have been already developed into the form of fibro-cells,) correspond exactly with those of the fatty degenerations observed in the cells of the liver or kidney, or in the fibres of the heart. There can be hardly a doubt of the nature of this process; and it presents an important parallel with the similar changes described in fibrine. For, we may observe, first, that where this degeneration is apt to occur in lymph, it is least likely to be developed. A proper induration and toughening of the lungs and brain, such as might happen through development of the products of inflammation, is extremely rare; it is scarcely seen, except in the scars by which the damages of diseases are healed. And, besides, this degeneration is a step towards the absorption of the lymph; for commonly we may trace yet later stages of degeneration in these granule-cells. They lose their cell-walls, and become mere masses of granules or fatty particles, held together for a time by some pellucid substance, but at last breaking up, and scattering their components in little clusters, or in separate granules.

Thus, if at no earlier period of their existence, or after no fewer changes, the lymph-corpuscles may pass into a condition as favourable for absorption as is that of the fibrine when similarly degenerate and broken up: and such as this, we may believe, is a part of the process by which is accomplished that "clearing up" of the solidified lung, which is watched with so much interest in pneumonia.

c. I may speak very briefly of the calcareous and the pigmentary degenerations of the lymph-cells. The former appear in cases such as Henle* refers to, in which granule-cells are composed not wholly of fatty matter, but in part also of granules of earthy matter. In this combination they correspond with

the common rule; for the fatty and earthy degenerations are usually coincident: they are combined in the advanced stages of the degenerations of arteries, and may be said to have their normal coincidence in ossification. Of the pigmentary degeneration of lymph-cells there are, I suppose, examples in the black matter effused in peritonitis: but the best examples are in the cells of bronchial mucus, to which I have already referred.

But I pass quickly by these, that I may speak of the degenerations of lymph-cells into pus-cells, or, more generally, of the process of suppuration that is connected with inflammation. The purulent is the most frequent degeneration of lymph, ensuing in nearly all cases in which it is placed in conditions unfavourable to its development—as, in the persistence of inflammation, or in exposure to air, or in general defects of vital force.

Of the conversion of lymph into pus we have numerous evidences; but a few may suffice for the proof.

1. The fluid of such vesicles as those of herpes is, in the first instance, a pure lymph, containing corpuscles which might be taken as types of the lymph- or exudation-corpuscles. If we watch these vesicles, we see their contents not increased,—rather, by evaporation, they are diminished; but the lymph is converted into pus, and pus-cells are now where lymph-cells were. And the changes may ensue very quickly: I think I have known it accomplished in twelve hours at the most.

2. In like manner, when we watch the progress of an abscess, we may find one day a circumscribed, hard, and quite solid mass, and in a few days later the solid mass is fluid, and this with little or no increase of bulk. Now the incipient solidity and hardness are due to lymph; the later fluid is pus, and the change is the conversion of lymph into pus.

3. The relation of lymph- and pus-cells is also shown in this that; they often cannot be distinguished from one another. Even in common suppuration of a granulating wound, the granulation-cells (which cannot be distinguished from lymph-cells) appear to be convertible into pus-cells; superficial cells being detached in pus, while deeper ones are being developed into filaments. But in worse-formed granulations, as in those lining a sinus or fistula, the cells are often by no characters, except by their forming a solid tissue, distinguishable from pus-cells. One may see the same conversion of lymph into pus thus illustrated: an amputation through the thigh was performed when all the parts divided were infiltrated with lymph effused in connec-

* In his Zeitschrift, B. II.

tion with acute traumatic inflammation of the knee-joint. Next day pus flowed freely from the wound. Now, in an amputation through healthy tissues, free suppuration does not appear till after three or four days: the pus here seen must have been formed by the conversion of the lymph previously infiltrated in the divided tissues.

These facts, while they may prove that the pus-cells are commonly the result of degeneration of lymph-cells, may also serve to show that the question whether pus-cells are ever formed in another way, or are ever an original or primary product of inflammation, cannot be positively settled. We cannot always discern a preliminary lymph-stage; but neither can we always distinguish lymph-cells from pus-cells, nor can we see in how very brief a time the transformation may be accomplished.

Assuming, however, that, when it appears as an inflammatory product, the pus-cell is always either an ill-formed or a degenerate lymph-cell, we may believe that, of the various forms which it presents, some depend on the peculiar defectiveness of the lymph; some on the state which the lymph-cell, previously well-formed, had attained before its degeneration began; and others on the further degenerations which may have taken place after the characters of the pus-cell have been acquired.*

I would especially observe, that in specimens of what might be called "good" pus, we may find three principal forms. There are—1st, some corpuscles presenting the peculiar and well-known granulated or wrinkled appearance of pus-cells, but from which water will raise up no cell-wall; 2d, from others, like these at first sight, water will raise a cell-wall, and will show that the former kind consist of only such a substance as forms the contents of these; 3d, in others, even when no water is added, a cell-wall is visible, and within this are granulated contents, with a more or less distinct nucleus imbedded in them. In all these forms, moreover, the addition of acetic acid usually displays a single or a bipartite or tripartite nucleus. Now, it may be that these represent three different stages of the pus-cell, either developing, or, more probably, degenerating; but I think it is much more likely that these forms are the results of the purulent degeneration beginning in lymph-cells at different stages of their development. There is so remarkable a correspondence between these three varieties of pus-corpuscles, and the three chief forms

which I described as observable in the development of the primordial cell of lymph, that one cannot but suspect that the three forms in the pus represent corresponding and similar degenerations from the three forms in the lymph.

When once formed, the pus-cells, if they are retained within the body, have no course but to degenerate further; it is characteristic of their being already degenerate, that they can neither increase nor develop themselves. The various corpuscles found in pus, besides those I have already mentioned, must find their interpretation in these degenerations; for the pus-cells are prone to all the degenerations that I described as occurring in the lymph-cells.

a. They may wither, as in the scabbing of pustular eruptions, or in long-retained and half-dried strumous abscesses.

b. Or, they may be broken up, whether before or after passing into the fatty degeneration, which is one of their most common changes, and in which they are transformed into granule-cells. It is this breaking-up into minute particles which, probably, precedes the final absorption of pus.

c. Or, lastly, both the cells and the fluid part of the pus may alike yield fatty and calcareous matter, and this may either remain diffused in fluid, or may dry into a firm mortar-like substance.

It is to such degenerations as these, in various degrees and combinations, and variously modified by circumstances, that we must ascribe the diverse appearances of the contents of chronic abscesses and of the substances left after their healing. In such abscesses we may find mixtures of pus-cells, granule-cells, and molecular matter, diffused in more or less liquid; or all cells may be broken up, and their debris may be found mingled with minute oily particles, which appear in all such cases to be always increasing; or, with these may be abundant crystals of cholesterine, or such crystals may predominate over all other solid contents. In yet other chronic abscesses (though, still, without our being able to tell why the pus should degenerate in these rather than in the foregoing methods), we find molecules of carbonate and phosphate of lime, mixed with fat-molecules and crystals, which are diffused in an opaque white fluid, and look like the deposit of lime-water, or like white paint; and as these contents dry, in the healing of the abscess, so are formed the mortar-like deposits and the hard concretions, such as are found in the substance of lymphatic glands, or other organs that have been the seats of chronic abscesses.

Time and patience would fail in an attempt to describe all the varieties of mate-

* It may be added that other rudimental cells, besides those of lymph, may be so altered as to resemble exactly the ordinary pus-cells. In many of the supposed cases of pus in the blood, the bodies taken for pus-cells were certainly only altered white blood-corpuscles.

rial that may thus issue from the transformations of pus. What I have enumerated are the principal or typical forms with which I believe nearly all others may be classed.

In conclusion of this part of the subject, a few words must be added respecting the degenerations which may occur after the lymph-products of inflammation are completely organised. All these degenerations, to which I have now so often referred, may be observed in fully-formed adhesions, or in the corresponding organised tissues in the substance of organs.

Of the wasting of adhesions we often see instances in the pericardium, where films of false membrane are attached to one layer of the membrane, while the opposed portion of the other layer is only thickened and opaque. A more remarkable instance, also, is presented in a case by Bichat, in which a man made twelve or fifteen attempts at suicide, at distant periods, by stabbing his abdomen. In the situations of the more recent wounds the intestines adhered to the walls of the abdomen; in those of the older wounds, the older adhesions were reduced to narrow bands, or were divided and hung in threads.

Of fatty degeneration I have seen no good examples in adhesions or similar inflammatory products, but of calcareous degenerations, or of such as present a combination of fatty and earthy matter, museums present abundant specimens. Among these are most of the plates of bone-like substances imbedded in adhesions of the pleura, in thickened and opaque portions of the cardiac pericardium, in the tunica vaginalis, in old hydroceles, in the thickened and nodulated capsule of the spleen, in the similarly altered mitral and aortic valves. So, too, many of the so-called ossifications of muscles and ligaments are examples of calcareous degeneration of fibrous tissue formed in consequence of inflammation of these parts, and imbedded in masses of fibrous-looking bands within their substances. In some of these cases, indeed, there may be an approximation to the characters of true bone; but in nearly all, the earthy matter is deposited in an amorphous form, and seems to take the place of the former substance, as if, according to Rokitsansky, it was a residue of the transformation of the more organised tissue, whose soluble parts have been, after decomposition, absorbed.*

Pigmental degeneration of adhesions may be seen, sometimes, in those of the

pleura, in which black spots appear like the pigment-marks of the lungs and bronchial glands.* Adhesions of the iris, also, may become quite black; I suppose by the formation of pigment like the uvea.

Such appear to be the chief instances of degeneration ensuing in the more or less developed products of inflammation. Doubtless, the account which I have given of them is very incomplete, and in its details sometimes erroneous; yet I venture to think it may be useful to my fellow-workers in pathology, by helping them to keep the right direction in our labours.

Among the signs of the attainment of complete membership in the economy we may enumerate this,—that the organised product of inflammation is liable to the same diseases as the parts among which it is placed; that it reacts like them under irritation; is like them affected by morbid materials conveyed to it in the blood; and like them may be the seat of the growth of new and morbid organisms. No more complete proof of correspondence with the rest of the body could be afforded than this fact presents; for it shows that a morbid material in the blood, minute as is the test which it applies, finds in the product of inflammation the same qualities as in the older tissue to which it has peculiar affinity.

The subject, however, of the particular diseases to which these substances, themselves the products of disease, are liable, has been little studied. As I have already said, lymph, while it is being highly organised, is often the seat of hæmorrhage; its delicate new-formed vessels bursting under some external violence, or some increased pressure, and shedding blood. Such are most of the instances of hæmorrhagic pericarditis, and other hæmorrhages into inflamed serous sacs.

Even more frequently, the lymph, when organised, becomes itself the seat of fresh inflammation. Thus, in the serous membranes, we may find adhesions in the substance or interstices of which recent lymph or pus is deposited; or, in other cases, adhesions, or the thickenings and opacities of parts, become highly vascular and swollen. It is, indeed, very probable that, in many of the instances of the recurring inflammations that we watch in joints, or bones, or other parts, the seat of the disease is, after the first attack, as much in the organised product of the former disease as in the original tissue.

As an example of the inflammation of organised inflammatory products, I

* Numerous specimens of the calcareous degeneration of adhesions were shown from the College Museum, including Nos. 103, 1493, 1494, 1516, 1548, 2367, 3456, &c.

* As in No. 96 in the College Museum.

may present a specimen which has some historic interest.* It is one of those by which attention was first drawn to the connection between acute rheumatism and disease of the heart. It was presented to the College by Sir David Dundas, in 1806, and he sent with it a letter,† in which he says that it exemplifies a disease of the organ "which is not described by any author that I am acquainted with, but which, from the number of cases which have fallen under my observation, is, I apprehend, very frequent in this country. The most remarkable circumstance of this disease is its being always connected with, or subsequent to, an attack of acute rheumatism." In relation to the subject of which I was speaking, the specimen has this point of further interest, that it shows such a deposit of recent lymph among the old adhesions as, we may be sure, was derived from their vessels.

I suppose, also, that to such inflammations of organised inflammatory products we may ascribe many of the occasional aggravations of chronic inflammations in organs—the renewed pains and swellings of ankylosed joints, of syphilitic nodes, and the like,—which are so apt to occur on exposure to cold, or in any other otherwise trivial disturbance of the economy. In such cases we may believe that the former seat of disease is again inflamed, and that with it are involved the organised products of its former inflammations. And in such cases there are, perhaps, none of the effects of inflammation which may not ensue in the newly organised parts: evidently, they may be softened, or thickened and indurated, and made more firmly adherent: or they may be involved in ulceration, or may slough with the older tissues among which they are placed.

Lastly, the products of inflammation may be the seats of the morbid deposits of specific diseases. In their rudimentary state they may incorporate the specific virus of inoculable diseases, such as primary syphilis, variola, and the rest; and when fully organised they may be the seat of cancer and tubercle. But on these subjects I have not time to dwell, although their pathology, especially as illustrated by the tuberculous diseases of serous membranes, is full of interest.

* Museum of the College, No. 1512.

† Inserted in the Pathological Catalogue, vol. iii. p. 183: to it are added an account of the previous notices of the facts referred to by Dr. Jenner, Dr. Baillie, and M. Odier.

Original Communications.

OBSERVATIONS ON THE CONDITION OF THE BODY AFTER DEATH FROM CHOLERA.

By WM. FREDERICK BARLOW, M.R.C.S.
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Hospital.

(Suggested by a Case communicated to the
Writer by THOS. GREEN, M.D. F.R.C.S.,
Surgeon to the Bristol Infirmary, &c.)

AN extreme apathy has, as might have been anticipated, long since succeeded to the intense excitement which prevailed whilst the cholera was raging; and there are many who may think it an injudicious moment to ask attention to anything respecting it; but I venture to address myself to those who, not behaving as though it were absolutely certain never to return, still contemplate its phenomena, and confess how much many of them need more research.

During October last I read an account in the *Times* of an "extraordinary case" of cholera, which happened at Bristol. It was described as "a supposed case of catalepsy," and subsequently as one of "suspended animation." It was reported to have "exhibited, perhaps, some of the most singular phenomena" on record. It was affirmed, too, that the subject of it had been heard to say, "that if he should die of cholera, he should like to be stabbed in the heart, so that he might be certain not to be buried alive." Rumours of every sort were spread on all sides: the public listened readily, as it always does,—was alarmed needlessly, as it often is.

The death which so many fancied to be simulated was, in truth, real all the while, as real as any death ever was, whether by cholera or other malady. An inquest was held upon the body after decomposition had become evident, and the fact of dissolution could be denied no longer. At this inquest "the utmost interest was manifested, and the jury-room was crowded, as well as the approaches to it." A great check must have been given to the

appetite of the marvelling, by the Coroner observing that "the usual signs of death were well known, and, though some of these signs were wanting, it did not follow that death had not taken place."

The body had very long remained warm, and rigor mortis had been extremely tardy, and the Coroner remarked, on these points, that "the absence of coldness and rigidity frequently occurred, but there could be no doubt that death had taken place."*

Dr. Wallis and Mr. Kelson, two of the medical attendants of the deceased, gave evidence during the inquiry which went to prove how little true cause had existed for the extravagant excitement which the case had occasioned; but at the same time there were circumstances stated by them of considerable medico-legal interest.

Desirous of the fullest particulars, I wrote to Dr. Green, Surgeon to the Bristol Infirmary, who also was consulted respecting the case, and he was so kind as to send me an account, which I have taken the liberty of abridging.

It appears that Mr. B. B.—h went to his counting-house at 9 A.M., Oct. 12, 1849, and returned at 10, feeling ill, with severe pain, vomiting, and frequent watery evacuations. Some medicine and brandy were prescribed him.

At 4 o'clock there was great pain in the abdomen and legs; the voice was feeble; there was no pulse at the wrist; the face was pale and collapsed; the eyes half closed and sunken; the mind undisturbed. No urine was passed; there were vomiting and the characteristic rice-water evacuations; he would "roll in bed" with the intensity of the cramp. The tongue, however, felt warm; so did the skin and extremities.

A scruple of calomel was ordered to be taken, and a draught containing opium and ammonia, acetate of lead with small doses of morphia, external warmth, a mustard poultice to the whole abdomen, &c., were subsequently had recourse to.

When seen by Dr. Wallis, Dr. Green, and Mr. Kelson, at a later period of the day, there was a relief obtained from the vomiting, purging, and some from the cramps, whilst the skin kept warm; but still there was no pulse at the

wrist. Difficulty of breathing, with intense pain through the waist (spasm of the diaphragm?) was noticed.

When visited at 10 o'clock, this pain was relieved; the dyspnoea had ceased; there had been no return of the vomiting and purging; the skin was warm and perspiring; but there was scarcely, if any, pulse at the wrist: and so he continued until about half-past 1 o'clock in the morning (Oct. 13th), when, his bowels pressing for relief, he would get upright (in spite of the nurse, and contrary to what had been strictly enjoined him), passed a watery evacuation, and died in about twenty minutes.

"At 6 A.M., Oct. 13th," says Dr. Green, "I saw the body: the skin was warm; limbs not rigid; features not collapsed. I directed that he should not be buried until I had seen him again.

"At 6 P.M. I found he had been screwed down in his coffin: the lid was removed by my direction, the body taken out, and found still warm, and in the same condition as in the morning. The entire body and extremities were then closely packed in saw-dust

"Oct. 14th.—Had remained during the night in the saw-dust; the warmth still continued; there was no rigidity; a vein in the arm was opened, but no blood came.

"At 12 at noon, tepid salt and water were injected slowly in a vein of the arm; some frothing of the mouth followed; * next powerful galvanic shocks were passed from the back of the neck in the direction of the heart for half an hour: no other means were used.

"The saw-dust was then removed, and the body laid out in the usual way.

"15th.—Heat less.

"16th.—Body cool.

"17th.—Body quite cold; limbs rigid; appearances of decomposition over the abdomen.—To be interred."

An account of the post-mortem examination was given at the inquest by Mr. Kelson: it presented nothing remarkable.

As to the measures which were resorted to with the apparent view of restoring animation, it will naturally be asked why they were not earlier employed? The answer is, that the me-

* The words "absence of coldness and rigidity" are evidently intended to imply only unusually long delay.

* This is commonly enough noticed in dead bodies on their being disturbed or moved.

dical advisers never expected any good from them, and only used them to satisfy the friends of the deceased. They never doubted that the death was real, but were placed in difficulty by the unusual circumstances which surrounded it.

Dr. Green informs me that he considers the patient hastened his death by the effort to raise himself upright. Very likely. Many victims of cholera have suddenly perished in the moment of exertion, and the same may be said of other diseases which severely prostrate. In 1832 I was called to a man who lay in the collapse of cholera: he was cold, livid, dying. On a sudden, and without my being able to prevent him, he rose from his bed, would have fallen on the floor had I not supported him, and died in an instant. I could cite other like instances, were it necessary.

In the history before us there was nothing during life-time, as the late epidemic will fully prove, remarkable in any way; and as to what happened after death, there was nothing which should have set people marvelling to the extent they did, making them most anxious for the daily journals, that they might learn what had transpired in the "Bristol case" since the last report. But there were undoubtedly certain circumstances of no trivial interest to the scientific inquirer, which it may be well to dwell upon shortly, and the rather as a misconception of them may tend to the extension of erroneous views, and, perchance, at some future time, cause like events to be wrongly canvassed.

That a body dead on the 13th of October, and so early as half-past 1 in the morning, should not have been "quite cold" until the 17th, on which day rigor mortis seems to have first shown itself, and signs of putrefaction to have been first seen, is a fact which certainly requires comment. I could wish the task were in abler hands.

Now was there anything in the situation of the body subsequent to death which could account for any of the phenomena observed? Was the corpse covered or not?—exposed to a high or low temperature?—to a tolerably or a quickly moving air? Was ought to explain an uncommon absence, or the absence of a common

There were four points in the which principally attracted attention.

1st. The long retention of wa-

2d. The deferred rigidity.

3d. The delay of putrefaction.

4th. The aspect of the features, of which nothing has been said at present: it was placid, like that of the sleeper.

As to the first matter, it will doubtless have struck the reader that the packing of the body in saw-dust had, in every probability, much to do with the retention of the heat. It would be against reason not to conclude so; and such is the opinion of Dr. Green, Dr. Wallis, and Mr. Kelson; such that of Dr. Alfred Taylor and others to whom I have mentioned the facts of the case. Still, it seems clear that there was something not a little uncommon in the temperature of the body, and that the long-continued heat was far from being altogether owing to the bad non-conductor wherewith it was surrounded: the corpse, as we see by the account, had less heat on the 15th, was called "cool" on the 16th, and not pronounced quite cold until the 17th, though it was removed from the saw-dust about noon on the 14th. Inferring from Dr. Green's narration that the face was left uncovered whilst the rest of the body was completely enveloped in the saw-dust, I made inquiry of that gentleman whether the exposed part did not become more quickly cold than the rest of the body. Dr. Green says, in a note, "I took care that the face should be entirely uncovered. The next morning there was not much, if any, difference between the temperature of the face and other parts; but on the morning of the 15th the face was decidedly colder than the rest of the body." From this we may gather that the saw-dust operated in keeping, or helping to keep, the body warm: indeed, it could not have done otherwise; but the corpse appears to have been very slow in cooling even when there seemed nothing to prevent its doing so. I asked Dr. Green to inform me of the circumstances under which the body lay subsequently to its being removed from the saw-dust, of the temperature of the atmosphere meanwhile, &c., and I learn from him that it was laid on a board, dressed as are usually, and covered with a sheet. The room in which it lay was at night. The following day the window was open in the day. The temperature of the weather at the time was about 50° in the shade. In five days the body was found dead:—

Saturday, Oct. 13th, 45°, wind East.
 *Monday " 15th, 45° " East.
 Tuesday " 16th, 47° " E. S.E.
 Wednesday 17th, 60° " W. S.W.

From hence we may conclude that the body was slow in cooling even when by no means unfavourably situated for losing its warmth quickly, when the temperature was moderately low, the wind being in the east the while, when the window of the room was open at day-time, and the body lightly covered.

No doubt it would have been very instructive to have had some observations made carefully by the thermometer in the case of Mr. B——, setting forth plainly a true comparison between the heat of the body in life and death, and marking to a nicety the slow degrees by which it lingeringly left the body; but it is not surprising that they were not effected, seeing how every moment of the time of most practitioners was occupied during our late dreadful and memorable visitation of cholera, wherever the pestilence thickly raged. And I must be permitted to say, that it would be absurd to put aside all observations on the ground that they might have been exacter and more delicate. As a rule, we know by our touch when bodies are hot and cold; and in taking the temperature of patients under different circumstances, the thermometer is continually verifying what we have ascertained, though not with enough minuteness, by the hand, which, to test a matter of this kind, is confessedly an infinitely ruder instrument. Persons have felt the bodies of these dead by cholera, and given a *concurrent* testimony that they were warmer, greatly warmer, than in life-time, and surely we must admit their evidence notwithstanding no thermometer was used.† I know that some of the profession have been summoned to the dead by reason of their growing warm; they obeyed the call as one of the fancy, but were soon obliged to confess the existence of, to their minds, a strange and novel phenomenon.

In the case before us it is not stated

* Sunday was not mentioned; but the temperature observed at Greenwich was, highest, 46°, wind N.E.; lowest, 37°, wind N. N.E.

† I presume in almost all cases where evidence is given upon inequities and trials, that where the warmth or coldness of the dead is mentioned by the medical witness, he refers to what he ascertained by the touch. The thermometer should certainly be oftener used.

that there was any *rise* of heat; the long duration of it was the striking circumstance. Perhaps the same phenomenon would have been seen in other instances of cholera had they been patiently, enduringly watched. But as far as my own experience goes I have nothing to contribute on this head. As to what usually happens after various deaths, Dr. A. Taylor says—"it is very rare that the heat of the body is preserved for so long a period as twenty hours. In general it becomes cold within ten or twelve."* This observation, however, only applies, and is doubtless intended only to apply, to the external parts of the body. Organs and strictures imbedded deeply hold their warmth long after the surface, exposed perhaps to the cold air all but entirely, has parted with its heat, or with so much of it as to chill the hand of living persons. A little inquiry has told me that the thermometer often exhibits the presence of a degree of heat far higher than that of the surrounding atmosphere when the body strikes cold to the touch, and that it proceeds to lose it very gradually.

In inquiries into the remaining heat of the dead the relative temperature of the different parts should be accurately taken: it is not enough to examine the axilla, and leave the matter there. In a girl, aged 18, a sufferer from morbus Brightii, whereto succeeded diffuse bronchitis, coma, and death, I found, 24 hours after dissolution, the atmosphere being of the temperature of 40°, that the thermometer proved 60° of heat in the axilla, whilst 72° were found in the kidney. In another instance, wherein there was also renal degeneration followed by a rapidly fatal apoplexy, there was a still more considerable difference between the heat of the kidney and the axilla. But let me refer to some remarks, by Dr. Davy, on the temperature of the body after death, in which the comparative amount remaining to various organs was minutely examined. The following is one of the instances which he cites:—

"—, aged 29; died of pulmonary consumption on the 26th January, and was examined eighteen hours after. There was great emaciation; little blood; there were the ordinary lesions

* See his instructive and valuable Lectures on the Signs of Death, published in the MEDICAL GAZETTE. Vol. for 1827 and 1828.

in the lungs, with empyema of left pleura, and ulceration of ileum and cæcum.

	Deg.
In longitudinal sinus (calvaria unusually thin)	55
In lateral ventricle	55
In upper part of spinal canal	55
Under integuments of sole of foot	48
" " dorsum of foot	44
" " thigh, close to artery	52
In purulent fluid of left pleura (48 oz.)	64
In right ventricle, moderately distended with blood, partly liquid, which afterwards coagulated	63
In left ventricle, empty	64
In central substance of right lobe of liver	65

The observations were made in succession, without interruption, in the space of five minutes. The temperature of the room the greater part of the time was 40°.* It appears from this and other observations that not only must the external and internal parts of the body be contrasted, but the latter compared one with the other. *Cæteris paribus*, the most exposed portions most rapidly cool. How could it be otherwise? Make the skin internal and the heart external, and the latter loses its heat the more quickly. The uncovered arm of the dead is soon frigid, the covered preserves its heat. The other day a man was brought into the Westminster Hospital completely dead. He had strangled himself with a small rope which had left the deep mark of its effectual compression. His ordinary dress was on him; the parts enveloped by it were warm, but the hands and other exposed portions of the body were quite cold. In the dying as in the dead we note frequently the effect of external cold, and the partial failure of animal heat is a not uncommon foreshadower of dissolution; the paralytic limb is warm or chilly, as it is laid open to the air or not; and the leg robbed of blood, though temporarily, by Hunter's operation shows the same fact. These are points known to all, but perhaps dwelt upon to the full by few. There is a multitude of occasions wherein the effect of exposure and covering must be well accounted for. *Imparted* must be distinguished from *self-generated* heat. A part will mortify despite the warmth

that is not its own; it can be raised in temperature as dead matter may. There may be life with extreme coldness, death with much warmth. The questions are, what has led to the production of either state—transient or permanent? and how is it influenced in every way?

The temperature of the body after death by disease in general has been hitherto but very insufficiently considered; and the consequence is that we are but ill prepared to treat of the matter as respects cholera. Yet the subject is of too great interest to justify our ignorance, is no doubt intimately connected with the phenomena exhibited by the disease in life-time, and has, probably, far more numerous, various, and useful bearings than we should make bold to guess. But as far as the profession itself is concerned, it is certainly incumbent that it should acquire some knowledge of the question: it should be prepared, at any rate, to be more informed than those who may consult it in legal inquiry.

The most curious matter connected with the subject is not the *maintenance* of temperature, but its *rise*.

The editor of the Cholera Gazette for 1832, remarks, in a note, that the warmth of the body increases long and remarkably after death; to him it was clearly no rarity, and yet I could point to most able writers on the scourge referred to, who devote not a syllable to the matter, and could also mention some of my acquaintances who have had a very unusual experience of the disease, and have never observed the phenomenon.

Cruveilhier, in his famous work on pathology, speaks of the coldness in this disease being less intense in the dead than the living, stating:—"Le froid de la peau est moins intense sur le cadavre que sur le vivant, ce qui tient en grande partie à l'absence de cette sueur visqueuse qui inonde la peau pendant la dernière période du choléra: chez plusieurs sujets, au bout de dix-huit heures, la chaleur du tronc s'était maintenue à peu près comme dans l'asphyxie."

The circumstance of the heat thus augmenting, and being retained thus long, is certainly one which I think could hardly have been anticipated in a disease wherein in life-time there is so general and signal a failure of the animal warmth,—which in fact takes

* See Researches, Physiological and Anatomical, by John Davy, M.D., F.R.S., Vol. i. p. 262.

rank amongst the most prominent and ominous features of this dreadful affliction. Strange, that the body which feels so cold in life, that we all but shrink from the icy chill of its impression, should, after the last pulse has beat, the last breath been taken, impart so much warmth to the toucher, alarm perhaps some anxious relative who, with senses made keen by affection, bends himself solicitously over the body, sure to discover anything unusual, and make the most of it when known.

What means this manifestation and retention of heat? With what is it associated? Is it connected with those singular disturbances of the muscular fibre whereby the face is agitated and the limbs moved? I have in a former essay referred to a case in which the heat rose and the muscles contracted. Dr. Taylor makes mention of another—and more, doubtless, have been seen—in which this elevation of the animal heat and marked signs of muscular irritability were beheld simultaneously; but there is no reason to believe that they appear together as any other than *coincident* events. The motion has often happened without the heat, the heat without the motion.

In the two following examples which occurred last year, under the notice of Mr. Hunt, who communicated them to me, there were no contractions noticed, though the rise of heat was palpable enough.

Ellen Connor, æt. 50, died at 49, Duck Lane (a place full of dirty, crowded, ill-ventilated apologies for dwelling-houses, and in which the late epidemic raged frightfully), of an attack of cholera, after being ill twenty hours; she was extremely cold in life-time, but after death was found much warmer than her son, who, stricken with the same malady, lay beside the corpse of his mother, and died three hours afterwards. Mr. Hunt was told by the relatives of the man, that his body, also, became warmer after life was past.

Jane Welch, æt. 7 years, died of the same disease, after an attack of 12 hours' duration. Mr. Hunt examined the body carefully in life-time, and it everywhere struck him with a "sudden chill." But after the child was dead two hours, he was sent for hastily by the parents, who were wishful to ask him if it were not living, for the body had become warm and the cheeks somewhat red-

dened. Mr. Hunt placed his hands upon the body and felt a "glow of warmth," and was for a moment inclined to hope, but soon convinced himself how irrecoverably the heart and respiratory muscles had ceased to act.

It cannot be questioned that very many instances of rise of temperature altogether escaped observation; first, because so many of the dead were not carefully examined; secondly, because the mode of examination by touch in some of the cases wherein it was employed, could not have measured those *slighter* elevations of the temperature which may be assumed to have occasionally happened. The extreme examples were no doubt detected readily; even the most ignorant could find them out.

Dr. Taylor refers to some instances in which the thermometer was used:—"In many cases of death from cholera in 1832-3, the body which had become moderately cold was observed suddenly to resume its warmth, so that the temperature is stated to have risen some time after death, as high as 87°, although the circulation and respiration had entirely ceased."

As to the cause of the *post-mortem* warmth, Cruveilhier, as we have seen, inclines much to refer it to the withdrawal of the cooling influence attendant upon the evaporation of moisture from the surface which took place in life-time. This view may be sometimes capable of explaining the lessened coldness to *some extent* in marked cases of increased heat, but not nearly to a degree that is satisfactory. Have any observations been made to show that those bodies which were most bedewed with cutaneous exhalation exhibited after death a higher temperament than others which were less moistened by it? But in reference to its refrigerating effect in life-time, we must remember how closely and thickly many of the bodies were carefully covered, so that a free evaporation from the surface, whereby heat could largely and quickly escape, was effectually prevented.

At the time of death by cholera there is an amount of internal heat which must be diffused and become external ere the body be cold throughout; but surely it cannot be successfully contended that enough remains to explain a very palpable rise and a much more than common retention of warmth. As

it is diffused so it is diminished; when it gains the surface it must be of less intensity than it was before. Other circumstances being equal, the warmer the body at the instant of death the longer does it take to cool: some lose their heat in great part before they die; not so others, certain of those, for instance, destroyed of a sudden by disease, accident, or purposed violence. In reference to the case which occurred in Bristol, it must be recollected that the patient was warm when he died, and further, that his dissolution was apparently hurried by a sudden effort: had he lived longer, so as more gradually to perish, it is very doubtful whether the heat of his body would have lingered so long as it did. The quicker the dissolution the more likely is the temperature to be long retained,—the more probable is it, also, that rigidity will be postponed, and decay retarded.

Since the rise of temperature which occasionally happens after death by cholera is found often in the absence of muscular contractions, it would be out of question to gravely refer thereto by way of accounting for the production of the heat. And were the heat never found except in combination with the movements of the muscles, it would still be futile to appeal to them to explain the cause of warmth. There is nothing in the observations of Becquerel and Breschet, who found that the temperature of a muscle increased 2° by violent exercise, which can show reason for any considerable augmentation and duration of heat. Besides, we must have a due care not to apply indiscriminately to the dead what has been discovered to happen in the living, as to change and amount of temperature. For animal heat bears a certain relation to the energy of the circulation, and the blood speeds more swiftly when the muscles play. But mere muscular action when no blood is circulating,—such, for example, as may be brought about by galvanism when life has left,—does not, in every probability, originate any thing like that increase of warmth which would be generated by motion under different circumstances. How cold the cholera-stricken are in life-time, despite those horrid and pertinacious cramps which violently, and sometimes almost universally, pervade the tortured frame!

Has putrefaction any concern in the production of the increased temperature

after fatal cholera? In some intestinal affections it is well known that putrefaction happens early: in cases of obstruction and puerperal fever decomposition has very hastily appeared; but in cholera I never remember to have noticed any unusually rapid signs of decomposition on the surface of the body. Cruveilhier asserts that “the putrefaction of the body is slow in this disease, as in all subjects drained of their blood; but that the putrefaction of the alimentary canal is, on the contrary, very rapid, as in all cases of considerable sanguineous congestion of the digestive organs.” Generally speaking, the obvious putrefaction of the dead is delayed until they have become cold: they are warmer at the moment of dying than at any time subsequently. Did the rise of temperature after death from cholera begin later than it does, keep pace with advancing putrefaction, become most intense where this process was most manifest, there might be better grounds for speaking of decomposition as the cause of the heat than seem to exist at present. Every one knows, who has been much in the dissecting-room, that bodies in the very height of putrefaction will strike the hand with no sensible degree of warmth. The degree of heat where the body sometimes rises after death is a subject likely to puzzle the philosopher, and for the reason, partly, that we are rightly wont to connect augmentation of temperature with increased nervous energy and accelerated circulation. When we lay our hand upon the dead body, and find that it has become warm, we are naturally surprised at a circumstance so very unusual. “The power of generating heat,” says Mr. Hunter, “seems to be a property in an animal while *alive*.”* But heat is, in some mode or other, generated in the dead sometimes, and, as I am persuaded, otherwise than by putrefaction. Chemical changes, of which the chief is the formation of carbonic acid, do, as it seems to me, go on in the body after dissolution, and give rise to that amount of temperature which is occasionally noticed.

* See Experiments and Observations on Animals with respect to the power of producing heat. Hunter's Works, Palmer's Edition, vol. iv. p. 131. Some of the many difficulties wherewith the subject is surrounded are most emphatically set forth, and the contribution abounds in the most invaluable suggestions as to further investigations.

Hitherto we have thought that the body of the dead when once cold invariably remains so, but we must remember how little, how superficially, the condition of corpses as to temperature has been investigated. In this, as in every other matter, the chief cause of ignorance has been deficient inquiry. Dr. Taylor observes, in reference to the state of the temperature of the dead by cholera—"It is scarcely to be imagined that the function of calorification should for an instant continue in the really dead; and yet certain facts connected with malignant cholera would almost seem to establish the possibility of this." But further on he says—"Possibly a sudden vital reaction in the nervous system may have given rise to the effect observed."*

Mr. (now Dr.) Mayo remarks in his *Outlines of Pathology*—"Magendie antithetically observed that Asiatic cholera begins where other diseases end—in death. And certainly many of its features impress us with the idea of some extraneous and peculiar force of prostration which extinguishes life; and—which is, indeed, most wonderful—seems to act upon life by depressing without exhausting the vitality of the organization. This language, I am aware, is too fanciful for sober physiology; but it is suggested by certain remarkable features of the disease yet unadverted to. It is well attested that in many cases of the most formidable type of cholera, when the patient for two or three hours before death has been cold, blue, motionless, the limbs stiffened with cramp,—upon death taking place the limbs and the jaws have begun to move in slow convulsions, the skin has become sensibly warmer, and the blue tinge has gone off, changing to a mottled red,—as if the organization, which had been borne down during the struggle between life and the disease, could temporarily re-exert its forces when the pressure which extinguished life had ceased."

But *what* "forces of the organization" are thus "temporarily re-exerted," and *how*? Is the heat, as to formation, simply similar to that of life-time, in being dependent on the like chemical combinations, though they take place under completely opposite circum-

stances? Supposing carbonic acid to be formed, whence comes the oxygen that combines with the carbon,—how are its elements brought together that they may unite? Does the oxygen exist within, or does it proceed from without, furnished in abundance by the air itself, as a distinguished animal chemist* will be seen to suggest presently? Is the elevation of temperature confined to the *skin*, or is it diffused wherever there are vessels? Is it always accompanied by a change of colour on the surface, by that mottled red which Mr. Mayo mentions, and which I have had occasion to observe myself? How much, how very much, remains to be explored ere we can gain any satisfactory knowledge of this subject! The late epidemic, with its thousands of deaths, has scarcely taught us any thing about it.

Knowing that Dr. Garrod had made much inquiry into the state of the blood in cholera, I requested him to have the goodness to state his view of the most likely way in which he considered the rise of heat after death to be occasioned. As it will be better to lay his own words before the readers, I beg attention to a literal copy of a letter wherewith I have just been obliged by him.

"63, Harley Street, April 1850.

"MY DEAR SIR,—You ask my opinion as to the probable cause of the increase of temperature observed frequently to occur in the bodies of patients who have died of cholera. I think, in the first place, that it is impossible that putrefaction should have any share in the production of the phenomenon, as the time at which it occurs shows; and also the fact that cholera bodies are not at all prone to decomposition. It must, therefore, be some other chemical change, and I conceive that the following must be the explanation;—During the collapsed stage of the disease, the blood, from imperfect circulation, &c., is prevented from being properly oxygenised, and hence a venous blood is circulating in all the vessels of the body, accounting for the cold stage. After death, however, the blood in the superficial capillaries becomes acted upon by the oxygen of the external air, and the formation of carbonic acid, with the production of heat, ensues: this heat

* Vide op. cit.

† Vide p. 328.

* Dr. Garrod.

often for a time much exceeds in amount that which is abstracted by the cooling influence of the surrounding air: after death from most other causes, the blood, having become oxygenated during life, no further heat is evolved, and the ordinary cooling process ensues.

"I am, dear sir,

"Yours faithfully,

"A. B. GARROD.

"W. F. Barlow, Esq."

It is unnecessary to ask the attention of the reader to these observations. That the oxygen of the air does act upon the blood in the capillaries may be inferred from the fact of the livid colour of the body giving way occasionally to a red tint. The same thing has been noted in the asphyxiated, and perhaps in these also some warmth may be generated, after the breathing has ceased, by the aerial oxygen acting on the carbon of the venous blood, where-with the superficial vessels are charged heavily. If the view suggested by Dr. Garrod be a right one, the skin would probably be found in such instances as he refers to (were a thermometer carefully and properly employed) warmer than the parts beneath.*

It would be interesting to know whether the augmentation of heat be invariably accompanied by change of colour, and whether those bodies become most elevated in temperature which are most livid at the time of death. It would be of moment to know, also, the relative effects of exposing some parts to the air, and of keeping others, as far as possible, excluded from it. Changes of the blood in the capillary vessels *may* partly explain long *maintenance* of heat where the body is exposed to the air, and not in cholera only, but other cases. As the chemical combinations, on which the heat may be supposed dependent, are of quick or gradual completion, so will the warmth of surface hasten away or linger. We are supposing a case in which a high temperature, so far as mere *chemical* changes go, is produced *immediately* by the same causes in the living. Though oxygen be no more supplied from within by ever fresh and flowing currents of arterial blood, it may be obtained from without; but now it acts upon stirless,

not moving blood, producing changes which, quite compatible with the most absolute death, serve no perceivable purpose. As the clot of blood drawn from the vessels reddens when exposed to the atmosphere, so does the still blood in them change colour when the surface of the body lies uncovered after death, for that which intervenes between it and the air suffices not to hinder the influence of the latter. The question is, not as to the fact of influence, but the extent and results of it.

The cause of the coldness of the body in cholera during life-time, is more readily explicable than the rise of temperature after death. The *immediate* cause of animal heat, whatever be the indirect influence of the nervous system,—which is not the less remarkable and obvious because indirect,—is the formation of carbonic acid in the general capillaries, and certain other chemical changes. There is nothing to show that nervous influence of *any* kind can produce heat independently of changes of the blood; but, as the experiments of Sir Benjamin Brodie, the effects of shock, the results of many affections of the body in which the nervous system is more or less concerned, and the physiological relations of that system with the respiration and circulation abundantly prove, we have to deal with no matter of simple chemical changes, but of changes varying with, and varied by, the complex and manifold alterations of condition which life, as a whole, is prone to undergo. All this must be granted.

There is no doubt that many affections of the nervous system may produce coldness indirectly. They act upon the heart and the respiratory movements so as to diminish the sources of warmth; but not to these must we look for the true explanation of the frigidity of cholera, which is certainly more rationally explained by reference to that condition of the blood which forbids the due occurrence of those changes without which the body must decline in temperature. The chief reason of the coldness of cholera would seem to be the non-formation of carbonic acid in the general capillaries. If this be the case, should there not be a great deficiency in the amount exhaled during the cold stage of this disease?*

* It had occurred to Dr. Basham and myself some time ago, that a comparison of the heat of the external and deep-seated parts might tend to some important conclusions.

* I am indebted to Dr. Snow for an interesting case, in which he administered oxygen. In this instance the quantity of carbonic acid exhaled was proved to be very scanty.

I believe it would have been discovered, had sufficiently exact experiments been made, that the intensity of the disease would have been nearly proportioned to the degree of diminution of the proper quantity of exhaled carbonic acid; and that an increased amount of its exhalation would have been found to denote recovery, even as it is foreshadowed by the restored secretion of bile and urine. —I mean, of course, recovery from collapse; for I forget not the coma, overwhelmed by which so many have perished, after exciting false hopes.

There seems to me, as there has seemed to others, some analogy (though I would not push it violently) between an animal with the pneumogastric nerves divided, and a patient dying of cholera. M. Provençal and Sir Benjamin Brodie found that a section of the nerves in question reduced the exhalation of carbonic acid. There is certainly less of it exhaled in cholera, far less than there ought to be.

The respiration becomes embarrassed after division of the pneumogastric; so it does in cholera. And I believe not only that it is purely *voluntary* in the former case, or nearly so, but that very often, in extreme cases of the disease in question, it is performed almost entirely by the will. In the former instance, impressions on the periphery of this nerve are useless, because its trunk is disabled from conveying them; in the latter, I believe that the periphery cannot be properly stimulated, in consequence of the condition of the blood in the putmonary circulation; and that the *reflex* movement of respiration fails from this cause.* Further, less oxygen is inhaled, and animal heat fails in both cases.

So far we may indulge comparison; but manifold differences separate the two conditions. In the first, the blood is affected because of the impaired respiration; in the latter, the respiration suffers and labours because of the damaged blood. Then there are the diarrhoea, the vomiting, the cramps of cholera, distressing with all the sudden and violent operation of some deadly, agonising poison. There is that strange state of blood which explains everything but its own condition,—explains the coldness, the pulselessness, the

lividity, the death,—explains also the too common and palpable failure of remedies; for how can they act when they are not absorbed,—how influence when they do not circulate? The blood's altered consistency alone would seem to explain much. When Mr. Hunter gave the reasons of the blood being fluid, he so hinted at what would happen if loss of fluidity should occur, as to draw a picture which fails not to remind us of some main symptoms of cholera:—"Without being fluid, it could not be propelled through flexible tubes, and distributed to all parts of the body; it could not be divided into portions, as the vessels branch off; it could not pass through the smaller vessels, nor admit of the various separation of its parts which are to produce the increase and repairs of the whole body; neither could it be adapted for furnishing the various secretions; nor could it be brought back to the heart."

Asking pardon for this digression, I would revert to my subject. It must not be supposed that extraordinary states of temperature after death are *peculiar* to cholera. Dr. Davy has observed some facts from which it might be surmised that the thermometer would, in certain parts of the body, demonstrate in some cases much higher degrees of post-mortem temperature than we have now any idea of. His remarks were made at the hospitals in Valetta. A man died of supposed acute rheumatism: there were found large collections of matter in the neighbourhood of the shoulder and hip-joints. The body was examined three hours and a half after death, when the temperature of the room was 86°. It was warm externally, but the deep-seated parts gave to the hand a burning sensation. The thorax and abdomen had been exposed to the air about ten minutes, when "a thermometer placed under the left ventricle of the heart rose to 118°; and under the liver, in contact with the lobulus spigelii, to 112°." What was there in this case which could explain so remarkably high a temperature? In another instance, wherein a man died suddenly, and was examined five and a half hours after death, in a room of the temperature of 86°, the thermometer placed under the left ventricle of the heart rose to 108°; placed under the liver, to 107°. Whence this great heat? In both these cases it will be seen that

* It is needless to say that I adopt Dr. Marshall Hall's view of the office of the pneumogastric as an excitor of respiration.

the greatest warmth was declared by the thermometer being placed in contact with the heart; but Dr. Davy makes statements of other examinations in which there was a difference (in one case of 8°) in favour of the liver. As to the two cases which revealed so extreme a temperature, it must be well remarked that the early period at which they were examined after death would seem to set aside the notion that it was attributable to putrefaction, which, Dr. Davy says, "had not taken place in these bodies,"—"had hardly obscurely commenced." It is not known whether there was a rise of temperature, or only a maintenance of that which, before death, was very high. It would have been most interesting to have known the exact heat of the skin at the time of dissolution.*

It would seem that the condition of the circulation of a part at the time of death may have much to do with its heat afterwards. M. Bouchut, in a very recent work on the signs of death, speaks of the full or empty state of the stomach in reference to the process of cooling, and cites a case placed on record by M. Ollivier d'Angers, in which the abdomen of a man, who died during digestion, was found hotter than other portions of the body. But the observation, like so many others in reference to the heat of the dead, is too vague. What was the difference in favour of the abdomen, and what was the warmest portion of this region?†

Dr. Dowler has detailed, in his "Experimental Researches," some instances of a singularly high post-mortem temperature. The following will show the temperature observed in some cases, and the sex and age of the subjects of observation:—

Sex.	Age.	Temperature.
M.	27	100
—	30	102
F.	26	103½
M.	24	107
—	25	107 to 106
—	29	107½ to 103
—	24	108
—	22	106 to 106
—	26	109 to 102
—	50	109 to 102
—	25	111 to 102

* See Dr. Davy's remarks on these cases, in the first volume of his *Researches*.

† *Traité des Signes de la Mort*. Par E. Bouchut, Ouvrage couronné par l'Institut de France. Paris, 1849. See p. 191.

The temperature was taken soon after death; but the time varied, as the reader may see by turning to the account. Sometimes an hour, or two hours, or more, elapsed before the thermometer was used. The decline of heat was frequently noticed up to a certain point. In many of the instances muscular contractility was readily made manifest by mechanical irritation of the muscles. The comparative temperature of life and death does not appear to have been fully and accurately estimated; but the subject is alluded to, and it is stated that the body of a Philadelphian, "which before death had been remarkably cold, had a temperature after death as high as 109°." The thermometer was, in one case, applied to the chest; in another, to the axilla; but, for the most part, we are left to guess with what spot it was placed in contact. It would, however, seem that the skin was always tested, for nothing is said any where of the temperature of the internal parts. But of what diseases did the subjects of the observations die? The great majority appear to have perished of yellow fever.

The great heat observed in the dead by Dr. Dowler reminds one of the high temperature observed in birds in life-time. The following is Tiedemann's table representing the latter; and I introduce it that the reader may at once compare it with the imperfect one I have constructed with Dr. Dowler's examples:—

	Degrees.
Great titmouse	111-25
Swallow	111-25
Starling, different species	111-25 to 107
Anna, different species	111 to 106
Common hen	109-94 to 102-99
Falcon, different species	109-74 to 104-5
Pigeon	109-58 to 106-7
Raven	109-28 to 105-99
Pullen	107-49
Common cock	103-78 to 102-99
White game	102
Gull	100

A very high temperature has been demonstrated in the human body during life. Even the highest ascribed to birds by Tiedemann, or the highest shown to exist in man, when dead, by Dr. Dowler, has been exceeded by the living frame. Let me allude to a remarkable case or two. The temperature of 106° has been noticed in scarletina by Dr. Dunglison; that of 110-75°

in tetanus, by Dr. Prevost, of Geneva, who communicated the fact to Dr. W. F. Edwards.* M. Riouy is said to have remarked the temperature of 112° in typhoid fever; but, a still higher heat, if observed in the living body,† would, I think, be less calculated to surprise us than the amount of warmth which has been already shown to have happened in the dead. In life-time, the balance between the processes whereby the frame is cooled and heated may be altered in various ways. Much may happen to favour these chemical changes, whereupon animal heat more immediately depends, whilst, simultaneously, the secretion of the skin may be so checked, that the surface shall operate most imperfectly as a diminisher and regulator of the warmth of the body. All parts of the subject need well tracing out, though there be writings,—and I may refer to the Memoir of M. Roger on the Temperature of Infants and Young Children,—in which the pathology of animal heat has been handled with no little ability and care. The subject, in reference to the nervous system, is of singular interest. Facts have come to light in reference to injuries of the spinal cord which compel even the attention of the incurious. M. Chossat found by experiment that division of the superior portion of this organ was the cause of a remarkable elevation of heat. Sir Benjamin Brodie noticed the temperature of 111° , both during life and directly after death, in a case of forcible separation of the fifth and sixth cervical vertebrae, giving rise to paralysis;‡ Dr. Gull lately informed me of an example of unusually high temperature succeeding to, and, no doubt, caused by, injury of the spine. Different parts of the body should, of course, be carefully tested by the thermometer in cases of this kind. Only lately I saw a man with his lower extremities entirely, his upper partially, paralysed by a large effusion of blood external to the

dura mater of the cord: it was consequent upon fracture of two of the cervical vertebrae. At a time when the temperature of the inner surface of the lip was 97° , and that of the axilla was the same, the thermometer rose to 100° on my placing its bulb between two of the fingers or toes. Probably the state of the skin as to perspiration differing in the unparalysed and paralysed parts, may explain some cases of disparity of heat. As is known well enough, there is no *uniformity* as to the matter of temperature in cases of affection of the superior portion of the cord. Cruveilhier has given a most interesting example of spontaneous displacement of the atlas, which gave rise to compression of the cord and hemiplegia.* Coldness of the skin, and the feel of shivering, were prominent amongst the symptoms. Cruveilhier says that the *constancy* of the coldness afforded him always fresh admiration, and even goes so far as to declare that it was so manifest and remarkable during the course of the malady as to lend support to the opinions of those physiologists who consider the spinal marrow as the main source of animal heat.

Of animal heat we have yet much to learn. Viewing it, to speak generally, as proportionate, *ceteris paribus*, to the fulness, freedom, and swiftness of the circulation, and the corresponding energy of the respiratory process, we are but ill prepared to find the high, the extremely high amount of it which has been noted in the dead. In *what* cases the heat absolutely rises after death, and in *what* also it rises *most*, remains to be ascertained. How can we talk of what we have not examined? It has been shown, at any rate, that the rise in question is not peculiar to cholera: and it is certainly required that the more important facts which touch upon this subject should be more generally and completely canvassed, for they have most interesting relations to the signs of death, and may at some future time give birth to medico-legal inquiries, the difficulties whereof, increased alike by rumour and credulity, will alone be solved, if solved at all, by the indispensable, and but too frequently undervalued evidence of medical witnesses.

[To be continued.]

* See On the Influence of Physical Agents on Life. By W. F. Edwards, M.D., F.R.S. Translated by Dr. Hodgkin and Dr. Fisher. See p. 257.

† It has been stated by Dr. Granville that the heat of the uterus has, during labour, risen so high as 120° .

‡ "I have made experiments similar to those of M. Chossat, and have met with similar results. I have also seen several cases in which an accidental injury of the spinal cord has produced the same effect."—See Sir Benjamin Brodie's Observations on Injuries of the Spinal Cord, Medico-Chirurgical Transactions, vol. xii.

BRIEF NOTES
OF
THE EPIDEMIC CHOLERA IN
BALLINASLOE,

IN APRIL, MAY, JUNE, JULY, AUGUST,
1849.

By EDMUND SHARKEY, A.B., M.B., T.C.D.

As Ballinasloe has obtained a world-wide notoriety by the high coloured descriptions of "S. G. O." in the *Times* newspaper, I have thought that a plain unvarnished account of the epidemic which raged here might not prove unacceptable to you. The mortality, though undoubtedly great, was not excessive, the circumstances being considered. The number of cases and deaths which fell to my share were as follows:—

Cholera cases, 624, of which 223 were males.

Do. deaths, 334, of which 126 were males.

The total number of cases of dysentery was 493, of which 197 died.

The total number of cases of dysentery and cholera which occurred in subjects under the age of 15, was 510, of which 218 were deaths.

I must, however, observe, that from the great rapidity with which the disease spread, and the consequent confusion, the accuracy of the above numbers is not to be *absolutely* relied on. It is, however, as accurate as I could make it from the materials I possessed, and is not, in my opinion, very far from truth.

An epidemic dysentery and diarrhoea having prevailed almost universally for several previous months, and proved particularly fatal among emaciated adults and children in the workhouses, giving latterly an average of from 100 to 120 deaths per week; and the keen north and east winds, with frost and snow at intervals, having given place to soft weather, with wind from westerly points, the disease broke out in Boyd's brewery (auxiliary workhouse) on the 23d of April. It spread so rapidly that, on the 28th, when I took charge, there were 85 cases in that establishment, almost all in the third stage. The weather continued warm and sunny, with westerly wind, and the disease increased with fearful rapidity. It was like a spark fallen among tinder. The miserable

remnants of famine and dysentery were carried off in numbers at last too great for a prompt supply of coffins and graves. In one of the auxiliary houses assigned to old and infirm men, there was scarcely such an occurrence as a recovery, however slight might appear the commencement of the attack, and though the subject of it might appear to have some little stamina (for they were not *all* old, many of them were young men, whom the workhouse diet had somewhat restored from the state of starvation in which they had entered), their stamina were only apparent. And I may here remark, in passing, that famine appeared to do its work with more appalling effect among the men than among the women. Nor was I the only visitant of their cabins who observed that for *one* emaciated woman there were met *ten* famine-stricken men; and where the wife kept up an average "embonpoint," the man was frequently a walking spectre. This, I believe, arose partly from the fact of the men continuing to attempt earning a day's wages on little or no food, and partly, I feel convinced, from the poor creatures forbearing to satisfy the cravings of nature in their own persons, for the benefit of their wives and children,—exhibiting in many instances a degree of heroism which I did not expect to see in any race of people, and which I verily believe is, to the same extent, unparalleled in any other country under the sun. It is, indeed, a *sad*, if not a "proud" pre-eminence, attained, alas! by the long continued practice of privation, at the expense of pristine vigour and manhood. There was at last scarcely to be seen an able-bodied labourer; and when such were required for the railway works (undertaken earlier than was expedient, for the purpose of employing the poor people), they were found incapable in very many instances of giving a fair day's work, and were consequently rejected. Thus did their extreme poverty present a barrier to their escaping from it. How true—"Haud facile emergunt quorum virtutibus obstat res angustæ domi," not only in poetry, but in the homely concerns of the working man.

With the exception of the absence of a sense of blowing in the ears, observed in former epidemics to usher in the attack, the symptoms presented no important difference from those observed

by me in former epidemics in 1832, 1833, and 1837. I have been confirmed (if confirmation were necessary) in the opinion, that the first step in the chain which leads from health into this particular disease resides in the nervous system. I was myself on the verge of it in 1837, and again in the late epidemic; and I had no difficulty in recognizing the peculiar, indescribable sensations, referable to the spinal marrow, which in both cases were experienced. In neither instance was there either vomiting or diarrhoea, but a feeling of motion in the spine, as if something were traversing it in the direction of the epigastrium, which required a strong mental effort and perfect quietude of body in the horizontal posture to prevent vomiting. No such feeling was experienced by me in any indisposition at any time before or since; but it was felt more or less, at intervals, during the prevalence of the epidemic.

The treatment which I adopted in the last was in principle and in detail nearly identical with that used by me in 1837, and which I have published in the *London Medico-Chirurgical Review* of Jan. 1839, to which I beg to refer: it was briefly as follows:—

When the disease was ushered in by diarrhoea, I at once commenced the acetate of lead and opium, which, though often useful, was not found possessed of the almost specific virtues which have been attributed to it.

When the first stage of the disease came on I administered a tea-spoonful of the following tincture in warm punch: if given in the very onset (as in my own case), the effect was frequently to produce a glow of heat, followed by profuse perspiration, which continued for some hours, and left the patient free from complaint, but weak. It was as follows:—*R. Camphoræ, 3j.; Tinct. Opii, ʒss.; Tinct. Cardam. C. ʒiss.; Ol. Ment. Pip. gtt. xij. M. Sum. cochl. min. j. pro dosi, si opus repetend.* The usual appliances for restoring temperature, viz. dry heat and dry friction to the surface and extremities, were adopted, and the most vigilant precautions used against any effort on the patient's part to leave the horizontal position. Small doses of calomel (gr. ss. to gr. j.), with capaicum, were given at short intervals, with ammoniacal mixtures and punch (cold water being also freely allowed). Opium was sparingly

administered, except per anum, in which mode it never seemed to do any harm, and frequently much good, an enema of Plumbi Acet. ʒij.—3j.; Tinct. Op. ʒj. in ʒiv. (or less) of Mucilag. Amyli, sometimes effectually checking the diarrhoea.

The secondary fever was found, of course, most formidable, especially in children; and from its rapidity, when once set in, I was driven to the practice, as soon as reaction began, of uniformly applying, as a prophylactic, blisters to the calves of the legs, not waiting till the dangerous symptoms should manifest themselves.

From the class of persons I had to treat, I was seldom called on to abstract blood; indeed, it was not to be thought of in the great majority of cases, for the usual signs of arterial action or sanguineous determination were absent. The subjects of this fever were principally children; and none but an eye-witness could conceive the phase of withered childhood which the Irish famine of 1849 produced. To see the little sufferers lying in their beds, you never could say (except by the smaller proportions) that you were not gazing on a specimen of most emaciated old age. The wrinkled skin, sometimes overgrown with tufts of long lank down; the rigid muscles running as well-defined cords, and appearing to loop up the angles of the mouth, throwing the sunken cheek into a deeper hollow, and further degrading the expression below humanity into that of a superannuated baboon; while the little face had so fallen away, that the naturally well-proportioned forehead seemed by the contrast expanded into hydrocephalic dimensions; and both viewed in front giving the appearance of an inverted wedge. Long had I been in the habit of seeing various forms of Irish poverty, but never before starvation—never before an infant population labouring under a universal marasmus. It was, indeed, a sight to harrow the heart. I often wondered how steeled I had become, that I could look on such things without weeping.

The head was the organ generally attacked. Only one case, within my knowledge, terminated in peritonitis. The form of the head affection in this secondary fever is peculiar. There is frequently an absence of the ordinary accompaniments of this complication in

other fevers, viz. heat of head, strong arterial action, violent delirium, &c.; its only indices being a heavy expression of the eyes, somnolency, muttering, but with the power of recalling thoughts and speaking rationally while attention was kept up, the patient afterwards relapsing into the same dreamy state; and this condition, accompanied by an uncontrollable vomiting, every thing being rejected, and yellow or green matter, in apparently inexhaustible quantity, ejected from the stomach. This form I have chiefly observed in strong adult subjects.

As to the contagious, or rather infectious, nature of the complaint, my views continue unchanged since I wrote a short paper in the *Dublin Journal of Medical Science*, Sept. 1889, to which I beg to refer. Two of our nurses died, and two others had the disease each three times. These latter were two of the strongest women in the house, and were perfectly free from fear. Another had it twice. One of the medical gentlemen sent down to our assistance had likewise a slight attack. That infection is not the only source of the disease I need hardly say. There is a peculiar atmospheric influence at work, which, acting especially on the nervous centres, predisposes it to the disease; and upon anything occurring to upset the equilibrium of the system, hurries it on into it. A melancholy case in illustration of this occurred in this town. Dr. James Colahan, being convalescent from some febrile attack, during the prevalence of cholera here, wished to go for change of air two or three miles into the country; and while driving on his way thither, through the misconduct of a drunken coachman, the horses ran away. Dr. Colahan jumped out, and fell. The shock brought on instant vomiting, the malignant form of the disease set in, and he sank. He was a young man of high promise, and was universally liked and deeply lamented.

In making these few observations I feel that I have not said anything new; but as it is the third epidemic which I have had an opportunity of witnessing, and as the confirmation of past experience is not to be despised in medicine, I submit them to my brethren with a feeling akin to that of the tempest-tossed mariner, who, when the storm is past, looks back with gratitude upon the waste of waters where many a stout ship has

been engulfed, while his own frail bark has been graciously spared; indulging the humble hope that this brief summary may not be destitute of interest.

Bellinasloe, April 9, 1890.

CASE OF HOMICIDAL WOUND IN THE ABDOMEN.

By C. A. GORDON, Esq., M.D.
Surgeon, 57th Regiment.

CORPORAL JOHN GRACE, 57th Regiment, aged 22 years, of strong and healthy constitution, was at half-past 5 A.M., on 2d November, 1849, brought to hospital, suffering from inability to void his urine, and complaining of severe pain in the perineum. There was a small and apparently trifling wound, into which a probe was introduced to the depth of an inch, in a downward direction in the parietes of the abdomen, about midway between the pubis and umbilicus, and a little to the left of the mesial line; and, on being questioned, he stated that about eleven o'clock the previous night, while in a scuffle with a civilian, he received a severe kick in the perineum, and was at the same time conscious of sustaining some injury in the abdomen; but afterwards walked upwards of a quarter of an English mile to his barracks, where, having to undress in the dark, he did not observe the blood that issued from the wound, nor did he suffer in any way until about half-past four o'clock, when he awoke labouring under the symptoms for which he was sent to hospital.

On admission, two ineffectual attempts were made to introduce the catheter. Venesection was then employed; fomentations applied to the abdomen, and a draught, consisting of half a grain of tartar emetic, twenty-five minims of tincture of opium, and an ounce of camphor mixture, administered at intervals of half an hour. After the third dose urine flowed freely.

The report of the case in the medical register states that, at a quarter past four P.M., he has not voided urine, and now suffers a good deal of diffused pain around the wound. The surface is cool, and he is free from pyrexia.

Bowels have not been moved since he came to hospital.

At half-past nine p.m. of the 2d, the pain in the abdomen continuing, and the pulse being 120, and small, venesection was performed. He had half a grain of tartar emetic, and half a grain of opium, every two hours; and the bowels being confined, an enema of gruel was administered, but did not operate.

On the morning of the 3d, he was reported not to have slept during the previous night; has voided urine three times; bowels have not been moved; tenderness of abdomen less; surface moist; pulse still rapid and small; tongue coated with grey ash-coloured fur. An ounce of castor oil was now administered in enema, and fomentations ordered to be continued to the abdomen.

The noon report states, that within the last hour the pain in the abdomen has increased in severity. The countenance is assuming a more anxious expression. He now states, that early during the night he was seized with shivering, and that he again experienced a sensation of coldness immediately before the present accession of pain. The enema has not yet operated. Pulse small.

At eleven p.m.—He continues to suffer much from pain in the abdomen. The bowels have not been moved, further than that the enema has been voided. The expression of the countenance is very anxious, and the features are assuming a sunken appearance. He is reported to have been incoherent during the evening, and now talks somewhat wildly. Surface cool; tongue loaded as above described; pulse 140, and small.

The treatment at this stage consisted in the administration of a pill, containing one grain of calomel, a quarter of a grain of tartar emetic, and half a grain of opium, every second hour since noon, fomentations being kept constantly applied to the abdomen; and now, while these remedies were continued, at regular intervals, an ounce of castor oil, with two drops of croton oil, and twenty drops of tincture of opium, were administered.

At seven a.m. of the 4th, it is stated that within the last half hour a decided change to the worse has taken place; he is deadly pale. The pulse at the

wrists is almost imperceptible, and he is quite incoherent. The medicine operated very freely during the night, and he now voids fæces and urine in bed. While this report was being written he sank and died.

Post-mortem appearances thirty-two hours after death.—Abdomen: a wound of half an inch in length and a quarter of an inch in breadth was observed in the abdomen, situated three inches below the umbilicus, and half an inch to the left of the mesial line. This wound extended through the left rectus and deep abdominal fascia, in which it formed an aperture of an inch and a half in length, and half an inch in breadth. It was then traced through the large omentum, and entered one of the convolutions of the ileum, in which it formed a small aperture of about a quarter of an inch in extent, the edges of which were surrounded with inflammation, with some lymph adhering.

The peritoneum presented marks of most extensive inflammation along its entire extent, and a large quantity of purulent effusion existed throughout the whole of the abdominal cavity, and occupied spaces between the folds of the small intestines, the external surface of which was also extensively inflamed.

REMARKS.—This case is looked upon as being of considerable importance, showing, as it does, the very serious nature of an injury that, under certain circumstances, it is possible to inflict upon a person, without his almost being aware of it; for here we have a soldier receiving his death-wound by the hand of a murderer, and yet afterwards walking a considerable distance; going to bed as usual, and for a period of upwards of five hours not being at all aware of the nature of the wound; to which his attention was soon then accidentally drawn.

We also from it see, that in some cases of wounds of the abdomen, that excessive sinking and anxiety which are supposed invariably to attend such injuries, do not actually occur.

Dublin, June 1, 1850.

ROYAL COLLEGE OF SURGEONS.

GENTLEMEN admitted members on the 21st inst.:—W. T. S. J. Hardy—W. S. Brown—J. T. Hillier—A. Fraser—S. E. Grammer—H. W. Jones—A. E. Brett—K. P. Watt—R. Elaper—C.

MEDICAL GAZETTE.

FRIDAY, JULY 5, 1850.

WE are now entering on that period of the year when, in 1849, the cholera began to declare its intractable character and fatal progress. The time is yet early to institute a comparison; nevertheless, we are disposed to draw a favourable augury from the present state of the weekly returns of mortality. Experience has shown that there is a close relation between the fatality of diarrhoea and the epidemic outbreak of cholera,—the former proving more or less the precursor of the latter. The deaths from these two causes during the months of June 1849 and 1850, are given in the subjoined table:—

1849.			1850.		
Week ending	Diarr.	Chol.	Week ending	Diarr.	Chol.
June 2	16	9	June 1	15	0
" 9	20	22	" 8	19	2
" 16	36	42	" 15	17	1
" 23	17	49	" 22	18	0
" 30	30	124	" 29	33	2

For some weeks past the deaths from diarrhoea have been slowly on the increase; but this is not unusual at this season of the year. We do not find that the increase of the deaths from cholera in 1850 has hitherto kept pace with those from diarrhoea, as in June 1849; and, so far, there is reason to hope we may escape another outbreak during the ensuing summer.

A close examination of the Registrar-General's returns shews that, since the disappearance of the cholera in the latter part of last year, the health of the metropolis has undergone a considerable improvement. An able statistical paper on this subject has been recently published by Dr. John Webster;* and

from this we learn, that while some diseases have exhibited a diminished rate of mortality, others have shown an increased rate, and others again have maintained a uniform progress. The last quarter of 1849, and the first quarter of 1850, compared with the corresponding quarters of 1848-9, shewed a difference in the total deaths of 4064, or about 13·5 per cent. During the first three months of 1850, the deaths were fewer by 2219 than during the same period of 1849; i. e. even before the cholera had manifested itself in the form of a malignant epidemic.

The principal diseases which have undergone a diminished rate of mortality are scarlatina, typhus, small-pox, cholera, influenza, and hooping-cough. Of the cholera, Dr. Webster remarks:—

"But during the first quarter of the current year the contrast has been marked, and highly satisfactory; not more than eight persons having died of cholera since the 1st of last January, instead of 516 during the first three months of 1849. The fact now stated is highly important, and may, I hope, be taken as an indication that cholera will not prove so epidemic and pestilential during next summer and autumn as it was last year."

The mortality from a few diseases has increased. This has been especially observed with respect to measles, and in a less degree with diarrhoea and dysentery.

The great and sudden diminution in the rate of mortality, which took place in the last quarter of 1849, is worthy of remark. Thus, if we compare July, August, and September, with October, November, and December, we find according to Dr. Webster that—

"In the former period the total deaths from all diseases were 27,109, but in the latter they decreased to 12,877, being a diminution of more than half, or 54 per cent. Cholera and diarrhoea continued to prevail throughout October and November of last year, though in a rapidly decreasing ratio, so that 976 individuals

* Dr. Webster's Remarks on the Health of London during the Six Months terminating March 30th, 1850.

died of these diseases during the entire quarter; nevertheless, this period, and the first three months of the current year, have proved more healthy than the corresponding period for several previous years."

What are the influences which have contributed to produce this favourable result even up to the present date? Dr. Webster refers them to—

"1. The improved physical condition of the population. 2. The abundance of good and cheap provisions. 3. The less variable, and hence more salubrious, condition of the atmosphere. 4. The increased attention recently given to sanitary measures. 5. The greater care paid by individuals to their own health, in consequence of the alarm experienced during the previous pestilence. And, 6. The removal, by the recent epidemic, of a large number of the debilitated, dissipated, and diseased portion of the population, so that there remained less *pabulum morbi* as compared with former years; while the survivors were also better able to resist the inroads of disease."

We must demur to 3, for since the commencement of March until the present time we have had great and sudden changes in the temperature of the atmosphere:—a difference of from 15° to 20° has been observed in as many hours. As to 4, Dr. Webster admits that more has been said than done:—

"Although much has been recently said respecting sanitary measures, and a few judicious improvements have been commenced, very little advance in the right direction has as yet been made. As I have previously adverted to this subject, and shewn its importance, it is unnecessary for me to repeat my arguments; but I would now again assert as strenuously as formerly, that all nuisances, whether public or private, must be abated. Vested rights, and individual interests, must yield to the health of communities. On this point, *salus reipublicæ est suprema lex*; and, as on such a question there can be no mistake, there ought assuredly to be no compromise."

It has been long an admitted fact, that the deaths in the year following an

epidemic, undergo a great diminution, owing to the removal of the infirm and unhealthy. The cholera epidemic of 1849 is, in this respect, like others recorded by medical writers.

"Similar results followed the disappearance of influenza, which prevailed so severely during the winter of 1847-48. In the quarter ending June 30, 1848, the number of deaths was 3,510 fewer than in the preceding three months. Again, after the cessation of scarlatina, which was unusually prevalent and fatal during the winter of 1848-49, the aggregate mortality decreased; the deaths from all causes registered in the quarter ending June 30, 1849, being 2,422 fewer than those in the three months ending 31st March. Hence, it may be generally predicted, with some confidence, that whenever any epidemic malady prevails, and removes numbers of the population, the subsequent season or year will probably be more healthy, and fewer deaths will occur, than under other circumstances. This arises much from the removal by the previous epidemic, of the subjects most liable to disease; but there may be also other evident causes in operation, although it is often impossible to point out, and still less to define precisely, the association of every influence, however consistent the results may seem with general experience."

For the first time for many years the deaths in the summer months of 1849 exceeded the births; nevertheless, according to the statistical paper before us, the hiatus made by the cholera has been already more than filled up, and we are now increasing in numbers as if no such malignant disorder had been so recently prevalent among us.

"The remarkable excess of births over deaths, which has taken place in London during the last two quarters, is worthy of notice; particularly as history informs us, that increased fecundity is the ordinary sequence of a pestilence. In the first three months of the above period, 4819 more persons were born than died in London; and during the second quarter, the excess was 5,428, thus making a total of 10,237 more births than deaths in six months! And if to that number we add the 4,067

fewer deaths registered during that period, as compared with the corresponding six months of 1848-49, the vacuum made by the cholera will be found already fully filled up; the excess of deaths during July, August, and September 1849, as compared with the same months of 1848, amounting to 13,606, having been replaced by 14,304 persons—10,237 more births, and 4,067 fewer deaths. In fact, excluding immigrants from the provinces, who may be set off against emigrants to the colonies or elsewhere, there are 698 more human beings now resident in London than after the devastating disease of last autumn. This fact is highly consolatory, and tends to shew the compensating power of the laws of nature, at the same time that it indicates the power and care of our beneficent Creator."

Dr. Webster has rendered good service to medical statistics by the publication of this interesting document. We trust that he will be induced to continue his plan of giving to the profession a biennial summary of the health of the metropolis; and that other medical men of equal experience will be induced to follow his example. The method which he has pursued is both scientific and practical, and any one desirous of a model of statistical research, will find it in Dr. Webster's pamphlet.

While on Sanitary Statistics we must not overlook the fact, that in at least one provincial town, a monthly report of the mortality and public health is regularly published. The town which has set this creditable example is Oxford; and the public and profession are indebted to Dr. Greenhill and Mr. Allen for this periodical record of the progress of disease and mortality. The plan pursued resembles that of the Registrar-General, with the useful difference, that instead of three there are six subdivisions of deaths according to ages. Some explanatory notes are also introduced with respect to individual cases.

The publication of such Reports in all the great towns and cities of the empire would be attended with the best results to medical science; and we do not think that members of the medical profession could be more usefully employed than in collecting and publishing facts of this kind. The funds required for this purpose should be provided by local subscription among the inhabitants; or, where this resource fails, the aid of Government should be extended to the support of a plan without which sanitary legislation can make but little progress.

We have great pleasure in announcing to our readers that a Society for the investigation of Epidemic diseases, to be called the *EPIDEMIOLOGICAL SOCIETY*, is now in course of formation. It has been long felt that the subject of the origin and diffusion of Epidemic disease had not received that practical investigation at the hands of medical men which a question so materially affecting the health of nations demanded. The recent visitation of cholera has given a new stimulus to this inquiry; and we think that all practitioners who have had experience in this and other epidemic diseases should at once associate themselves with the new Society.

"Nur wenig Honig giebt die einzelne Biene, aber zusammengetragen schmeichelt er dem Gaumen von Tausenden."

The Society is still in want of support: it is slowly but steadily adding to its numbers. The list of vice-presidents has been filled up; and several gentlemen, likely to promote very effectually the important objects of the Society, have accepted the invitation of the President, Dr. Babington, to become members of the Council. It is expected that in a very short time the Society will be in a position to commence active operations.

Reviews.

On the Operation for Strangulated Hernia. By HENRY HANCOCK, F.R.C.S.E., Surgeon, and Lecturer on Surgery, Charing Cross Hospital, &c. &c. 8vo. pp. 94. London: Churchill. 1880.

THE object of Mr. Hancock's work is the investigation of the merits of the operation for relieving strangulated hernia by the division of the stricture without opening the sac. To this end the author first examines the validity of the objections to the usual operation; he next inquires into the statistics adduced in favour of not opening the sac; and in the last place submits some very judicious observations on the general treatment of these cases.

In reference to the first point, we may observe that an affirmative answer to the following questions will put our readers in possession of Mr. Hancock's opinion on the subject. For the facts and reasonings with which he ably supports this opinion we must refer our readers to the work itself: to quote portions thereof, would be, by mutilation, to impair the practical value of the author's remarks.

"In the first place, then, are these objections valid or tenable? Have not the authors in question, in their anxiety to support their own peculiar views, overlooked the real cause of failure, and gasped at the shadow whilst they neglected the substance? Have they not, by heedlessly exaggerating the danger of the usual method, and erroneously attributing to the operation the failures which, in reality, resulted from causes entirely independent of such operation, created unnecessary alarm and anxiety as to its probable termination; and thus tended to perpetuate that system of delay, which we have all so much reason to deplore, as leading to 'unsuccessful efforts,' pressure, and bruising the intestines, and postponing the operation until it is merely regarded as a forlorn hope?

Mr. Hancock's examination of the statistics which have lately been adduced in favour of dividing the stricture without opening the sac, shows them to be on the contrary, conclusive against this mode of operating.

In discussing the subject of treatment, the author expresses a strong opinion, which he supports by reference

to the best authorities, that the too long employment of the taxis has been a copious source of fatal consequences. Mr. Hancock demonstrates also the injurious effects of the employment of purgatives, and supports his statements by quoting cases recorded by Mr. Cock, in the Guy's Hospital Reports, in which the advantage of a free exhibition of opium after the operation, and the abstinence from purgatives, are well shown. Under the head of treatment, Mr. Hancock speaks of the beneficial effects of chloroform, which he says he has found to supersede the use of the warm bath, bleeding, opiates, &c. &c., in the speedy reduction of the hernia in many cases, while the patient is spared the dangers resulting from delay and unnecessary handling, where the operation after all has been found necessary, and in the performance of which the author has found chloroform a very valuable aid.

Mr. Hancock appends a few cases and tables which illustrate and confirm his views. We strongly commend the "Deductions," with which the author closes his work, as indeed the whole work itself, to the earnest study of all surgeons.

A Manual of Elementary Chemistry, Theoretical and Practical. By GEORGE FOWNES, F.R.S., &c. 3d Edition. 8vo. pp. 605. London: Churchill. 1880.

WE take blame to ourselves that we have not sooner announced the appearance of a third edition of this useful manual. This edition has been brought out under the superintendence of Dr. Bence Jones; and it is proper to observe that the *Animal Chemistry*, left unaltered by Mr. Fownes, has received from the hands of the editor those corrections and alterations which the advances made in this branch of science had rendered necessary. The work has deservedly found a large circulation among students; and this edition will no doubt have as favourable a reception as those which have preceded it. Its convenient size and portability are strong recommendations, especially when other well-known treatises on chemistry are acquiring a magnitude and reaching a price which cannot fail to frighten a beginner.

Proceedings of Societies.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, June 25, 1850.

DR. ADDISON, PRESIDENT.

Two Cases of Idiots with Absence of the Thyroid Body and Symmetrical Swellings of Fat Tissue at the sides of the Neck. By T. B. CURLING, F.R.S., Surgeon of the London Hospital.

Case I.—In July 1849, Dr. Little invited the author to see a case of *cretinism* at the Idiot Asylum, Highgate, and to examine some swellings at the sides of the neck, the nature of which was doubtful, but which had been suspected to be enlargement of either of the lobes of the thyroid body, or of the lymphatic glands. The inmate was a female child, 10 years of age. She measured two feet six inches in height. The head was heavy-looking, the forehead flat, and the fontanelles inclosed. The countenance had a marked and very unpleasant idiotic expression. The mouth was large, and the tongue thick and protuberant. At the outer sides of the neck, external to the sterno-cleido muscles, there were two tolerably symmetrical swellings, which had a soft, doughy, inelastic feel. Similar swellings, but smaller and less defined, were observed in front of the axillæ. No enlargement existed in front of the neck. The girl had no power of speech, but was able to recognise her parents. She had a severe attack of erysipelas, which was followed by a considerable abscess in the thigh. A second attack, accompanied with glossitis and stomatitis, caused her death fifteen months after her admission into the Asylum. On examination of the body by Mr. Callaway, the swellings in the neck were found to be composed of fat, and to occupy the posterior triangle of either side of the neck, dipping downwards behind the clavicles and filling the axillæ. They were not enveloped in capsule, but consisted of fat of a loose lobular structure.

Case II.—In November 1849, a female infant, *et.* six months, was sent to Mr. Curling for examination, on account of some anomalous swellings in the neck. She had a marked idiotic expression—a large face, with a small head and very receding forehead. The tongue was large and protuberant. On the sides of the neck, lying obliquely across, there were two soft symmetrical swellings, of an oval shape. The

author was struck with the strong resemblance which this case presented, both in respect to the tumors and the general aspect, to the idiot at Highgate. The infant died convulsed December 7th. On dissection, nothing abnormal was observed in the brain, except a remarkably small development of the anterior lobes of the cerebrum. A very careful examination was made of the neck, but no thyroid, nor trace of this gland, could be discovered. The swellings in the neck were found to consist of fat tissue, without any investing envelope, and loosely connected to the surrounding parts.

The author remarks that he is not acquainted with any case on record in which a deficiency of the thyroid gland has been observed in the human body. He considers it highly probable that the abnormal deposits of fat in the neck were dependent on the absence of those changes which result from the actions of the thyroid, or on some imperfection in the assimilating processes consequent on the want of this gland. In conclusion, the author states that in the foregoing cases we have examples of a defective brain, or of *cretinism*, combined with an entire absence of the thyroid, which may be regarded as tending to confirm the more modern opinion, that the coincidence of these two affections is accidental, or that they have no direct relation.

A Case of Hemiplegia, in which Loss of Speech was a prominent symptom. By ROBERT DUNN, M.R.C.S.

The subject of the communication died in a state of coma on the 18th of April last, in the 66th year of her age. It was her third apoplectic seizure. She was of a mixed temperament, the sanguineous and nervous, and of active habits. Her first attack occurred on the 6th of October, 1844, at 4 o'clock in the morning, up to which time she had enjoyed good health. The author, on being called, found her in a state of coma, with stertorous breathing, head hot, face flushed and turgid, mouth drawn down to one side, pulse full and labouring. She was bled and purged freely. In a few hours she became conscious, but was found to be hemiplegic on the right side. Her recovery was quick; she regained the free and full use of the arm and leg, and in a few months her general health appeared to be completely re-established. One peculiarity remained, which arrested the attention of the author, and led him to suspect the existence of some structural lesion of the encephalon,—the habit of using one word for another, and of not applying the proper and appropriate names to the things signified.

Her second attack took place on the 17th

May, 1847. She was found by her daughter on the morning of that day, lying in a state of insensibility on her back, upon the floor of her bed-room. The author was struck with the contrast between the symptoms of this and those of the former seizure. There was the same deep coma, but no stertor; the face was pale, and bedewed with a cold clammy moisture; the extremities were cold, and the pulse was feeble, weak, and fluttering. An opposite treatment was indicated and followed. Ammonia and Hoffman's anodyne were given freely. She was again found to be hemiplegic on the right side. Active reflex movements were excited by tickling the foot, but none could be induced in the upper extremity. She slowly but gradually improved, and ultimately got remarkably well. But she was paralysed and speechless for the remainder of her life. All the special senses were intact; the motions of the tongue were free, and there was no difficulty in deglutition. She was perfectly sensible, and took an interest in passing events. But she could not say *yes* or *no*, and never got beyond the utterance of the monosyllable *dat! dat!* The conscious failure of the effort found expression in a hopeless shake of the head, and often in a gush of tears.

Her last and fatal attack took place on the 14th April, when in the act of being undressed for bed. She was found by the author in a state of extreme collapse and comatose, with loud stertor, and with the left side as helpless as the right, completely paralysed. The teeth were so firmly fixed that nothing could be got into the mouth, and she died on the fourth day of the attack.

Post-mortem examination.—At the post-mortem inspection, the upper two-thirds of the anterior lobe of the left hemisphere was found in a state of disorganization—a pulpy mass; the middle and posterior lobes were healthy. The corpus callosum was destroyed, except at its anterior and inferior reflexion, and so also was the upper half of the corpus striatum on the left side. The optic thalamus was likewise shrunken to less than half its natural size, its upper surface being greatly wasted. The anterior commissure and fornix were gone, but the corpora geniculata were sound.

On the right side the hemisphere was healthy, but when its lateral ventricle was laid open a small and recent apoplectic clot was seen upon the upper and anterior surface of the corpus striatum; the whole of the upper portion of the corpus was in a state of ramollissement, and indications of white softening were seen also on the outer surface of the thalamus. The cerebellum and basis of the brain were healthy.

The author considers the case to present some points of interest in reference to the localization of the faculty of speech. He observes, a great mass of evidence has been collected in support of the opinion of Gall, that the seat of the faculty of speech is in the anterior lobes of the brain. He refers to the memoirs of M. Belhomme and M. Bouillan, read before the "Académie Nationale de Médecine de Paris" in 1848, in support of Gall's opinion, and then cites two opposing cases from Andral's "Clinique Médicale, Maladies de l'Encephale," in which the disease was in the corpus striatum. In considering this subject, he says, it is never to be forgotten that perfect speech—that is, the power of giving utterance to our thoughts in suitable and appropriate language—depends upon the due relation between the centres of volition and of intellectual action; and that, thus considered, the apparently conflicting evidence which has been adduced as to the seat of the faculty of speech, admits, in his opinion, of a satisfactory explication. The thought is framed and moulded for expression in the centre of intellectual action, but the due agency of volitional power, and consequently the integrity of its seat of action, is needed to give it utterance. And while it is universally admitted that the cerebra are the great centres of intellectual action, he believes with Gall, and of which the present case affords corroborative proof, that the anterior lobes are the seat of the faculty of speech; and he asks, Is it not equally true that the corpora striata are the great centres of volition, and therefore does it not necessarily follow that loss of speech may alike result from diseases of the anterior lobes, or of such portions of the corpora striata as are in direct relation with them? He concludes by saying, in the case narrated it is manifestly obvious that with the disorganization of the left anterior lobe its functional power was entirely abolished, and that although the right hemisphere was healthy, and there is every reason to believe, from the history of the case, that it maintained and exercised its function as a centre of intellectual action, still the volitional power was wanting to give utterance to the passing thought, for the corpus striatum was not in its integrity.

On a Case of so-called Chylous Urine.

By HENRY BENGE JONES, M.D. F.R.S.,
Physician to St. George's Hospital.

G—, harness-maker, *æt.* 32, born in Trinidad, came to Scotland when eight years old, and had been in London for twelve years. In the winter of 1848-9 he observed that his urine was at times thick and white. This becoming more continuous, with increasing weakness, he

up work on the 26th of May, 1849. Dr. Prent, among other things, gave him cod-liver oil, and he was sent to Margate for hot sea-baths. On the 19th of October he came to me, complaining of severe pain in the loins, and he passed milky water in my room. The minute microscopical details of the appearance of this urine, which contained fat, albumen, fibrin, blood-globules, and alkaline salts of the blood, are given at length. The urine, each time it was passed, was brought daily to me. The chemical details, with an analysis of the blood, will be published in the next volume of the *Philosophical Transactions*.

Previous to trying any medicines, careful observations were made on the effect of different diets on the appearance of the fatty matter in the urine.

The first series of observations was 71, from Nov. 6 to Nov. 16, when diet was chiefly animal food.

The second series of observations was 91, from Nov. 16 to Nov. 30, when it was chiefly vegetable food.

The third series of observations was 17, from Nov. 30 to Dec. 2, when it was chiefly animal food.

Hence total—

	Very chylous.	Chylous.	Slightly chylous.	Not chylous.
Animal food, 88 observations	3	37	40	8
Vegetable food, 91 ditto	2	20	49	29

Whether the diet was animal or vegetable, the urine was most fatty after dinner; and least fatty, or not at all so, before breakfast. It was more frequently chylous after animal than after vegetable food; and it was oftener free from chyle before break-

fast, when the diet was vegetable, than when it consisted more of animal food.

The effect of pressure by means of a belt was then tried the diet being chiefly vegetable.

	Very chylous.	Chylous.	Slightly chylous.	Not chylous.
4th series, 27 observations, from } Dec. 7th to 11th	2	7	6	12
5th series, 42 observations, from } Dec. 12th to 18th	5	11	12	14
6th series, 53 observations, from } Dec. 18th to 26th	1	16	17	19

From this it appears that, during the days when the belt was tight, the urine was less chylous than on the days when the belt was loose. The difference was not very

great; but the patient stated that the pressure relieved the pain in the back very much. Matico was then tried:—

	Very chylous.	Chylous.	Slightly chylous.	Not chylous.
7th series, 211 observations, from } Dec. 26th to Jan. 26th	7	27	69	108
In much stronger doses— 8th series, 89 observations, from } Jan. 29th to Feb. 11th	1	10	28	60

By comparing these results with the observations when no medicine was taken, the improvement is very evident.

Gallic acid was then tried,—a drachm in the course of the day.

9th series, from Feb. 13, after breakfast, to 16th, 26 observations: after the 14th no fatty matter was observed in the urine.

From Feb. 16th to 20th, 19 observations: no fat or albumen present.

From Feb. 20th to March 4th, 75 observations: no chylous appearance.

From March 4th to 24th (he was at

work), 93 observations: no chylous appearance.

From March 24th to April 6th, 72 observations: no chylous appearance; the gallic acid was stopped.

From April 6th to June 14th, 350 observations: the urine on each occasion was free from chyle or albumen.

The results from the 6th of November to the 14th of June are best shown by supposing 1000 observations in each series had been made:—

	Urine chylous in different degrees.	Free from chyle.
1st. On animal food	968 times.	32 times.
2d. On vegetable food	910	90
3d. With pressure, belt loose	667	238
4th. With pressure, belt tight	638	362
5th. On matico	474	526
6th. On gallic acid	17	983
7th. After gallic acid.	0	1000

The gallic acid was taken for 53 days. On the third day the albumen and fat disappeared from the urine; and for 69 days after the medicine was left off the disease did not reappear, and the patient has resumed his work for 102 days.

A second case is related in which the urine was once observed to be chylous in St. George's Hospital in 1840. Three months afterwards this patient died at Plymouth, and, on post-mortem examination, no disease of the kidney was perceived.

Observations on prolonged Expiratory Murmur as a Sign of Incipient Phthisis. By THEOPHILUS THOMESON, M.D. F.R.S., Physician to the Hospital for Consumption and Diseases of the Chest..

The author, after referring to the opinions of various auscultators, particularly Laennec, Jackson, Fournet, and Cowan, proceeds to state that, in the ordinary healthy condition, the expiratory murmur is very slight, and very little more distinct in one part of the chest than in another; and that in some individuals it is naturally inaudible, excepting perhaps during hurried breathing. In the natural condition he considers the duration of the expiratory sound equal to about a fourth of that of the inspiratory; the inspiratory and expiratory movements being about equal in duration, and three-fourths of the latter being performed in silence. In determining how much of altered rhythm may depend on extension of the expiratory murmur, and how much on diminution of the inspiratory, the author adopts Dr. Sibson's plan of counting the number of strokes which can be given in beating time with the finger during the presence of each murmur. The natural elasticity of the lungs is essential to soft and uniform expiration. Wherever there is considerable consolidation from tubercular or pneumonic deposit, bronchial expiration may be induced; and when the walls of the pulmonary cells are only slightly thickened, and the neighbouring structure is becoming occupied with the glairy, greyish deposit characteristic of early phthisis, the diminished contractility of the cells, interrupted passage of air, and increased power of conducting sound, may be sufficient to render the expiratory murmur more durable, coarse, and audible. A diversity of opinion, however, exists regarding the value of this change of murmur as a sign of incipient phthisis; and the author, with a view to determine its significance, gives in this paper the results of an analysis of 308 cases selected from those out-patients of the Hospital for Consumption in whom he noted prolonged expiratory

murmur as the most prominent symptom. In 20 there was either bronchitis, or emphysema. In the remaining 288 the symptom under consideration was observed almost invariably near the summit of the lungs. In 108 on the right, in 56 on the left, in about 124 on both sides; in this respect contrasting with wavy inspiration, the frequency of which the writer has found to preponderate greatly on the left side (Monthly Journal of Medicine, June 1846). With a few exceptions, referable chiefly to bronchitis or congestion, he has not recorded any instances in which prolonged expiratory murmur, when once established, ever ceased to be obvious, unless superseded by signs of more advanced disease. After describing the usual course of the symptoms, the author mentions that the most frequent special accompaniment was hæmoptysis, which occurred in 91 cases out of the 288; that is, in more than 31 per cent. Assuming that, of the hospital patients taken generally, about 43 per cent. have hæmoptysis before softening takes place, he calls attention to the fact that, whilst on the one hand the coexistence of hæmoptysis in patients with prolonged expiration is sufficiently frequent to strengthen the supposition that there is a connection between this altered murmur and phthisis,—on the other hand the frequency of hæmoptysis in these patients is so much below its average in consumptive cases generally, as to point towards two important conclusions, namely—

1st. That prolonged expiratory murmur takes precedence of other signs.

2dly. That hæmoptysis more frequently follows than precedes the deposition of tubercle.

The writer proceeds to prove, from the facts recorded, the fallacy of the opinion that prolonged expiratory murmur may exist on the right side irrespective of disease; but as this delicate indication may, in a majority of instances, be first detected in that situation, he recommends the careful examination of the upper part of the chest, especially on the right side, with reference to this sound, whenever hereditary phthisis is probable,—a measure which has sometimes led him to detect disease not previously suspected. He concludes with the expression of his opinion, that in the absence of certain sources of fallacy, such as bronchitis and emphysema, this disturbed rhythm of the murmur is the most early and significant sign of phthisis; and that, especially under favourable circumstances, and with appropriate treatment from the first detection of this symptom, the duration of the disease will be found considerably to exceed the period commonly assumed.

Account of a Case in which an Abscess formed in the Vesiculæ Seminales, and proved fatal by Perforation of the Bladder and Extravasation of Pus into the Abdominal Cavity. By MITCHELL HENRY, Assistant Surgeon to, and Lecturer on Morbid Anatomy at, the Middlesex Hospital.

The patient in question, a sailor, 20 years old, was under the care of Dr. Crawford in the Middlesex Hospital. He was admitted on the 19th December, 1849, with symptoms simulating those of hip-joint disease, and extreme pain in the loin and of the left side. His urine was very thick and scanty, and caused burning pain in its passage along the urethra. There was much febrile disturbance, with a quick small pulse, constipated bowels, and a loaded tongue. Leeches to the groin, with calomel, opium, and colchicum internally, alleviated his symptoms for the time; but they afterwards returned with increased severity.

The day after his admission the urine was observed to be loaded with pus, which the patient stated he had noticed at intervals for three weeks before. He denied that he had ever suffered from gonorrhœa, or any form of syphilis, and said he had never experienced difficulty in passing his urine. On the 22nd his febrile symptoms ran so high, that ʒviij . of buffed and cupped blood were drawn from the arm, with some relief. In the evening of the same day blood passed with the urine in considerable quantities, and continued to do so until the 26th, in spite of gallic acid and other medicines which were given to check it. On the 29th violent purging came on, and continued until the next day: it was preceded by pain in the abdomen, but there was no rigor. Medicines and leeches relieved the pain in some measure, and stopped the purging; but on the 3d of January the abdominal tenderness returned, and about 12 o'clock he died in much agony, retaining consciousness to the last.

Post-mortem examination by Mr. Henry, 12 hours after death.—The body was blanched and emaciated, the abdomen tympanitic. There was nothing abnormal discerned in the cavities of the head or thorax.

On cutting open the abdomen it was found to be filled with a great quantity of cream-coloured fluid pus, evidently from an abscess, and not the produce of peritonitis. The peritoneum covering the liver and intestines was inflamed, but not in an extreme degree. The abdominal organs, including the kidneys, were healthy and natural in appearance, except that they were pale. The bladder, which contained some purulent fluid, and was perforated

by an ulcer as large as a sixpence, was removed, together with the penis, testicles, ureters, and kidneys, for further examination.

The source of the pus was so far extremely obscure: the spine, pelvis, hip-joint, particularly the left one, where there had been so much pain, were carefully examined, without throwing any light upon its origin; and it was not for some time that the case was cleared up at all. Finally, however, the remains of a large abscess was discovered in the situation of the left vesiculæ seminalis, which was nearly destroyed by suppuration. The abscess had perforated the bladder by a small rounded hole, and discharged its contents into that viscus; and as at the same time the peritoneum reflected from the side of the bladder had been perforated too, the pus had found its way into the abdominal cavity, and set up the fatal peritonitis. What had excited the inflammation in the vesicula which had preceded the formation of the abscess, is not clear, but the perforation of the bladder must have taken place long anterior to death. The time when the peritoneum became involved cannot however have been far distant from the attack of hæmorrhage by the urethra, which was shortly followed by the abdominal inflammation.

The interest of the case lies principally in this—that the vesiculæ seminales, though not exempt from disease, are very rarely affected with active inflammation, giving rise to symptoms recognizable during life: but there is no reason why the more chronic inflammation, described by Lallemand as extending to them from the ejaculating ducts in severe forms of spermatorrhœa, may not occasionally become more acute, and result in abscess. Such may be the explanation of obscure cases of inter-pelvic abscess, the seat of which is uncertain. In like manner, abscess of the vesicula seminalis may be confounded with abscess of the prostate gland: and lastly, the excessive pain at the neck of the bladder, which now and then follows gonorrhœa, accompanied with shivering and discharge of pus with the urine, may result from an abscess of the vesicula seminalis, which is not of itself, of course, a mortal disease, unless it chance to discharge its contents into the abdominal cavity.

To the case is appended one by Mr. Cock, of Guy's Hospital, where abscess of the vesicula seminalis was diagnosed during life, and punctured through the rectum, with immediate relief. As the patient fortunately got well, the accuracy of the diagnosis could not be tested; but the symptoms were very like those detailed in the case above, especially of the existence of pain in and around the hip-joint.

After the papers had been read, as it still wanted a few minutes to 10 o'clock, the President invited Mr. Solly to favour the Society with any observations he might wish to make on the interesting and practically important case which he had communicated.

Mr. SOLLY observed, his object in bringing the case before the Society was rather to elicit the remarks and opinions of others on the treatment of such cases, than to offer any observations of his own. It was certainly a deeply interesting case,—one involving grave responsibility, which was keenly felt by the surgeon; and therefore it was that free discussion, and the open avowal of the opinions of men of experience, was what he courted and desired. After a most careful review and consideration of the whole case, his own mind rested on the conviction, that the treatment could not have been varied with any chance of different result.

Mr. Solly much regretted there could be no discussion upon Mr. Dunn's valuable paper. From what he could gather from the reading of it, in the first seizure, it appeared the *structure* of the brain was implicated, and the faculty of speech affected, and that this lesion laid the foundation for the subsequent serious affection and loss of speech.

Mr. DUNN said, the peculiarity to which Mr. Solly had alluded, of using one word for another, and of not applying appropriate and proper names to the things signified, was noticed from the time of the first attack. He considered the case to have an important bearing upon the *localisation of the faculty of speech*. One main object of his paper was to show how opposing evidence on this point admitted of a satisfactory explication. The case itself afforded corroborative proof of the opinion of Gale, that the faculty of speech was located in the anterior lobes of the brain. But as the power of perfect speech depended upon the *due relation between the centres of intellectual action and volition*, he maintained that loss of speech would alike result whether the disease was situated in the anterior lobes or in such parts of the corpora striata as are in direct relation with them.

The thought was framed and moulded for expression in the seat of intellectual action,—the anterior lobes; but the *due agency of volitional power*, and the integrity of the corpora striata, as its seat of action, were needed to give it utterance.

The President then congratulated the fellows on the prosperous condition of the Society, &c. &c. and the meeting broke up.

ACADEMY OF MEDICINE, PARIS.

June 18, 1880.

Disinfecting Liquids.

M. HUGUIER read an official report on a disinfecting liquid prepared by MM. Raphanel and Ledoyen, and which had been submitted for the report of the Academy by the Minister of Agriculture and Commerce.

The reporters stated that this liquid, which consisted of a solution of nitrate of lead, appeared to possess disinfecting properties in a high degree; that it is more especially useful where sulphuretted hydrogen is evolved by decomposition; and that it is also effectual for the prevention of putrefaction.

In the discussion which followed on this report, it seemed to be the opinion of the Academy that the report was founded on insufficient data; it was therefore returned to the commission for a more extended series of comparative experiments. MM. Robert and Caventou were added to this commission.

Synovial Cysts of the Wrist.

M. GOSSELIN read a memoir on this subject, of which the following is a summary:—The author made two divisions of those growths—the hydropic, and ganglionic cysts. The first are most frequently found on the sheaths of the flexor tendons of the wrist-joint; they are those that were named "hydatids of the wrist" by Dupuytren, and "crepitant tumors" by M. Velpeau. M. Gosselin states that two distinct bursæ exist on the flexor tendons of the wrist, instead of one, as usually mentioned by anatomists; that some variety, however, is met with in the position of these. This anatomical fact is applied by M. Gosselin to the explanation of the more frequent occurrence of these tumors on the inner side of the joint. The second, or ganglionic cysts, are met with on the dorsal aspect of the wrist, and, according to M. Gosselin, originate in certain normal appendices to the synovial membranes of the joint, which may be regarded as synovial crypts or follicles. The author found a pathogenic analogy between these tumors, mucous cysts, and sebaceous wens.

BIOLOGICAL SOCIETY OF PARIS.

PRESIDENT, M. RAYER.

Monthly Summary, March 1880.

Influence of the Nervus Vagus on the Heart's Action.

M. BROWN-SEQUARD related experiments

from which he deduced conclusions the reverse of those which attributed to the *nervus vagus* a direct influence on the action of the heart. He stated that, having destroyed the ganglia of this nerve, and the *medulla oblongata*, in the frog, the pulsations had remained unaffected for twenty or thirty days. M. Brown-Sequard exhibited two frogs in which these ganglia had been destroyed fourteen days previously, and in which the pulsations of the heart were regular and energetic.

The same physiologist also exhibited several animals in which the reflex and other phenomena of animal life were persistent, notwithstanding the whole or greater portions of the spinal cord had been destroyed.

The Anatomical Characters of the Membrana Decidua.

M. CAZEAU presented an ovum of seven or eight weeks old, expelled entire with its deciduous membranes, by which it was completely enclosed. On minute examination this membrane was found to present two surfaces,—one uterine, the other in relation with the chorion, and between these a cavity; thus supporting the theory of the formation of the *decidua* which regards it as a complete sac, formed by the double layers of the membrane, surrounding the ovum as entirely as the shell does the egg of a bird.

Pathological Specimens.

M. GUBLER exhibited a sample of fibrous hypertrophy of Mery's glands (Cowper's).

Vaginal Tumor containing Portions of a Fetus.

M. JOBERT DE LAMBALLE presented a tumor about the size of a hen's egg, which had been removed from the vagina of a woman 41 years of age, in whom it had formed an impediment during labour. It was attached by a pedicle, and after delivery had been removed by ligature; and on examination was found to consist of a sac filled up with adipose matter, intermingled with which were found hair, a fragment of bone, a portion of maxilla with three teeth—a very large molar, an incisor, and a canine, of the size of adult age. In contact with these was a portion of the cranial bones. The summit of the tumor was covered with a cellular-adipose tissue, which proved to be omentum. Its walls consisted of three membranes—an external serous, a middle muscular, and an external mucous.

M. Jobert suggested that this tumor originated in an extra-uterine pregnancy; that the omentum had become adherent to the posterior wall of the vagina by inflammation, drawing with it a portion of omentum and of the broad ligament; and that

in the course of time it had perforated the wall of the vagina, whence it was finally protruded by the child's head during the last labour.

A Case of Obstruction of the Vas Deferens, accompanied by severe pain in the testicles, was related by M. DUNLAP.

Adipose Hamaturia (Milky Urine).

M. BAYER stated that he had, several years since, pointed out the frequent occurrence of this form of disease in tropical climates, especially in the Mauritius. M. Bayer related a recent instance thereof, accompanied with stricture of the urethra. The patient was a native of the Isle of Bourbon, 40 years of age. The milky character of the urine was at first concurrent with the presence of blood in that excretion. Occasionally the urine had been voided for short periods perfectly healthy. M. Bayer had sometimes seen a beneficial influence exerted in these cases by a residence in France.

Teratology.

M. Bayer exhibited a specimen of a supernumerary claw in a craw-fish.

M. DAVAINE submitted observations on the cyclopic malformations, with the following conclusions:—

1. That in cyclocephala the encephalon is always seriously deformed.
2. That its lesions are seated exclusively in the hemispheres.
3. That these lesions, though variable, generally consist in a diminution of the hemispheres, or their fusion into one lobe.
4. A rudimentary nose or snout exists in all these cases.
5. The eye may be present without an optic nerve.
6. The inferior region of the face (the tongue excepted) participates, more or less, in the atrophy of the upper parts.
7. The olfactory nerves may be distinct, or united into one cord.

ABSORPTION OF INSOLUBLE SUBSTANCES.

M. MEISSONIER, repeating the experiments of Osterlin, states that he has succeeded in detecting globules of mercury and particles of sulphur, charcoal, and starch, in the blood, and in the parenchyma of the organs. Having inflated the lungs of an animal that had been fed upon charcoal, and submitted very thin slices to microscopical examination, he says he detected the carbonaceous particles in the intervascular and interlobular spaces, while the same could not be found in other animals of the same species not fed on carbon. By examining the blood of the mesenteric vessels, particles of starch, readily detected by iodine, were met with.—*Archives de Médecine d'Utrecht, et L'Union Médicale.*

Hospital and Infirmary Reports.

WEST NORFOLK AND LYNN
HOSPITAL.CASE OF EXCISION OF THE HEAD OF THE
THIGH-BONE.

BY CHARLES COTTON, M.D., F.R.C.S.
Senior Surgeon to the West Norfolk and Lynn
Hospital.

[Continued from page 166 of Vol. ix. N. 3.]

Dec. 30th, 1859.—Jessie Bullen has improved in every way since the report of the 18th. She moves herself more easily in bed, but yet complains of pain in the back and loins whilst doing so. The hip wound is filled up with granulations, and nearly healed over. The limb operated upon lies comfortably extended on a pillow; but the other still remains awkwardly drawn up and contracted at the knee, resisting every attempt by friction and extension, as well as the endeavours of the patient, to straighten it. Takes the cod-liver oil occasionally. Eats and sleeps well.

1850, Jan. 2d.—Experienced pain and much uneasiness in the hip during the night; thinks she sat up too long and moved too much in bed yesterday. On examination it was found that through some inexcusable negligence the patient had been replaced in bed last evening without the usual outer support to the limb. The limb was much everted, and the tissues about the hip in consequence considerably stretched. A livid red streak was also observed coursing from the perineum over the front of the joint, and the groin was very painful and swollen. The faulty position of the limb instantly rectified, and splints loosely adjusted. Hot chamomile fomentations ordered to the groin. To remain perfectly quiet. The cod-liver oil to be discontinued.

4th.—Has been comfortable and easy; the red streak in the front of the joint has nearly disappeared.

12th.—Is cheerful and happy, and eats heartily, *but gains no flesh*, and the hip wound, though nearly healed over, has a fistulous communication with some burrowing abscess through which a rather profuse purulent discharge issues. Give again the oil. A glass of wine, and $\frac{1}{2}$ pint of porter, daily.

23d.—Has been much ruffled the last three or four days by a bed-sore over the sacrum, from which a subcutaneous passage was found leading to the back of the *opposite* hip, creating an impression of its being also

involved in disease. There is a recurrence of hectic irritation. The cheeks flush as formerly; the pulse is hurried, and tongue furred; the appetite bad, and she has evidently lost flesh; she, however, sleeps well, is free from cough, and the bowels act daily; there is some excoriation about the hip wound, but no pain. Placed on a water bed, and the sore over the sacrum further protected by an ox-bladder partially distended with water. Ordered a palatable sweetened mixture of quinine and sulphuric acid, and to leave off the wine and porter.

Feb. 6th.—Has improved in appearance; is easy and comfortable, but really gains no ground.

9th.—Again relapsing; much discharge from the sores. An abscess pointing below the left hip; ordered *sarsaparilla* and iodide of potassium, and any food or beverage she may wish for.

16th.—Looking better; appetite good. Bed sores easy, and improved in appearance.

22d.—There exists now, in addition to the sacral bed-sore, a large abscess below the left trochanter; the granulations of the right hip wound have given way, and the groin is oedematous and painful. The ex-haemating discharge has not yet made very great impression, owing to her being able to take freely generous and varied nourishment. Ordered *Syr. Papav. et Acid. Nitr.* Dil. p. r. n.

28th.—Hectic distress, and slow emaciation. Tumefaction and fluctuation in front of the joint operated upon.

March 6th.—Large abscess pointing in right groin.

14th.—Abscess dispersed, the discharge escaping through the hip wound.

April 6th.—Psoas and gluteal abscesses on both sides. The poor girl is quite resigned, and is slowly sinking.

16th.—Death.

Post-mortem.—Body emaciated to an extreme degree, the lower vertebral spinous processes denuded of skin, and the integuments over the sacrum and sacro-iliac articulations destroyed. Extensive caries of sacrum and lumbar vertebrae, and destruction of intervertebral substance, communicating internally and externally to each ilium, with the different abscesses. Lungs and other viscera healthy.

Appearances at the seat of operation.—Limb shortened about four inches; the extreme point of the femur discoloured and dead-looking, protruding at the outlet remaining at the hip wound. On reflecting the integuments, the end of the bone was found rising just above and upon the upper margin of the acetabulum, in a promising joint cavity, having the gluteal muscles at-

tached by tawny-looking ligamentous structure, serving in front and externally as the capsule of the joint; internally strong ligamentous and muscular attachments also existed.

The new joint end of the femur excited considerable interest; it was found much expanded out, and presented a considerable quantity of ossific reproduction, forming an admirable compensatory head to the shaft of the thigh-bone and miniature trochanters for the attachment of muscles; posteriorly it had suffered from pressure, and was somewhat damaged by caries. An attempt was made to straighten the obstinately contracted state of the left knee: this was effected after dividing the hamstring tendons, when a similar laceration of integuments to that reported during the life of the patient took place.

REMARKS.—In the notes already published in the *GAZETTE*, referring to this case, it is stated that the suffering and hopeless condition of the patient, and the favourable agency of chloroform, alone prompted the attempt at relief by means of excision of the luxated head of the femur, and that nine weeks of progressive amendment, and the greatly improved state of the girl's health, had not only seemed to sanction the operative procedure, but had even led to anticipations of further well-doing and the possible favourable termination of the case. This impression was much strengthened on the 18th of December, when nature spontaneously threw off the only apparently remaining portion of the diseased shaft, leaving the extremity of the bone in a condition similar to that observed in the stump shortly after amputation. From this period it will be seen, by the clinical reports now given, that the case continued to wear a most satisfactory aspect up to the 2d of January, when for the first time a disagreeable train of symptoms presented themselves at the seat of operation, following, if not caused by, provoking negligence. These, though succeeded by periods of amendment, failed to lull suspicions of lurking mischief, the patient remaining in a stationary condition, and the hip-wound continuing to discharge freely as if from some distant source of mischief. The misgivings entertained were soon after confirmed by the appearance of fresh manifestations of constitutional scrofula, placing the case beyond hope: from this time, Jan. 23d, the patient lingered in a state of calm resignation, until April 16th, when she sunk, emaciated and exhausted, after surviving the operation six months, during which she very frequently expressed her thankfulness for the relief and comparative comfort it had afforded her. The post-

mortem appearances at the hip demonstrated how admirably nature adapts herself to such emergencies, and confirmed the opinion previously formed, that failure in the present instance was alone attributable to the advanced stage of disease and *exhaustion* at the time the operation was undertaken.

The want of success, though not at all surprising under such unpromising circumstances, may possibly serve equally the exclusive advocate and the unmitigated opponent of the operation, as a subject for criticism; the one referring to the period of relief, prolongation of life, and the state of the parts operated upon in favour of the proceeding, but alleging from the result that the case was improperly selected. The other, perhaps not less captiously, insisting that the operation was altogether futile and inadmissible, and that the same amount of ease might have happened had it not been performed at all. In such a spirit of partizanship I take no share: neither do I presume from my limited observation to offer any positive opinion as to what cases are most fitted for the operation. On the score of severity or difficulty I certainly think no objection can be urged against it, having found the exposure, isolation, and sawing off the head of the bone a much easier proceeding than I had experienced in two resections of the elbow; and in another case, now in progress at the hospital, at which I assisted, the head of the femur (truly greatly absorbed) was readily reached and turned out of the wound, and the operation completed with the greatest ease. Though fully alive to the prejudicial influence of scrofulous cachexy, and inclined to believe that the operation is rather applicable to a necrosed than to a carious condition of the bone, I yet should feel justified in again resorting to excision (in the absence of visceral complication), even under like unfavourable circumstances, to those above reported, as well as in cases less advanced of *open* hip disease, attended with luxation and disease of the head of the femur.

The complete success attending Mr. Morris's case of excision of the head of the femur, reported in the *Medical Times* of Jan. 19th, 1850, led me to visit Spalding to gain a sight of the patient; and truly nothing could be more gratifying or unexpected. A period of about twelve weeks had sufficed to convert the case into a sort of talipes equinus: the wound was soundly closed, and free motion existed at the hip, the end of the femur being locked apparently against the upper rim of the acetabulum. The youth planted the toes and extremity of the sole of the foot with firmness upon the ground, and skipped and

hopped about with evident ease and satisfaction. Mr. Morris has some reason to be proud, and may be excused being somewhat didactic, his case having been pronounced, by a gentleman who omits no opportunity of writing in favour of the operation, one "*altogether the most satisfactory and interesting of those hitherto recorded.*" I cannot, however, but feel, that, whilst errors of diagnosis as to the position and state of the head of the bone occur even in able hands, and a doubt exists as to the fit cases in which the operation may be undertaken with propriety, it will be right to exercise the greatest caution to avoid a too ready and unnecessary resort to the operation, and I am strengthened in such a view by the recent observation of a formidable case of diseased hip, *attended with lussation*, and considerable sero-purulent effusion, where the joint has been tapped several times, and in which the head of the femur now lies quietly upon the dorsum of the ileum with no other inconvenience than a shortening of the limb and some considerable lameness.

Medical Trials and Inquests.

DEATH FROM CHLOROFORM IN SURGICAL OPERATIONS.

AN investigation took place last week, before Mr. Payne, coroner for London and Southwark, and a special jury, in the board-room of Guy's Hospital, respecting the death of Alexander Scott, aged 34, a police constable of the B division, who died from the effects of chloroform, while undergoing an operation for the removal of a portion of the right hand.

James Scott, of Creek-road, Deptford, stated that the deceased was his brother. In May, 1849, the deceased received an injury to the right hand, while on duty, by having one of his fingers bitten by a man whom he had taken into custody for creating a disturbance. The middle finger was soon afterwards amputated by a surgeon at Deptford, but deceased always complained of great pain in the limb. On Wednesday week, deceased was advised to apply at Guy's Hospital, with a view of having some operation performed to relieve him. Witness came to the hospital on Wednesday last, just after the operation had been performed, and then found his brother dead. By the coroner.—Had heard since the death that deceased had died under the influence of chloroform, taken by his own wish, as he (the deceased) could not bear the pain attending the operation.

John Lacy, dresser to Mr. Edward Cock, one of the surgeons, stated that he first saw the deceased on the morning of Wednesday last. Witness examined the right hand, and found that the second finger had been removed at the knuckle-joint. Mr. Cock was present, and every thing having been arranged chloroform was administered to the deceased by the machine, but not taking effect it was applied by a napkin. In about two minutes the chloroform had the proper effect. The operation was then commenced by Mr. Cock, when the deceased appeared to be fainting, and just as Mr. Cock had completed it the deceased appeared to be dead.

Mr. Edward Cock, surgeon to the hospital, said, the deceased was sent to him by a surgeon at Deptford for severe pain in the right hand. One of the fingers had been removed, and deceased suffered great pain at the stump. Witness found him to be a strong vigorous man. The bone was in a diseased state, as were also the nerves, producing very great tenderness of the arm and right side of the body. The only remedy was to remove the end of the bone and diseased nerves. The deceased came into the hospital to have the operation performed; when deceased said, "I hope, Mr. Cock, you will give me chloroform, for I cannot bear the pain." Witness informed him that he would rather not use it, and endeavoured to dissuade him from taking it; but the deceased added that he had made up his mind to have it administered to him. Witness always objected to the use of chloroform, for it could never be given without some degree of danger. The ordinary machine was used, and, as it had not the effect, witness directed that a napkin should be folded into the shape of a cone, which was applied with chloroform. The operation of removing a portion of the bone occupied one minute and a half, but before it was completed, the blood which was gushing out suddenly stopped, when witness directed Mr. Lacy to feel the pulse of deceased, and they then found that deceased had expired. *The Coroner.*—What was the cause of death?

Witness.—The chloroform, most undoubtedly. In this instance, a very small quantity had been used, not a *tenth part* of what had been administered in other cases. Witness could not account for the patient dying, and was certain there was no disease about him. So strong and powerful an agent was chloroform, that it could not be administered without some amount of risk and danger, and the penalty the public must pay for the alleviation from pain would be a death occasionally. A similar death occurred, about 12 months since, at St. Thomas's Hospital, and many

other deaths might be recorded. It might be used one or two thousand times, or more, successfully, and was of great assistance to the operator. *The public ought to know the danger and great risk attending its administration.* In many cases where chloroform had been used witness had waited with breathless attention for the recovery of the patient. *Witness strongly objected to its use.*

The coroner then summed up the evidence, and the jury returned a verdict of "Died from the effects of chloroform."

Medical Intelligence.

THE MEDICAL REFORM QUESTION. RAIL- LATING QUESTIONS OF THE NATIONAL INSTITUTE.

Notice and Schedule addressed by the Council of the National Institute to the General Practitioners of Medicine, Surgery, and Midwifery in England and Wales.

Hanover Square Rooms,
4, Hanover Square,
June 24th, 1880.

SIR,—Five years ago the sentiments of nearly five thousand general practitioners were recorded in favour of a new and independent incorporation of the general practitioners in this kingdom, and recent circumstances have rendered it incumbent that the opinions of the same class of the profession should be again taken on the necessity and expediency of this measure.

Having, by repeated communications with the Secretary of State for the Home Department, and with the Council of the College of Surgeons, fully ascertained the impracticability of so altering the constitution of the College as to render its arrangements acceptable to the general practitioners throughout the country; and since any alteration that could be effected in the College of Surgeons would not meet the just complaints of the large number of medical men practising legally, although not members of the College, the Council of the Institute consider that any further attempts to open the College must prove fruitless, as respects the main objects of medical reform, and that the only means of improving the status of the general practitioners, and restoring peace to the profession, would be by the establishment of an independent college, giving to the general practitioners the full control of the education of their members in medicine, surgery, and midwifery, and enrolling, as

one body corporate, in an institution of their own, every general practitioner of the kingdom.

I have the honour to be, sir,
Your obdt. humble servant,
GEOFFREY ROSS, Secretary.

Objects of the National Institute of Medicine, Surgery, and Midwifery.

The principal objects of the Institute are, to maintain an effective organization of the general practitioners—to expose, discourage, and suppress, by registration, and every other practicable means, illegal and unqualified practice—to employ all legitimate means for the purpose of urging upon the Government and the Legislature the claims of the general practitioners of this country to corporate rights—to promote a high standard of education and qualification, with a satisfactory test, by efficient examination, for every individual authorized by law to practise medicine, surgery, and midwifery—and to form, irrespective of the special colleges, an Institution comprising within itself the entire range of medical and surgical knowledge.

The body of general practitioners includes—Every gentleman who was in practice previous to the 1st of August, 1815; and every Licentiate of the Apothecaries' Society; also every Fellow or Member of any Royal College of Surgeons in England, Ireland, and Scotland; every Doctor or Bachelor in Medicine, of any University of the United Kingdom; and every Fellow or Licentiate of any College of Physicians of the United Kingdom,—in actual practice as a general practitioner.

NOTE.—According to the principles of the National Association and of the National Institute, the dispensing of medicines ought not to constitute a disqualification for offices of honour and emolument. The dispensing of medicines must at all times rest with the individual, and must depend upon the circumstances under which he may happen to be placed with his patient and the public.

Schedule.

Are you desirous of a separate incorporation of all the qualified general practitioners of medicine, surgery, and midwifery in an independent college of their own, upon the elective principle, comprising within its own limits the entire range of medical and surgical science and practice, with the control of the education and examination of future members?

Signed

Date

POOR LAW UNIONS.

ON Saturday last, a Parliamentary return was issued, showing that in 1849 the sum of £78,424 16s. 7d. was expended in the salaries of medical officers, and £20,529 in the salaries of schoolmasters and school-mistresses; making a total of £98,953 16s. 11d. under the head of Poor Law Unions, in England and Wales.

GERMAN HOSPITAL, DUBLIN.

ON the 27th ult., the annual court of the governors of this hospital was held at the London Tavern. The report spoke favourably of the progress of the institution during the past year; the income having enabled the committee, after paying off the expenses of the institution, to pay the £500 which remained due for the premises, and to invest £1,000 in the Three-and-a-quarter per Cents. At the anniversary dinner the large sum of £1,847 18s. 7d. in donations, and £80 1s. subscriptions, was realised, including contributions from the Emperor of Austria, the King of Bavaria, the Senate of the free city of Frankfort-on-the-Maine, and his Royal Highness the Duke of Cambridge. The number of patients had greatly increased: the in-patients during the year numbering 474, of which 21 were accidents; and the number of out-patients being 2,895: making a total of 10,484 patients relieved since the opening of the hospital in 1846. The income during the year had amounted to £2,831 10s. 9d., in addition to the £1,400 for the sale of the garden; and the expenditure amounted to £2,866 19s.: leaving a balance of £144 11s. 9d. The expenditure included the purchase of £1,000 stock. This report was adopted on the motion of Dr. Sieveking.

THE HARVEIAN ORATION.

ON Saturday last, according to ancient usage, this oration was delivered at the Royal College of Physicians, Trafalgar-square, in the presence of Dr. Paris, President of the College, several members of the council in their robes of office, and a numerous assemblage of other distinguished members of the medical profession. The oration, which was in Latin, was delivered by Dr. Wilson, of St. George's Hospital, and drew forth repeated marks of approbation from the audience. Allusion was made, among other topics, to the ravages of cholera during the past year, and the degree of skill, perseverance, and philanthropy exhibited by the medical profession during that trying period. The new-fangled doctrines of hydropathy, homoeopathy, and mesmerism, were also touched upon and criticised. The learned doctor did not omit the usual tribute of respect

to the genius of Harvey, for his great discovery.

MEMOIR OF THE LATE DR. S. A. BARDSELEY, OF MANCHESTER.

WE recently recorded the death of this excellent provincial physician, and we here subjoin a few particulars regarding his life and professional progress.

Samuel Argent Bardsley was born at Melvedon, in Essex, on the 27th April, 1764. When in his fifteenth year he was apprenticed to a respectable surgeon in Nottingham. At the termination of his apprenticeship, at the age of 21, he continued his medical studies in London, Edinburgh, and Leyden, at which latter university he graduated in the year 1789. It was the first idea of the deceased to settle in London, but he was diverted from this intention, and for two or three months resided in Doncaster, with the view of practising as a physician in that town. Towards the decline of 1790, however, the year succeeding that of his graduation, circumstances rendered an appointment to the Manchester Infirmary open to him, and he was finally induced in November of that year to take up his residence in that town. Hereupon, his election as physician to the infirmary ensued, and for thirty-three years the deceased was actively and zealously engaged in the performance of these responsible duties which attach to such an office. During the whole of this period he constituted the very model of an hospitable physician, both as regards the scientific direction of his medical observations, and the warm-hearted benevolence and cheerfulness with which his services consoled the objects of his care. He resigned this appointment on the 4th of August, 1828. From the period of Dr. Bardsley's relinquishment of the medical duties of the infirmary, he gradually ceased to undertake the care of private patients; so that, in fact, at the time of his decease, he had been for nearly twenty-seven years a retired physician. Early in May last, he left Manchester, and after passing one week in Cheltenham and another in Leamington, he went up to London, and remained there for a week apparently in his best health. On the 24th he left the metropolis, to pay a visit to a friend near Hastings. The day after his arrival, he complained of slight indisposition, as if he had taken cold; feverish symptoms ensued, which, continuing unabated, terminated in his death on the 28th ult. He sank gradually, with little or no pain, in a state of very imperfect consciousness. Dr. Bardsley was a man of uncommon powers and attainments. Though possessed of no remarkable depth or originality of mind, there was yet a keen sagacity and accuracy

of observation, that, to a practical physician, were not probably of less importance. His reading, both medical and general, was extraordinary, and there were few facts upon which a singularly retentive memory did not enable him to shed light. In early life he published a valuable and useful volume of "Medical Reports," which to this day ranks in the standard literature of the profession. He also wrote several papers published in the Manchester Philosophical Society's Transactions, and contributed to the medical journals of the day several papers upon topics of practical interest. His body was privately interred in the family vault at St. Saviour's, Chorlton-upon-Medlock, it being conformable to the express injunctions of his will that his funeral should thus be conducted.

THE INFLUENCE OF THE MIND ON DISEASE. THE GREAT EFFICACY OF SUGAR OF MILK, WHEN PRESCRIBED UNDER THE FORM OF HOMŒOPATHIC GLOBULES.

SOME genuine homœopathic globules were lately submitted to chemical analysis, under the following circumstances. A lady who had been suffering from chronic gastric irritation, and who could get no rest at night, swallowed *three* of the globules by the advice of a homœopathic friend, who assured her they would procure refreshing sleep. In about two hours after having taken them, she fell into a sound sleep, and rested well for the first time for many weeks. The good effects were of course ascribed to the globules.

The globules were white,—smaller than millet-seeds, and three of them (*i. e.* the dose taken) weighed 0·04 gr. or about one twenty-fifth part of a grain. The paper represented that they were globules of *Nux vomica*. When heated on platina, they burnt with the smell of burnt sugar: they were quite soluble in hot water, solution perfectly neutral, and the tests of Trommer and Moore showed that sugar (glucose), was the principal if not the only constituent. The powder tested by the nitric and iodic acids was unchanged: it gave not the slightest indication of morphia or brucia. The most delicate test for strychnia, *i. e.* sulphuric acid and chromate of potash, shewed that not a particle of that alkaloid was present.

It may be said that the principles of *nux vomica* were in the globules, but in too small a quantity to admit of detection by chemical processes. If this be so, how can it be shown that there was any *nux vomica* in the globules at all? No assertion of this kind can be received without satisfactory proof, or we may as well all back at once to the days of super-

stition, and put faith in amulets or charms. If chemistry fail to reveal the active constituents of homœopathic globules, how are those containing arsenic to be distinguished from those which contain sulphuric acid; or how does a homœopathic practitioner, receiving these globules from his druggist in implicit faith, know that he is not, by some mistake, giving his patient *cayenne pepper* instead of *nux vomica*. A slight shake of the box, containing sets of the globules, will at once confound antimony and opium, arnica and arsenic; and in fact, but for their being, as we shall presently prove, quite innocent of the results ascribed to them, globules of the most dangerous medicines might be substituted for others which are comparatively inert.

The first element of homœopathic therapeutics is, therefore, that a man does not know, and has no means whatever of ascertaining, the nature of the medicine which he is prescribing! He believes his druggist, and his patient believes *him*, when he says she is taking arnica and not arsenic: and there is an end of the matter. Prescriber and patient must be equally credulous, assuming that the former is acting *bonâ fide*, and not attempting, like some of the craft, to CURE his patients by giving them formidable allopathic doses of morphia and calomel, under the name of homœopathic powders.

There is, however, another point of view in which this case deserves to be contemplated. Admitting that the three globules contained half their weight of some narcotic, what is the medicine which in the dose of the *fiftieth part* of a grain will procure a sound sleep of six or seven hours duration?

But these globules were marked *nux vomica*, of which the active ingredient is strychnia, an alkaloid that forms about 1-200th part by weight of the powder. On the assumption that *nux vomica* formed the 50th part of a grain in the three globules, the quantity of strychnia therein contained would be only the 1-10,000th part of a grain. Can any rational person believe that this dose of strychnia, mixed with the 50th part of a grain of sugar of milk, would produce in an adult a sound sleep of seven hours duration? If so, he is bound to believe in the cure of diseases by amulets and charms. But it may be said, what produced the sleep? The answer to this is very simple—the patient was subsequently made to swallow, *without being aware of it*, a similar dose of the globules; and on this occasion, she passed as restless a night as when she had been under allopathic treatment! It was not therefore the globules, but the impres-

sion produced on her mind and feelings, which gave rise to sleep.

The following is another curious case communicated to the writer by an eminent physician, now practising in London. He says, I met R. C. at Florence, whither he had come from some of the baths in Germany. He informed me that he had suffered martyrdom from indigestion and confinement of bowels, but that the latter had been quite cured by a homœopathic physician, whom he accidentally fell in with in his travels, and who had proffered his advice, unasked, to one who had no previous faith in his system.

This gentleman, as he stated, without enjoining any particular rules of diet, merely gave him *three small pills*, and assured him that each of them would infallibly keep his bowels in a *regular state, causing an evacuation once a day, for a month*. R. C. told me that the event perfectly bore out the prognostication, and that at the end of three months a habit of regularity was completely established!

The nature of these pills was not determined by analysis; but is there any one acquainted with the physiology of man, and the nature of drugs, who could believe that three small pills would exert the powers here assigned to them? We think not.

Homœopathic therapeutics fail as much when submitted to the balance and chemical analysis, as when examined by the light of reason. They rest entirely upon hypothesis. Nothing physical can be proved with respect to them. A lately deceased homœopathic physician remarked, when no trace of sulphur was found by chemical tests in his sulphur-globules, that in his opinion *the matter became spiritualised by trituration!* In what respect does such an hypothesis as this differ from the curing of ague by suspending a written charm to the neck? or from curing warts by cutting notches in an elder stick.

The homœopathic practitioner wishes us to believe that the power of medicine is increased by the diminution of dose, and that the power acquires its maximum when no chemical test can show the presence of the medicine in a globule of sugar. Further, that substances like charcoal, which have no action in large doses, become very energetic when the dose is so reduced that no charcoal can be discovered. This it will be seen is simply asking us to believe that a part is greater than the whole, and to disbelieve all that experience has hitherto taught us.

APOTHECARIES' HALL.

NAMES of gentlemen who passed their examination in the science and practice of

medicine, and received certificates to practise, on Thursday, 27th June, 1860:—John Thompson, Cowpen, near Newcastle-on-Tyne—Crosby Leonard, Bristol—Henry Briggs, Halifax, Yorkshire—George Pain, Salisbury—William Felliter, Wareham, Dorsetshire—Thomas Andrews Chaldecott, Montague Place.

Selections from Journals.

UTERINE NEURALGIA.

M. VALLEIX has pointed out a class of cases in which the cervix uteri, usually insensible, becomes the seat of the most acute suffering of a neuralgic nature, and has pointed out the diagnostic distinctions between these and cases of painful inflammatory congestion of that organ.

This neuralgic form of disease is frequently met with, but is not often recognized. In all the cases which have come under M. Valleix's notice, the affection has been accompanied by pain following the course of the lumbo-abdominal nerves. He, therefore, regards the neuralgia of the cervix as part of the more extensive lumbo-abdominal neuralgia. This feature is of importance in reference to the diagnosis of neuralgia from other pains of the cervix uteri.

The subjects of this neuralgic affection present all the characters of sufferers from chronic uterine congestion, with which disease the former is almost always confounded. The pain is much augmented at the menstrual period, producing dysmenorrhœa. The vaginal discharge which occurs in these cases is analogous to the increased secretion from the mucous membrane of the eyelids in certain affections of the trifacial nerve. They are both functional disturbances originating in disordered nervous influence.

Careful exploration is of the first importance to its detection. The neck of the uterus will be found tender to the touch, often so to a great degree, the tenderness being most acute at the sides of the cervix, while the anterior and posterior surfaces are free. The cervix is of its ordinary form and size.

On examining the abdomen and loins, a neuralgic pain will be detected in the hypogastrium a little beyond the middle line on one side, most frequently the left only. Along the course of the first pair of lumbar nerves will be discovered other points, more or less acutely painful, and more or less isolated. The painful point always corresponds with the seat of pain in the uterus.

The chief diagnostic features are, the degree and isolation of the tenderness, the intermittent character of the pain, and the occurrence of the neuralgia of the abdomen and loins.

The treatment adopted by M. Vallin has been: Misture to the hypogastric region, cauterization of the cervix, and narcotic injections, with absolute rest and general treatment.—*L'Union Médicale.* x

OBITUARY.

On the 28th ult. at Holloway, after a few hours' illness, Dr. Carrington, for several years resident medical officer to the Holloway and North Islington Dispensary.

On the 12th of April, at Accra, western coast of Africa, in the 31st year of his age, Frederick Burton Phillipson, Esq., Assistant Surgeon to the Forces, third son of the late Major George Burton Phillipson, of the Hon. East India Company's Service.

BOOKS & PERIODICALS RECEIVED FOR REVIEW.

DURING THE LAST TWO WEEKS.

Diseases of the Breast, and their Treatment. By John Birkett, Assistant Surgeon to Guy's Hospital.

Bedside Sketches on the Pathology, Diagnosis, &c. of Consumption. By J. C. Hall, M.D.

* * * We may here remark for the information of our readers, that this pamphlet contains the series of papers, by Dr. Hall, recently published in this journal.

Memoir on the Influence of Hypertrophy and Dilatation on Diseases of the Heart, &c. By A. H. Douglas, M.D. Ed.

Braithwaite's Retrospect of Medicine. Jan. to June 1850.

Dr. Ranking's Half-Yearly Abstract of the Medical Sciences. Jan. to June 1850.

The Cyclopædia of Anatomy and Physiology. By R. B. Todd, M.D. F.R.S. Part 89.

Report of the Committee of Visitors and Medical Superintendent of the Devon County Lunatic Asylum. 1850.

Casper's Wochenschrift der ges. Heilkunde. Nos. 21, 22—25 Mai; 1 Juni.

Henke's Zeitschrift für die Staatsarzneikunde. 24. Ergänzungsheft. 1850.

Comptes Rendus. Nos. 23, 24—3 and 10 Juin.

British-American Medical and Physical Journal. June 1850.

The British and Foreign Medical-Chirurgical Review. July 1850.

Dr. Winslow's Journal of Psychological Medicine. July 1850.

London Journal of Medicine. July 1850.

The Monthly Journal of Medical Science. July 1850.

Edinburgh Medical and Surgical Journal. July 1850.

The Pharmaceutical Journal. July 1850.

The Veterinary Record. July 1850.

BIRTHS & DEATHS IN THE METROPOLIS

During the Week ending Saturday, June 22.

BIRTHS.	DEATHS.
Males... 748	Males... 335
Females... 690	Females... 429
1447	965

CAUSES OF DEATH.

ALL CAUSES	965
SPECIFIED CAUSES	953
1. Zymotic (or Epidemic, Endemic, Contagious) Diseases...	150
Sporadic Diseases, viz.—	
1. Dropsy, Cancer, &c.	34
2. Brain, Spinal Marrow, Nerves, and Senses	128
4. Heart and Bloodvessels	46
5. Lungs and organs of Respiration	91
6. Stomach, Liver, &c.	59
7. Diseases of the Kidneys, &c.	17
8. Childbirth, Diseases of Uterus, &c.	10
9. Rheumatism, Diseases of Bones, Joints, &c.	10
10. Skin	3
11. Old Age	32
12. Sudden Deaths	37
13. Violence, Privation, Cold, &c.	115

The following is a selection of the numbers of Deaths from the most important special causes :

Small-pox	10	Convulsions	39
Measles	17	Bronchitis	30
Scarlatina	14	Pneumonia	35
Hooping-cough	19	Phthisis	137
Diarrhoea	33	Lungs	6
Cholera	2	Teething	6
Typhus	34	Stomach	5
Dropsy	15	Liver	5
Hydrocephalus	26	Childbirth	6
Apeplexy	28	Uterus	3
Paralysis	20		

REMARKS.—The total number of deaths was 55 above the average mortality of the twenty-sixth week of the previous years.

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer	29.98
" " " Thermometer	64.7
Self-registering do. Max. 70.4 Min. 63°	
* From 12 observations daily. * Sun.	

RAIN, in inches, 0.62.—Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was 5° above the mean of the month.

NOTICES TO CORRESPONDENTS.

Communications have been received from Dr. Barnes, Dr. Routh, Dr. Bird, Mr. R. H. A. Hunter, and Mr. Jennette. These will be inserted with as little delay as possible.

We regret that we are still obliged to postpone the insertion of several papers which are in type.

Lectures.

LECTURES

ON INFLAMMATION,

(Delivered in the Theatre of the Royal College of Surgeons of England).

BY JAMES PAGET,

Professor of Anatomy and Surgery to the College.

LECTURE V.

Effects of inflammation on the part in which it is seated; their generally destructive character—Softening of inflamed parts, as of brain, bones, ligaments, &c.—Fatty degeneration of inflamed parts, as in the liver and heart.—Interstitial absorption of inflamed parts, as seen in bones, cartilages, glands, &c., and in the spontaneous opening of abscesses—Ulceration; ejection of tissues disintegrated after degeneration; supposed effects of corrosion and solution of tissues by ichorous discharges.—Gangrene.

THE account of the results of inflammation in the part in which it has its seat will include the chief among its second class of effects—the destructive effects. For I believe that nearly all the effects of inflammation are injurious, if not destructive, to the proper tissues of the part in which it is seated. All the changes I shall have to describe are characteristic of defective or suspended nutrition in the parts: they present varieties of degeneration, modified by the circumstances in which they have occurred, and especially by this,—that while the changes of the proper tissues of an inflamed part are making progress with all the characters of degeneration, the lymph-products of the inflammation may be in process of either development or degeneration in their interstices, or on their free surfaces.

One of the most common effects of inflammation in an organ is a more or less speedy *softening* of its substance: and this is due not only to infiltration of it with fluid, but to a proper loss of consistency, a change approaching to disintegration, of which, indeed, it is often the first stage. Of such mere softening, some of the best examples are in the true inflammatory softening of the brain and spinal cord, in which the softened part is usually found to consist of spoiled nervous substance, together with more or less abundant granular

products of inflammation. Such softening also may be found in the lungs: the peculiar brittleness and rottenness of texture, which exists with the other characters of hepatization, are evidently due to changes in the proper tissue, more than to incorporation of the products of inflammation. In staphyloma of the cornea, similar softening ensues in connection with the opacity and other changes of appearance. But, perhaps, the most striking instance of softening in inflammation (and it is the more so because the softening probably precedes the other evident signs of inflammation*) is to be found in bones. One may generally notice that an acutely inflamed bone is soft, so that a knife will easily penetrate it. Thus it may be found in the phalanges of the fingers when they partake in deep-seated inflammation, and thus, sometimes, in the neighbourhood of diseased joints. The change depends partly on an absorption of the earthy matter of the bone, this constituent being removed more quickly, and in greater proportion, than the animal matter; but the entire material of the bone is softened.

The softening of bones may permit of peculiar subsequent changes, especially of their swelling and expansion. Thus, in a remarkable case communicated by Mr. Arnott to Mr. Stanley, after excision of the corresponding ends of the radius and ulna, inflammation ensued in the shaft of the humerus, and after four months the patient died. The end of the inflamed humerus was full-red, and swollen, with an expansion or separation of the layers of its walls. And the case showed well the coincidence of absorption and of enlargement by expansion; for though the diseased humerus was thus enlarged, and contained more blood than the healthy one, yet it "was found not to weigh so much by half."†

Similar expansions of bone, with all the characters of inflammation, and such as could not have happened without previous softening of the tissues, form part of the many swollen and enlarged bones which are common in all our museums.‡ Doubtless, in many of these cases, the disease has been of very slow progress, and the separation of the several layers of the compact bone, which the specimens display, must be ascribed to their gradually altered form, as they have grown about the enlarging blood-vessels and interstitial inflammatory deposits. But in other cases the expansion has in all probability been more rapid, the softened bone yielding and extending, as

* See Kuss, as quoted by Virchow, in his *Archiv.*

† Mr. Stanley's Illustrations, pl. i. fig. 4, 5, 6.

‡ In the College Museum, No. 593 to 600, and 3062 to 3064; and in the museum of St. Bartholomew's, Series I. Nos. 55, 94, 113, 114, 115, 120, 197, 198, &c.

the naturally softer tissues do, in an inflammatory swelling.

The characters of a bone thus expanded are easily discerned. Its substance may be irregularly cancellous or porous; but the most striking change is a more or less extensive and wide separation of the concentric laminae of the walls of the bone, so that the longitudinal section of the enlarged wall appears composed of two or more layers of compact tissue, with a widely cancellous tissue between them: and these layers may sometimes be traced into continuity with those forming the healthy portion of the wall. Usually, the separated layers are carried outwards, and the bone appears outwardly enlarged; but sometimes the inner layers of the wall are pressed inwards, and encroach upon the medullary tissue. In the first periods of the disease, the cancellous tissue between the separated layers of the wall has wide spaces, which are usually filled with a bloody-coloured medulla: but this tissue, like the often coincident external formations of new bone, appears to have a tendency to become solid and hard; and its fibrils and laminae may thicken till they coalesce into a compact ivory-like substance, harder than the healthy bone.*

Again, for examples of softening in inflammation, I may adduce the softening of ligaments, such as permits that great yielding of them which we see almost always in cases of severely inflamed joints. This is not from mere defective nutrition; for it does not happen in the same form, or time, or measure, in cases of paralysis or paraplegia engendering extreme emaciation. Neither is it from the soaking of the ligaments with the fluid products of the inflammation; for it does not happen in the abundant effusions of the slighter inflammations of the joints; and when ligaments are long macerated in water they yet retain nearly all their inextensibility. It appears to be a peculiar softening, or diminished cohesion, of the proper tissue of the ligaments, the result of a defective nutrition combined with infiltration of inflammatory products.

We may see such changes in the ligaments of all joints; in the hip, in the cases of spontaneous dislocation occasionally seen, independent of suppuration or ulceration of the parts belonging to the joint; in the wrist; when the ulna after disease becomes so prominent; in the vertebrae, especially in the ligaments of the atlas and axis. But we see the effects of this softening best in diseased knee-joints and elbow-joints; and in all these cases we may often observe an

interesting later change when the inflammation passes by. The ligaments softened during the inflammation yield to the weight of the limb, or perhaps to some muscular force, and the joint is distorted. Then the inflammation subsides, the normal method of nutrition in the joints is restored, the elongated ligaments recover their toughness, or are even indurated by the organization and contraction of the inflammatory products deposited in them; but they do not recover their due position; and thus the joint is stiffened in the distortion to which its ligaments had yielded in the former period of inflammation. In the crowds of stiff, distorted, and yet not immovably fixed joints that one sees as the consequences of inflammation, these changes must generally have happened to the ligaments:—first softening and yielding—then recovering, with induration, and perhaps some contraction, due to their atrophy and the organization of the inflammatory deposit. The cases are aggravated by similar changes in the adjacent parts; for the stiffness of such joints is not due to the ligaments alone; all the subcutaneous tissues are apt to be adherent and indurated.

The softening of the tissues of an inflamed part may be regarded as one of the instances of degeneration in the inflammatory process; and its diversity from ordinary degenerations may be ascribed to the simultaneous infiltration of the inflammatory product, and perhaps to some other circumstance we cannot at present trace or guess. But a more general and unmixed form of degeneration may, I think, be occasionally observed in the tissues of inflamed parts—namely, fatty degeneration; and this in such a manner, as to make it probable that the degeneration takes place even during the inflammation. Thus fatty degeneration of the hepatic cells appears an usual coincident of the form of inflammation which produces the so-called "brawny" liver. I think, too, that I have seen fatty degeneration of the muscular fibres in inflammation of the heart; especially in a recent case, in which the heart was punctured with a needle, and the patient died four days afterwards. The portion of the heart near the needle was more degenerate than the rest of its substance. So, also, in some instances of acute ulceration of cartilage, I have found that a fatty degeneration of the contents of the cells, together with similar degeneration or disappearance of the nuclei, constantly precedes the removal of the whole substance of the cartilage.* I am inclined,

* Abridged from the *Pathological Catalogue of the College*, vol. ii. p. 97.

* Similar observations are made by Dr. Redfern, especially in the third case of his excellent

therefore, to believe that this method of degeneration may occur not unfrequently in inflammation of certain parts, and may be, like the softening last described, a principal constituent of the changes preceding their complete absorption.

Both the foregoing degenerative changes in inflamed parts are favourable to complete removal by *absorption*, in which we find another example of the destructive effects of inflammation. And this absorption, which in many inflammatory conditions is a peculiarly rapid event, may affect at once the proper elements of a part, its bloodvessels, and the inflammatory products that may have been previously deposited among them.

I shall refer here only to that which has been called *interstitial absorption*: to the removal of parts from within the very substance of the tissues, as distinguished from the removal by the ejection of particles from the surface, of which I shall next speak as occurring in *ulceration*.

We may believe that such degenerations as I have just described usually precede this interstitial absorption of parts; but we cannot be quite sure of it, because we cannot see the parts immediately before absorption; or, rather, because we cannot be sure that what we see was to have been very soon absorbed. But, as I stated in the last lecture, we are justified in holding that no absorption of the living tissues can take place without previous change or degeneration of that which is to be absorbed. And we may the more certainly believe that, in the interstitial absorption which takes place in an inflamed part, degeneration and disintegration of its tissues always ensue before they are, as the expression is, "taken up," from the observation that the degenerative changes, which I have just described, are sometimes evidently the precedents of absorption. Of such interstitial absorptions of inflamed parts, we find well-marked instances in that form of ulceration of articular cartilages in which the deeper portion of the cartilage is removed, together with the adjacent osseous layer covering the head of the bone; for here we can hardly suppose that the disintegrated particles are cast out, whatever they may be in the superficial ulceration of the same tissue. Such interstitial absorption is seen, too, very well in inflamed bones. The head of a bone may be scarcely enlarged, while its interior is hollowed out by an abscess; what remains of the bone

may be indurated, as by slight and tardy inflammation, but so much of the bone as was where now the abscess is, must have been inflamed and absorbed. Here, too, the evidence of absorption is completed by the similar excavations formed in bones within which cysts and tumors grow; for in these cases no other removal than by absorption seems possible.

To similar absorption of inflamed tissue we may refer the wasting and deformity that we notice in the heads of bones that have been the seat of chronic rheumatism. The best examples of this are in the head and neck of the femur; and the retention of the compact layer of bone, covering in the wasted cancellous tissue of the shortened neck and flattened head, is characteristic of interstitial absorption, as distinguished from ulceration, by which the cancellous tissue is commonly exposed. In these cases of chronic inflammation of the bones, we may notice, also, an appearance of degeneration that precedes a peculiar mode of absorption. While the articular cartilages are passing through the stages of "fibrous degeneration," and are being gradually removed, the subjacent bone is assuming the peculiar hardness which has been termed "eburnation," or "porcellaneous" change. Now this change is effected by the formation of very imperfect bone,—of bone that has no well-formed corpuscles; and resembles the result of mere calcareous degeneration rather than of a genuine ossifying induration. And its character as a degeneration is further declared in this; that it is prone to the destructive perforating ulceration, which often gives a peculiar worm-eaten appearance to the bones thus diseased.*

With these changes in rheumatic bones we may also cite, as instances of absorption during inflammation, the changes which Mr. Gulliver† first described as apt to ensue after injuries about the trochanter of the femur. In such cases, without any appearance of ulcerative destruction, the head and neck of the femur may waste by absorption, the neck becoming shortened, and the head assuming a peculiar conical form. We might regard these effects as a simple atrophy, if it were not that they are like the effects of the more manifest inflammation in the rheumatic cases, and

* A change, which appears to correspond with the eburnation of bone, is described by Mr. Tomes, as occurring in the part of a tooth which lies just beneath a carious cavity. In both cases the induration might suggest that it is calculated to retard the progress of the disease, but we have no evidence that it does this in an effective manner; and in the case of the bones there is every appearance that the destruction is most rapid where there is most induration.

† Edinburgh Med. and Surg. Journal, vol. lxxi. — The change is illustrated in No. 3512 in the College Museum.

essay on "Abnormal Nutrition in Articular Cartilages." Perhaps, also, we ought to classify with the degenerations that may ensue in the proper tissue of an inflamed part, the ossification of the laryngeal cartilages when involved in inflammation, and the fibrous structure acquired in slightly and long inflamed muscles.

that the existence of inflammation during life is often declared by the symptoms following the injury.

Again, other examples of the absorption of inflamed parts, or of parts that have been inflamed, are presented in the wasting of glands after inflammation; as in cirrhosis of the liver, in some forms of granular degenerations of the kidney, in the indurated and contracted lung after pneumonia.

No doubt, in these cases, the reduction of the organ depends, in a measure, on the contraction of the diffused inflammatory product, as it organizes; but in many cases the quantity of new tissue is extremely small (it is so in the shrivelled granular kidney); and, in all the cases, we may well doubt whether the contraction of organizing lymph would produce such extensive and uniform absorption of the proper substance of an organ, if there were not a previous condition favouring the absorption. The most probable explanation of these cases seems to be, that as, in the early periods of the inflammation, the softening and the degeneration of the inflamed tissues coincide with the production of the lymph; so, as the inflammation subsides, and subsequently, the absorption of the degenerated tissues may often coincide with the full organization and contraction of the lymph. And it is altogether most probable that these events are independent though concurrent; that each occurs as of itself, not as the cause or consequence of the others.

To all these cases must be added the fact of the absorption of the blood-vessels, and other accessory apparatus, of the inflamed tissues. The absorption of the absorbents themselves must coincide with that of the tissues. What a problem is here! These, that had once been the apparatus maintaining life, that had been adjusted to its energy and fashion, now, as it fails, remove themselves in adaptation to its failure. How can this be? We can only guess that its method is just the reverse of the method of formation; that, as in growth the blood-vessels and lymphatics follow in the course of evolution of the growing parts, opening and extending into each new part as it forms, so, in decrease, they follow, and closing-in harmoniously with the general involution, mingle their degenerate materials with those of the tissue, and are absorbed by the nearest remaining streams of blood.

Once more; not only the original elements of the tissues may be absorbed, but, even more rapidly, the new-formed products of inflammation. We have the best example of this, as well as, indeed, of many of the facts which I have been mentioning, in the spontaneous opening of a common

abscess; which, though it be so common a thing, I will venture to describe here.

Let us suppose the case of an abscess formed in the subcutaneous tissue; of such an one as may have had its origin in lymph infiltrated through a certain area of the tissues, and forming therein a hard circumscribed inflamed mass. Of this lymph we may suppose the greater part degenerating into pus. It may begin to do so at a central point,—the point at which the conditions of nutrition are the most impaired: or the suppuration may begin at many points at once, and, thence extending, the several collections of matter may be fused together. Sometimes masses of the infiltrated and softened tissue are thus detached and cast loose in the cavity of the abscess (as in these specimens).^{*} But at length we may suppose all the central portion suppurated, while the peripheral part, as happens usually in chronic abscesses, may be more organized, may acquire blood-vessels, and may thus assume the character of a granulation-layer, and form the proper wall of the abscess.

The pus of such an abscess as this will contain, probably, besides its proper constituents, some of the disintegrated tissue of the part in which it has its seat. We cannot, indeed, be quite sure of this; for it may be, that while the lymph is being formed, or being converted into pus, the proper tissue of the part may be undergoing absorption; and although, in the pus of abscesses thus formed, we often find abundant molecular and granular matter, yet this may be the debris, not of the tissue, but of the lymph-cells or pus-cells, or of the fibrine which may have coagulated with the lymph-cells. We cannot, I think, be sure on this matter; but we may be sure that the circumscribed portion of tissue, in which such an abscess as I am describing has its seat, degenerates, and is either absorbed, or else disintegrated so as to mingle more or less of its substance with the pus.

In such an abscess, moreover, we often find a layer of cells, spread out like a thin yellowish-white membrane, on the interior of the wall. They are only like lymph-cells or pus-cells, not yet mingled with the rest of the contents of the abscess: but they have been made to seem more important by being called a 'pyogenic membrane,' and by its being sometimes implied that it is their work to secrete the pus. But the existence of such cells is far from constant in abscesses, and we cannot suppose a special membrane necessary for the formation of pus, while we see the best examples of it formed on

* College Museum, 112, 114.

granulating wounds, and on mucous surfaces, which have no such 'pyogenic' membrane.

The abscess thus formed has a natural tendency to open, unless all the inflammation in which it had its origin subsides. Inflammation appears to be essential to the spontaneous opening of abscesses; for, where it is absent, the matter of chronic abscesses will remain, like the contents of any cyst, quiet, for weeks, or months, or years; and when in chronic abscesses, or in cysts, inflammation ensues through the whole thickness of their coverings, it is usually certain that their opening is near at hand. This difference between acute and chronic abscesses makes it very doubtful whether the inflammation of the coverings of an abscess can be ascribed to any local influence of the pus. But to whatever it may be ascribed, we may refer to this inflammation the comparatively quick absorption of the integuments over the collection of matter: and thus the fact, however we may account for it, that the integuments are more prone to inflammation, and more actively engaged in it, than the other tissues about an abscess are, may be used to explain the progress of matter towards the surface. Possibly—though this I think is much less probable—the tissues between an abscess and the surface may, after the degeneration which accompanies their degeneration, be disintegrated, and may mingle their molecules with the purulent contents of the abscess. But in favour of the belief that they are absorbed we have the evidence of analogy; for just the same thinning and removal of integuments takes place when they inflame over a chronic abscess, with a thick impenetrable cyst, or over an encysted or even a solid tumor.* Here absorption alone is possible; and the cases are so similar to the ordinary progress of abscesses, that I think we may assign all the changes of the integuments over these to the same interstitial absorption.

As the absorption proceeds, the integuments grow not only thinner, but softer, and more yielding. And this softening is worth notice, because one might suppose that as pus accumulates; so the integuments over it would become tenser and more resisting. It is, probably, in great measure, such a softening as I have already spoken of in degenerating inflamed parts; but it may be also due, in some degree, to such a change as that to which Mr. Hunter refers as "the relaxing or elongating process." He used to show this specimen, and say of it in his lectures,

"This preparation represents the front of a chest which contained an aneurism of the aorta; and here, on the right side, you see an instance of the elongating process, the cartilages being bent outwards, or elongated, to adapt themselves to its figure." Of the same process he says elsewhere,* "Besides these two modes of removing whole parts, singly or together [that is, besides the interstitial and the progressive absorption], there is an operation totally distinct from either; and this is a relaxing and elongating process carried on between the abscess and the skin, and at those parts only where the matter begins to point. It is possible that this relaxing, elongating, and weakening process, may arise in some degree from the absorption of the interior parts; but there is certainly something more, for the skin that covers an abscess is always looser than a part that gives way from mere mechanical distension, excepting the increase of the abscess is very rapid.

"That parts relax or elongate without mechanical force, but from particular stimuli, is evident in the female parts of generation, before the birth of the fœtus; they become relaxed prior to any pressure. The old women in the country can tell when a hen is going to lay from the parts becoming loose about the anus."

I have quoted the whole passage, because I believe that more recent researches have done nothing towards either improving the description or explaining the fact, except in so far as they make it probable that the change is due in a measure to the inflammatory softening of the pointing skin.

While these changes of degeneration and removal, of softening and relaxing, are ensuing in the cutis over such an abscess as I have described, we commonly notice that the cuticle separates, leaving the very point, or most prominent part, of the abscess bare. The cuticle is not raised as in a blister, but peels-off like dead cuticle; and we may believe that it is dead, partaking in the failure of nutrition in which all the parts over the abscess are involved, and being removed as a dead, not as a merely degenerated, part: for thus cuticle is always removed.

At length, after extreme thinning of the integuments, they perish in the centre of the most prominent part.—Sometimes the perished part becomes dry and parchment-like, with a kind of dry gangrene; but much more commonly a very small ordinary slough is formed, and the detachment of this gives issue to the purulent matter. The discharge is usually followed by a

* As in No. 121, College Museum.

† College Museum, No. 122; and Catalogue, vol. i., p. 53.

* On the Blood, &c. Works, vol. iii. p. 477.

more or less complete cessation of the inflammation in the integuments, and then the wall of the abscess, having the character of a cavity lined with healthy granulations, heals.

Such appears to be the ordinary course of an abscess; and I venture to hope that the numerous principles of inflammation that it illustrates will justify my having used so much time in describing it.

I proceed now to the consideration of *Ulceration*, as one of the effects produced by inflammation in the proper tissue of the inflamed part.

I need hardly say that, ever since Hunter's time, confusion has existed in the use of the terms employed for various kinds or methods of absorption and ulceration. Of all that Hunter wrote, nothing, I think, is so intricate, so difficult to understand, as his chapter on ulcerative inflammation; and much of the obscurity in which he left the subject remains. For a general consideration of the subject, it may suffice to speak (as I have done) of the removals of the particles of inflamed parts, which are not on an open or exposed surface, as the "interstitial absorptions" of inflamed parts. Then, the term "ulceration" may be employed to express the removals of the superficial or exposed particles of inflamed parts. If these superficial particles may be supposed to be absorbed, the process of removing them may be termed "ulcerative absorption;" but if it is more probable that their removal is effected entirely by ejecting them from the surface of the inflamed part, then the term "ulceration" may sufficiently express this ejection, and will stand in stronger contrast to the "interstitial absorption" of the particles that are not so ejected.

I have lately referred to the uncertainty whether, as the cavity of an abscess enlarges, the tissues that are removed from the inner surface of its boundary walls are absorbed, or are disintegrated and mingled with its fluid contents; in other words, whether they are absorbed or ejected. The same uncertainty exists in the case of ulceration. Is the enlargement of an ulcer effected by absorption of its boundaries, or by the gradual detachment and casting-off of particles from their free surface? Both methods of enlargement may, perhaps, in some cases, ensue; but the probabilities are in favour of the enlargement being, as a rule, effected by the ejection of particles.

Thus:—1. Parts to be removed from a surface are generally cast-off rather than absorbed, as cuticles of all kinds are, and the materials of secretions; so that, by analogy, we might assume that the particles

of the surface of a spreading ulcer would also be cast-off.

2. The materials of the ulcerating tissue may be sometimes found in the discharge from the ulcer. In most cases, indeed, this is impossible; but perhaps it is so only because, when the tissues are degenerate, and broken-up, or decomposed and dissolved, we have no tests by which to recognise them. In the case of bone, however, some of the constituents of which are not so easily disguised, the ejected materials may be found. In one of his lectures delivered in this theatre, Mr. Bransby Cooper mentioned that, while in pus from soft parts only traces of phosphate of lime were found, the pus from around diseased bone contained in solution nearly 2½ per cent.* A similar, but less complete observation, had been made by Mr. Thomas Taylor,† and by v. Bibra;‡ and we may be nearly sure that the phosphate of lime was, in these cases, some of what had existed in the diseased bone.

3. It strengthens this belief to observe, that, in many cases, small fragments of bone and other tissues are detached and cast-out with the fluid secreted from the ulcerating part. These, indeed, when they are not fragments of tissue detached by ulceration extending around them, are good examples of the transition that may be traced from ulceration to sloughing or gangrene of parts, between which, if ulceration be always accomplished by ejection, the only essential difference will be one of degree: the ulceration being a death and casting-off of invisible particles of a tissue, while gangrene implies the death and casting-off of visible portions.

4. And it may be proved of many that we call ulcers that they begin as sloughs which are cast off, and leave the ulcerated surface beneath. We may often see this, on a large scale, in the instances of what

* Medical Gazette, May, 1845.

† Stanley, on Diseases of the Bones, p. 69.

‡ Chemische Untersuchungen verschiedener Eiterarten, p. 85. It may seem that, in these cases, a further proof is needed that the quantity of bone-earth discharged with the pus is proportionate or equal to the quantity lost by the ulcerating bone. But this proof may be neither possible nor necessary; for if what has been already said, of the conformity of the properties of inflammatory and reparatory products with those of the tissues from which they are produced, be true, then will also pus from diseased bone possess more bone-earth than pus from any other tissue, even though the bone be not ulcerating. Granulations upon bone doubtless contain more bone-earth than those from soft parts, and they may ossify: now the relation of pus to granulations is commonly that of degenerating cells to the like cells developing; therefore we might expect that pus from bone, like granulations from bone, will contain a large proportion of bone-earth, independent of what may be derived from the ulceration of the bone.

are called sloughing ulcers; but Dr. Baly has proved it for a much wider range of cases, in his observations on dysentery, in which he has traced, how even the smallest and the most superficial ulcers of the intestine are preceded by the death and detachment of portions of the mucous membrane, with its covering of basement-membrane and epithelium.*

From these considerations, we may hold it as probable that ulceration is, usually, the result of the detachment of dead portions or molecules of a tissue, and that the substance removed in the process is not absorbed but ejected. There are, indeed, some cases which may make us unwilling to admit, at present, that all ulceration is by ejection; such as those of bone ulcerating under cartilage, or in the rapid extension of inflammation within it, or such as the spreading ulceration of the vertebrae, or the heads of bones, that is not attended with external discharge of fluid. These may interfere with the universality of the rule, but not with its generality.

But, if we may believe that the removal of a tissue by ulceration is generally effected by ejection of its substance, the question may be asked, in what form is it ejected? Dr. Baly's observations enable us to say that, in the first instance, a visible slough is detached, a portion of the tissue dying and being disconnected from the adjacent living tissue. But, after this is done, when an ulcer enlarges, or extends and spreads, is the material of the tissue still removed in visible sloughs or fragments? Certainly it is so sometimes; for we may find little fragments of bone in the discharge from ulcerating bone, especially in strumous ulceration. But in other cases we have no evidence of this kind; we cannot detect even microscopic fragments of tissues in the discharges, and we must suppose that they are removed, in a state of solution or of molecular subdivision, in the discharge from the diseased part.

To speak of the solution of tissues in the discharges of ulcers may seem like the revival of an old error long since disproved. But though the expression may be revived, it is with a new meaning. The proof has, truly, been long completed, that healthy tissues, even though they be dead, cannot be dissolved in pus, or any such discharge; but the tissues that bound or form the walls of a spreading ulcer are not healthy; they are inflamed, or otherwise diseased and degenerate; and they may now be soluble in fluids that could not dissolve them while they were sound. Insolubility is as great an obstacle to absorption as to ejection in

discharges; no tissue can be absorbed without being first so far changed as to be soluble in fluids with which it was before in contact and unharmed. Therefore, whether we hold the ordinary spreading of an ulcer to be by absorption of its boundaries, or ascribe it to their ejection, we must, in either case, admit that they are first made soluble. And if this be admitted, then it is most consistent with analogy, and most probable, that the extension of an ulcer, independently of sloughing, is accomplished by the gradual degeneration of the tissues that form its walls, and by their being either disintegrated and cast-off in minute molecular matter, or else dissolved and ejected in solution in the discharges from the ulcer.

The solution here spoken of is such as may be effected by the fluid discharged from any spreading ulcers; and we may doubt whether all discharges from ulcers possess a *corroding* property, such as Rokitsansky seems to ascribe to them, and such as he considers to be the chief cause of the extension of all ulcers. We may doubt, I say, whether all ulceration can be described as a corrosion or erosion of the tissues by ichor; but, on the other side, we cannot well doubt that the properties of the discharge from an ulcer, or a sloughing sore, may have a great influence in accelerating the degeneration and decomposition, and thereby the solution, of the tissues that form its walls or boundaries. Many ichorous discharges from ulcers inflame and excoriate the parts over which they flow; and one constituent of inflammation is the defective nutrition of the proper elements of the affected tissue. Many such discharges, also, are in an active state of decomposition; and their contact with the tissues cannot but have some tendency to excite decomposition in them; a tendency which the tissues will be the less able to resist, in the same proportion as they are already feebly maintaining themselves, or as they have been moved by inflammation from their normal conditions, and their normal tenacity of composition.

On the whole, then, we may conclude, respecting the process of ulceration, that its beginning is usually the detachment of a slough, or portion of dead tissue, by the removal of the layer of living tissue that bounded it; that the spreading of an ulcer, independent of such visible sloughing, is effected by the tissues that bound it becoming degenerate, and being detached in minute particles, or molecular matter, or being decomposed and dissolved in the fluid discharge or ichor; and that this spreading may be accelerated by the influence of the discharge itself, which may inflame

* Gulstonian Lectures. Medical Gazette, 1847.

the healthy tissues that it rests on, and may exercise a decomposing "catalytic" action on those that are inflamed already.

I have already said that the products of inflammation are commonly removed, in ulceration, together with the elements of the tissues in which they are deposited. And all that has been said of the changes that the tissues undergo previous to ejection or solution, may be said, also, of the products of the inflammation which commonly precedes and accompanies the ulcerative process.

I need hardly say that we have no knowledge by which to explain the peculiar and characteristic forms of certain ulcers. We seem wholly without a guide to such knowledge; but the existence of such specific forms is conclusive against the supposition that the extension of an ulcer is entirely due to corrosion by an exuded fluid. Such a fluid would act uniformly, unless the various effects of disease on the tissues bounding the ulcer should make them variously amenable to its influence.

The last enumerated effect produced by inflammation in the tissues in which it is seated is gangrene, mortification, or sloughing.

In the usual enumeration of the effects of inflammation, it is commonly implied that the processes of effusion of lymph, suppuration, ulceration, and mortification, may be taken as the expressions of so many successive degrees of severity of the morbid process. But this is far from being unconditionally true. The exciting cause of the inflammation, the tissue affected, the condition of the blood, the general amount of vital force, and the degrees in which the bloodvessels can adapt themselves to the transit of various quantities of blood; all these, and, perhaps, several other things, have as marked an influence as the severity of the disease has in determining the result of an inflammation to be gangrene of the affected part. It may be nearer the general truth, to say that the probability of gangrene ensuing in inflammation is proportionate to the sum of the intensity of the disease *plus* the debility or defective vitality of the affected part, whether that debility have a general or a local origin. But, indeed, in any case of gangrene that may be regarded as inflammatory, it is very hard to say what may be ascribed to the inflammatory process, and how that process affects the issue, so great are the number, and so diverse the natures, of the several morbid conditions that are from the first, or in succession, involved. Certainly, it is not possible to speak clearly of inflammatory

gangrene without constant reference to many other forms of local death which lie far beyond the range of these lectures. For this reason, and because a separate series of specimens in the Museum is devoted to the illustration of the death of parts, I shall here conclude the account of the phenomena and effects of inflammation.

ON THE NATURAL HISTORY AND CULTIVATION OF IPECACUANHA IN THE BRAZILS.
BY M. WEDELL.

THE *Cephalis Ipecacuanha* was discovered in 1824, in the province of Matto-Grasso, but its cultivation in this district was not commenced until the year 1832. The forests in which the plant grows are readily recognised. They abound in the basin of the Rio Paraguay and its tributaries. It does not thrive on the immediate banks of rivers, their periodical inundations being prejudicial to its growth. It flourishes best in a soil consisting of mixed sand and vegetable mould. The *Cephalis* attains to about the size of the *Daphne* of our own woods. It grows in clumps, which the gatherers, or *poayeros*, (so called from *pe-aya*, the Indian name for *Ipecacuanha*) call *redoleros*.

In collecting the root, the *poayero* seizes in one hand, if he can, all the stalks of each clump, and with the other hand digs them from the earth with a see-saw movement. The proper roots are then picked out. A labourer, in this way, collects from eleven to thirteen pounds a day; which, when dried by the heat of the sun, loses half its weight.

The propagation of the *Cephalis* is by seed, or more frequently from the suckers, which the *poayero* rejects from among the roots, as unfit for commercial purposes.—*L'Union Médicale.* x

*. M. Wedell is not very clear in his account of the early history of *Ipecacuanha*. He no doubt refers to its cultivation in a particular district, since we learn from that excellent authority, Pereira, that *Ipecacuanha* was used in Paris in 1686. The plant was brought from the Brazils in 1800. In 1802, it was described by Brotero under the name of *Callicocca Ipecacuanha*, and in 1818 it received from Richard the name of *Cephaelis Ipecacuanha*.

Original Communications.

OBSERVATIONS ON THE
CONDITION OF THE BODY AFTER
DEATH FROM CHOLERA.By WM. FREDERICK BARLOW, M.R.C.S.
Resident Medical Officer to the Westminster
Hospital.(Suggested by a Case communicated to the
Writer by THOS. GREEN, M.D. F.R.C.S.,
Surgeon to the Bristol Infirmary, &c.)

[Continued from p. 21.]

THE *long delay of rigor mortis* in the case which occurred at Bristol was certainly a very remarkable feature of it. Was it attributable, in any measure, to the warmth of the body being preserved so long? We must not be too ready to violently force *simultaneous* circumstances into the imaginary relation of cause and effect; and so commit an error which has led so often and obviously, elsewhere, to the spread of false knowledge and the discouragement of true. It happens frequently that rigor mortis lingers until the body cools; but it does not invariably happen. I have seen, as many others have done before, animals become rigid, perfectly rigid, whilst warmth remained. But there is an observation which would of itself suffice to prevent any one attributing the deferred rigidity, in the case in question, to the tardy cooling of the corpse. M. Ollivier (as Dr. Taylor has reminded us in his lectures on the signs of death), has found some bodies dead from cholera, *at once very warm and perfectly rigid*. More cases might be cited without trouble or research; but, as they are furnished not by *cholera*, they would, though perfectly applicable, seem less strikingly in point. It is very easy to conceive long retention of warmth, long maintenance of irritability, long retardation of rigor mortis, and long resistance to the changes whereupon putrefaction depends, occurring *together*; and this would seem to have been exemplified in the instance which was observed at Bristol.

There appears to be a marked disuniformity as to the time at which rigor mortis sets in after dissolution by

cholera,—a circumstance to be in part, if not *altogether*, explained, by the fact, that death surprises in this disease at a time when the muscular fibre is most differently situated as to its amount of irritability. It is known well enough that the phenomenon of rigidity is generally postponed until the contractibility of the fibre is either entirely or almost exhausted. And there is certainly no more cardinal feature of this form of muscular contraction, for I cannot but regard it as such, than the fact that, though its *power* and *duration* is as a rule proportionate to the amount of irritability at the time of death, its *occurrence* should be hindered until that property be completely or all but abolished.

When we consider that cholera attacks both the weak and the strong, that it respects no age, and that it happens sometimes in the course of maladies which have produced manifest, perhaps alarming prostration; that its duration varies not a little; that it is attended by cramps, which, to speak comparatively, are slight and partial, or universal and violent, and endure unto death;—we may safely conclude, without any further consideration, that very opposite states of muscular irritability must necessarily prevail at the moment of dissolution, and that the time at which rigor mortis will appear, the force it will be exerted with, and the period of its remaining, will all, as a consequence, be far from uniform.

The state of nutrition of the muscular tissue may be mentioned in reference to rigor mortis, not only as respects death by cholera, but other forms of it. Muscular, or, as they should be called, extraordinarily muscular persons, are of course most likely to exhibit the phenomenon in the most marked and lasting way. Atrophy, or that impaired amount of nutrition which would hardly be recognised as such, (and we do but observe this matter rudely), leads on the other hand to a less palpable and abiding form of it. The remark, as need scarcely be observed, applies equally to the involuntary as to the voluntary muscles—to the heart as to the biceps. Paralysis does not prevent rigor mortis, unless, as Sommer shows, it interferes with the nourishment of the muscles. This physiologist once remarked an entire absence of

rigidity on the paralysed side, the muscles whereof appear to have been in an unhealthy state.

Fatty degeneration, (which I speak of from its commonness), has no doubt a great influence on the degree and duration of rigor mortis.* It would too, I should think, if excessive, either lead to the prevention of cramps in life-time, or very much modify their power, for we cannot but suppose that *cateris paribus* the best nourished and most irritable muscles would be most liable to this peculiar form of spasmodic action. It is with the state of the living, (only in a larger, more emphatic sense) as with the condition of the dead. It is modified by many circumstances that we know, by more that we know not. How can we expect diseases to run their course in sameness, seeing that the subjects of them so manifoldly differ? How look for precisely the like states in death, perceiving clearly that at the time of dying there are many parts and actions of the body extremely differing in different persons?

Even the remedies given for the disease, may, if they have circulated, have more relation to the time of commencement of violence, and duration of rigor mortis, than might be at first supposed. I am led to the remark by the influence of chloroform, which not only, as may be shown by experiment, impairs the irritability of the muscles during life-time, but leads to unusually early rigor mortis. This I have seen myself.† It is well known that the exhibition of chloroform has diminished, and sometimes allayed, the cramps of cholera; and it may be presumed reasonably to have accomplished this by virtue of its power to lessen, or

annihilate, muscular contractility. And it is surely not extravagant to suppose that rigor mortis *might* set in somewhat earlier than common in certain cases where chloroform has been largely given, seeing that whatever impairs the irritability of muscles is favourable to their rapid post-mortem rigidity. Animals killed in the chase, being "tired to death," have been known to stiffen almost as soon as dead. Why so? Because their irritability was utterly, or almost utterly, spent. Had they been destroyed of a sudden, during sleep, there would have been just the opposite condition of the muscular fibre, and a state wherein the postponement of rigor mortis might have been properly looked for.

The degree of swiftness with which a man dies in cholera, and the circumstance of death happening in the cold stage or not, must, of course, be considered in reference to the question of rigor mortis; nor must the fact of dissolution being apparently hurried by sudden efforts on the part of the patient, be forgotten, for death, in such a case, may be supposed to leave the muscles more irritable than they would have been, had the disease been a more lengthened course, and more exhaustion taken place, from distress, sleeplessness, and other sources of prostration ere life was past. But with regard to cholera, as well as with reference to disease in general, the state of the muscular system after death is very apt to be most cursorily touched upon, and the pathologist fancies he has done enough if he note vaguely the circumstance of rigidity, without looking to the period at which it occurred, or sufficiently and fully connecting it with the condition of the body which prevailed in life-time. And yet the phenomenon, taking simply its universality into fair account, is full of interest, but that interest is unmeasurably added to, by the diversity it puts on in conformity with differences of the muscular system in various persons, and the great dissimilarity of their mode of dying. It is a chapter in the history of death, but it forms a portion also of the history of maladies; and it is certainly deserving of a far more general and comprehensive study than it has hitherto received.

Observations have been made upon the position of limbs rigid after death

* It would be of no little interest to examine this question thoroughly, in reference to the heart. What has been called concentric hypertrophy, by a grave pathological mistake, would, I should suppose, never be found where this degeneration is both general and extreme. I asked Dr. Quain to tell me his experience on this point. He says that hearts in such a state of degeneration are "generally found dilated, and their pariete. flabby," but that he has seen them "firmly contracted" where the degeneration was not extensive nor excessive.

† Dr. Tyler Smith says that he has seen it happen in the frog with a ten minutes of the heart ceasing to beat. See London Journal of Medicine, Dec. 1849. It would be important to determine accurately the time at which rigor mortis is wont to occur after death by chloroform in the human subject. Where death is suspected to have been caused by it, the knowledge of this matter *might* perhaps much help inquiry.

by cholera, which might lead to the idea that postures of distortion are *peculiar* to this disease; but this would be an erroneous notion, though it be far from improbable, looking to the nature of cholera, the rapidity of its progress, and the strange attitudes in which its victims perish, sometimes, that such postures are more commonly met with than in other affections. I was told by a nurse, whose task it was to lay out not a few who had died of cholera, that she was always careful to straighten the limbs very soon after the occurrence of death, because they were wont to stiffen very quickly, and she was then unable properly to 'compose' the corpse. M. Cruveilhier states that he has rarely seen those semi-flexed positions which some have delineated as proper to this disease; but this might have been owing, in part at least, to those whom he examined having died in a hospital where due care was taken to arrange the bodies in due form. This pathologist, however, gives a striking example of the disordered postures whereto he makes reference. It is related by him, that an extremely vigorous man, 25 years of age, died of cholera in *seven* hours, enduring cramps to the last. Twelve hours after death, the body was rigid, and there was flexion of the legs upon the thighs, and of the latter upon the pelvis. The conjunctivæ were dark, the body livid, the eyes shrunken, and the whole aspect singularly frightful. The question to be asked clearly is—was this body "laid out?" were the limbs straightened as usual? Another question is—did any muscular contractions occur after death, and so discompose and distort the corpse? Some trifling alteration in the position of the parts may occasionally be owing to rigor mortis, and I may refer to some observations of Sommer, which bear directly on this point, but I think any physiologist of judgment would pause very long before he attributed such an attitude as this to the effect of that phenomenon; and I should not imagine that Cruveilhier, who is silent upon the matter, would think of so explaining it. The knowledge which we have of post-mortem contractions affords a far better explanation of some changes of posture occurring after death than any which could be furnished by rigor mortis. But in reference to this topic, it is

indispensable to note most accurately the position at the moment of death, for, if left to itself, it will remain as it was. The common practice of "laying out" the dead, has, I need not say, reference to the acknowledged fact, that persons stiffen in the form they are left in. In various cases of violent death strange attitudes are found, well calculated to amaze the vulgar, and make yet deeper the horror of the scene. No one was present to arrange the body; the attitude of death remains. The drowned have been found in all kinds of postures. Lately I saw the arm retracted to the utmost, in a body stiffened firmly by rigor mortis. It was that of a paralytic, and had been so retracted before death. Rigor mortis may be held to fix positions rather than make them. At the moment of my writing this, there are two frogs before me; rigor mortis has occurred long since. In one, the limbs, disposed symmetrically, lie a little apart; the other rests in a somewhat grotesque attitude. They are, with the exception of a slight flexion of the toes of one of them, exactly as I placed them before rigor mortis was established.

For myself I have never seen a greater degree of rigor mortis after cholera, than I have noted after other affections that have run their course speedily. In strong and unusually muscular subjects who die of acute diseases, it is not singular to remark the extreme firmness wherewith the limbs are stiffened, and the clear outline of many of the superficial muscles.

The involuntary muscles have sometimes been found remarkably contracted after death by cholera. The heart, the bladder, and the intestines, have all been discovered, though anything but invariably, in a marked condition of rigor mortis.

Whether or not the involuntary muscles are, to speak *generally*, most contracted (where they are so), in those cases in which rigor mortis is specially evident in such as are voluntary, I cannot say, but it is most likely that both sets of muscles would be discovered, in some instances, similarly affected to an extreme degree.

The heart has been often seen very rigid, but the rigidity of this muscle is far from universal, and in not a few instances it is quite flaccid.

The bladder has been very commonly

found in extreme contraction. One writer has likened it to the unimpregnated uterus, on remarking it in this condition, from the thickness of its walls and the smallness of its cavity.

The intestinal tube has presented more or less general marks of rigor.

In the "Madras Reports" it is stated that the intestine was sometimes, but rarely, contracted; one case is mentioned in which the arch of the colon was in a state of rigor, and another, in which there were several contractions of the same part.

There are some examples to be found in the "Bombay Reports."

In one case there was a contraction of the colon from the cæcum to the sigmoid flexure, so that the sides of the intestine were brought into close contact, and it felt thick and solid; the part so influenced was not larger than the middle finger. In a second case, the colon is reported to have been "nearly obliterated," in consequence of contraction. In a third, the transverse arch of the same portion of the canal is spoken of as "almost impervious."

The Cholera Gazette for 1832, also contains facts of the like nature.

In a case examined by Mr. Pilcher, about twelve inches of ilium were found contracted; and instances are given of partial contraction of the colon.

In a post-mortem examination, made in the presence of Dr. Sims, Dr. Hope, Mr. Mayo, and Mr. Perry, the stomach was found "contracted."

Dr. Kirkes has kindly informed me of a case which occurred during the late epidemic, wherein, 15 hours after death, "the heart was pretty firmly contracted; the urinary bladder, and large intestines, were contracted also. The limbs were exceedingly rigid, and the muscles in the most extreme degree of contraction."

But no form of rigor mortis of the involuntary muscles is *peculiar* to cholera. Rigidity of the heart is no uncommon circumstance, as every one knows; and I have seen lately some well-marked cases of it in various affections.

It was probably partial rigor of the intestines that was referred to by Harvey, famous for his great discovery, in his "Anatomical Examination of Thomas Parr," a man remembered for having lived long. "The small intestines pre-

sented several constrictions, like rings, and were muscular."*

The following observations on rigor mortis of the involuntary muscles, to be found in the "Supplement to the Second Volume of Professor Müller's Elements of Physiology,"† may not be inappositely cited here. "The rigidity of voluntary muscles, from being the most evident, has attracted most attention, and the phenomenon has, until lately, been described solely in relation to this class of muscles; but sufficient evidence has now been accumulated to warrant the conclusion, that the involuntary muscles also are affected by a post-mortem rigidity, which is, in all essential respects, comparable with that seated in the voluntary muscles. And this is true, not merely in regard to those involuntary muscles which, such as the blood and lymphatic hearts, are constructed of striped fibres, but also with regard to the tissues composed of unstriped fibres, such as the contractile coat of blood-vessels, and of the large excretory ducts. The observations of Dr. George Budd and Mr. Paget have proved this in the case of the heart; and the occurrence of rigidity in the digestive canal has been shown by Valentin, who found that if a graduated tube be connected with a portion of intestine taken from a recently slain animal, filled with water and tied at the opposite end, the water will in a few hours rise to a considerable height in the tube, owing to the contraction of the intestinal walls. The contraction of the blood vessels after death was observed by John Hunter, and is now regarded as a well-established fact, and one by which the empty state of the arterial system after death is in a great measure explained."

In considering the *duration* of rigidity, be it in reference to cholera or not, regard must always be had to *temperature*. It is related by Nysten that Laennec shewed him a squirrel which in a cold season was rigid seven days after death.

In a paper, which I wrote, not long ago, on the muscular contractions which occasionally happen after death from cholera, I observed that in one respect (if not in more than one) they may be

* See Works of W. Harvey, M.D., Edition of the Sydenham Society, v. 596.

† By William Babt, M.D., F.R.S., and William Seabrook Kirkes, M.D.

likened to the effects of rigor mortis which, as may sometimes be observed of these contractions, affects parts in *succession*. This remark is far too general, and may be thought to imply more than I intended. Rigor mortis and the movements in question, have, as will be apparent to every body, many and extreme points of difference. Rigor mortis does not ensue until the irritability of the muscle is entirely or almost exhausted; but these movements are signs of irritability, sometimes of a great degree of it, are, in some cases at least, to be *excited*, whereas nothing can excite this form of rigidity, but its one common cause. Rigor mortis influences both sides of the body equally, producing effects not less symmetrical than the disposition of the muscles themselves, and repeats itself in various subjects, making an allowance for occasional differences, with a striking sameness; but these contractions are diversified as to form and power, affect different muscles in different subjects, cease and return repeatedly; whereas the contraction of rigor mortis being once over, is never renewed. But why proceed further?

The more we contemplate the rigidity of death, the more remarkable does its individuality appear. No other form of muscular contraction can properly be compared with it; no other so comes, so progresses, so endures, so departs. Yet such rigidity, though it be caused by a special action, which is in nowise to be explained as other kinds of action are, is quite within the pale of the admitted law, that the force of an action, no matter what its nature, is in dependence on the condition of the part performing it. Nor can it be necessary to enforce the observation by repeating at length what has been said already, as to the relation between the power and endurance of rigidity and the perfect nutrition of the muscular fibre.

Two points yet remain to be considered in reference to the case which occurred at Bristol; one relates to the retardation of putrefaction which was observed. This doubtless considerably added to the doubts of the relatives of the deceased. Putrefaction is, of course, always watched for with intense anxiety by those who are unassured as to the reality of death. Five days, in this case, passed away ere putrefaction showed itself. The time was long, notwithstanding death by an affection wherein

this process tarries. But instances might be cited of its far more protracted appearance, though actual death had happened—not death-trance, a name used sometimes most improperly where dissolution was absolute, because things had happened not to take their accustomed course in the interval between the cessation of the respiration and the beginning of decay. A case occurred at Deptford, in 1844, in which a body, dead in reality, was supposed by some to be in a death-trance. It was watched long, and opinion varied as to its true condition. Putrefaction had not “far advanced” when so many as thirty-five days had elapsed after dissolution. The deceased was *young*, and had died *suddenly*. In the case at Bristol the circumstances were such as to make the late putrefaction no such very especial matter for surprise. The death happened in a *young* subject, was accomplished with *celerity*, and effected by a disease wherein the dead are slow to putrefy. The temperature was not high, and fresh currents of cool air were from time to time admitted into the room the body lay in. We must not construct marvels, nor magnify everything which may somewhat deviate from the course of things.

Let me here add one word further on the relation of putrefaction to the temperature of the dead. Prolonged heat and extremely tardy putrefaction have been observed *together*: but has the body been seen at the *same time* quick to putrefy and slow to cool? Dr. Taylor not long ago published in the *MEDICAL GAZETTE* an instructive “Case of Rapid Decomposition of the Human Body.” I inquired of him how quickly it parted with its warmth, and he replied:—“Unfortunately no trustworthy history could be obtained of the cooling of the body, as it was in the hands of the nurse during the night and early part of the morning. It was quite cool when I saw it *seventeen* hours after death.” It is observed by M. Deschamps, that “so long as the body preserves its natural heat there is no discoloration of the abdomen.”* It is to be observed of the case for which I am indebted to Dr. Green, that the loss of heat, the occurrence of rigidity, and the

* He remarks, also, that the discoloration in question is very often coincident with cadaverous rigidity.

abdominal putrefaction, were nearly *coincident* marks of phenomena.

Dr. Taylor has been good enough to send me the following note of some experiments which he has been performing in reference to the question of animal putrefaction producing heat:—"I have tried experiments by inserting delicate thermometers in the midst of putrefying animal matter, but have not found the least change of temperature during the process, although the viscera were kept in glass vessels covered."* There was a thermometer outside for comparison.

It need scarcely be added, that the tendency to putrefaction is much greater after some diseases than others. The remark holds good of affections which agree in leaving their principal traces in the same region. In cholera, as we have seen, it is later than common; but in child-bed fever it is often, as Dr. Kirkland and others have noticed, quick to appear. The same may be said of ordinary enteritis. Mr. Phillips observes of a case of intestinal obstruction:—"Although the death occurred only thirty-four hours before the examination was made, and although only sixty hours before he was apparently in good health, decomposition was greatly advanced.

"The abdomen was enormously distended; the scrotum was as large as a child's head; the surface of the trunk was covered by very large vesicles containing a dark-coloured and very offensive fluid; and a good deal of dark bloody fluid had escaped from the mouth and nose. The cellular tissue was so distended with gas, that as soon as an excision was made through the skin it escaped with a loud hissing noise."†

The aspect of the countenance in the Bristol case was described as placid, like that of one asleep. It probably would have excited less attention had it not been associated with retained warmth, deferred putrefaction, and delayed rigidity. It was certainly, considering the mode of death, an unusual aspect; for cholera leaves, generally, rude and deep marks of its swift, desolating course. The dead look much as the dying: there is death in the visage

some time ere it comes, and the features at the moment of the last expiration seem hardly to undergo the shadow of a change. How different when some face, crimson with health perchance, turns deadly pale from sudden, fatal hæmorrhage, the blood flying from the cheeks scarcely more swiftly than life from the body!*

It must not be thought that the countenance has *always* a striking haggardness after death by cholera. Not only was there none in the example before us, but Cruveilhier mentions an instance in which a person who died of this disease might have been supposed to have perished from some other malady. But, generally speaking, the eyes are unusually sunken, and there is a peculiarly shrunk and ghastly countenance. Even the faces of children manifest it, though in them there is so frequently observed, after other maladies, and before the time of rigidity, a most serene, nay, to some a momentarily delusive, physiognomy.

A change has been seen in the colour of the face, which becomes less livid, but *gradually always*, as I should suppose. It sometimes puts on quite a red tint, which, like the less conspicuous change, is doubtless owing to alterations in the blood of the superficial vessels. As to the general hue, it varies much: in a case recorded by Cruveilhier the superior extremities were so livid that they at first sight seemed gangrenous to a medical observer; but in some instances the lividity is infinitely fainter, and might pass without any particular remarks.

Let us, before concluding, pass in short review some of the principal points which have been noticed by observers as to the state of bodies dead from cholera.

I. The aspect has been unusually cadaverous, though scarcely more so, in many cases, than it was in life-time, for the disease has power to make all physiognomies subordinate to its own: but the features have more than once

* A beautiful allusion, as some readers will remember, is made to the remarkable paleness which overspreads the features when death is owing to a sudden loss of blood, at the end of the tenth book of the *Æneid*. The following lines are from the touching description of the death of Lausus:—

"At verò nit vultum vidit morientis et ora,
Ora modis Anchisiades pallentia miris:
Ingemuit miserans graviter, dextramque
tetendit."

* Temp. 55° to 57°.

† In this case a portion of the ilium was strangulated, another part of the intestine looked gangrenous, and there was some dark fluid blood in the abdomen. See *Medico-Chirurgical Transactions*.

been observed in a different state, in nowise talling of the cause of mortality.

II. The whole body has looked extremely shrunken; the hands have been very shrivelled; the distinction between the ages of corpses has not been nearly so easy as in general.

III. The colour has been dusky, often remarkably livid, and has been noticed to become lighter, and even to redden; but the hue, not changing equally, has sometimes presented a peculiar mottled appearance.

IV. Rigor mortis has set in, occasionally, with an unusual hurry; it has also been deferred extraordinarily late; has occurred when the limbs have been more or less flexed, and so fixed them; has been seen in every degree; has been noticed to influence the involuntary muscles, the bladder very frequently, the heart not rarely, the intestines occasionally. It has presented all those varieties which might have been anticipated, seeing that the victims of the disease have been furnished by persons of every age, strength, and condition.

V. Contractions have taken place in the voluntary muscles, and been excited by percussion: they have been partial, weak, transient, or more general, stronger and long-enduring; they have varied considerably as to time of occurrence, progress, appearance, and effect, and have, for the most part, been observed in males, the most marked instances having been furnished by very muscular subjects who perished rapidly.

VI. The case which occurred at Bristol exemplifies, amongst other things, an unusually long maintenance of temperature; and there seems no doubt of many examples having happened in which the heat of the body rose considerably after death.

VII. Putrefaction was generally more delayed than usual, and has been noticed to be especially tardy. I believe it may be stated that none of the foregoing points are, strictly speaking, *peculiar* to cholera; though there was much that was indicative of the nature of the affection in the general appearance, whereof words, after all, can but give an imperfect history. The lividity, the change of colour, the varieties of rigor mortis, the lingering temperature, the rise of the same, and the muscular contractions, have all been observed in other cases than those of cholera.

I confess, when I began the considera-

tion of the subject, that there was much in the condition of the body after cholera which, in my eyes, stood by itself. I speak not, of course, of the state of the intestines,—of what may be termed the morbid anatomy of the affection, in a peculiar sense, but of the phenomena which may be observed in the skin and muscles, the state of the temperature, and the event of contractions. But it must be admitted that we know next to nothing, from observations made at home, as to these two questions, if we except what cholera has taught us; and we cannot too moderately estimate such information as we have gained therefrom. The inquiry into the whole state of the dead, not in cholera alone, but other maladies, between the last respiration and incipient putrefaction, is in absolute infancy.* It needs and invites labourers. It would prove not of interest merely, but of *use*. It is called for by physiology, and especially demanded by forensic medicine; nor can medicine in general afford to forego it, since the *history* of some diseases cannot be truly called complete without it. Of course, many will ask *cui bono*—words heard commonly even in this advanced age.

As with the phenomena of cholera during life, so with others which may be witnessed after dissolution,—unless they be viewed *comparatively*, there is no chance, no hope, of appreciating them aright. We view subjects narrowly, and therefore obscurely, at first sight: not until we dwell upon them patiently, thoughtfully, do they reveal their features, and show forth their analogies. It is plain, even from the foregoing, that we ought to be well familiar with the diverse states of the body after death. In ignorance thereof we have no chance of checking absurd terrors, and of giving sound and reputable evidence on some matters of legal investigation. Novelties are ever exciting, ever popular, and such as relate to the dead, inasmuch as they more or less touch all of us, are ever likely to remain so, in a peculiar sense. Exceptional conditions will be sure to strike every one. Everybody will speculate on muscular contractions, talk of rise of temperature, and retarded putrefaction: a few will be apt to converse of “death

* I may here refer the reader to some of the questions discussed by Dr. Davy in his ingenious researches.

trances," and to suggest, on the strength of the wildest rumours, the holding of inquests. But why this occasional, ill-founded amazement? Is it, seeing the unutterable complexities of life, at all likely that the last breath should always leave the body in the same state? Perhaps we ought more to wonder than now we do that the condition of corpses is, on the whole, so uniform. I say not that this thought will weigh anything with those who have never spent ten minutes in reflecting upon life, but yet presume to speak of the vital functions as if it were the very easiest of all matters to understand them in health, and right them in disease. They expect a specific for every malady, as a matter of course; and, instead of being thankful for what medicine has accomplished, they are full of disappointment that it has done no more. How can they, who are ignorant of the phenomena of the living, judge truly of the condition of the dead? Is not the study of life the only means whereby the nature of death can be comprehended? Amongst the things least to be feared in this country, premature burial may be named. There were rumours afloat during the past epidemic which might seem to imply some danger of it; but where was there a solitary instance which could bear inquiry? We hear of marvels in the *distance*, and begin to puzzle ourselves; but everything would be different if we were *near*. The phenomena of the case at Bristol were not the same to the medical attendants of the deceased as they were to those who knew them by report alone. I could not help observing, when the cholera was raging, that it was more dreadful to some persons to read about than to others to see. They who were in the midst of it, trying to give help, were less full of dread and discomfort than many who were safely breathing, in pleasant places, the purest air. They had no time, at any rate, for needless terrors,—no time to sit shivering at mere fictions. There are some remarks in De Foe's "History of the Plague" which apply well to persons who take fright at nothing. Speaking of some strange rumours, he says: "But these stories had two marks of suspicion that always attended them, which caused me always to slight them, and look upon them as mere stories that people continually frightened one another with. First, that wherever it

was that we heard it, they always placed the scene at the farther end of the town, opposite, or most remote from where you were to hear it: if you heard it at Whitechapel, it had happened at St. Giles's, or at Westminster, or Holborn, or that end of the town; if you heard of it at that end of the town, then it was done in Whitechapel, or the Minories, or about Cripplegate parish: if you heard of it in the City, why then it happened in Southwark; and if you heard of it in Southwark, then it was done in the City, and the like." Inquiry should ever precede belief; but it is a pretty general habit to make belief precede inquiry, and so all sorts of delusions are cherished and abound. Mystery has innumerable worshippers. Would that truth had so many! Men will wander in the world of shadows: there abuse they and exhaust their energies. But students of Nature must be severe historians, not tellers of dreams; their object is not to mystify, but to make clear—not to invent, but to discover; and they may be naturally inclined to observe—"And surely you will easily believe that we, that have so many things truly natural, which induce admiration, could in a world of particulars deceive the senses, if we would disguise those things, and labour to make them more miraculous. But we do hate all impostures and lies, inasmuch as we have severely forbidden it to all our fellows, under pain of ignominy and fines, that they do not show any natural work or thing adorned or swelling, but only pure, as it is, and without all affectation of strangeness."*

STATISTICS OF UTERINE DISEASE.

M. HUGUIER gives the following statistics of 2527 patients under his care:—131 suffered from uterine congestion. Of these 8 had congestion of the body and neck of the uterus; 13 congestion of the body alone; 106 of the neck alone; and in 4 it was not determined which was the seat of disease.

According to the tissue affected, M. Huguier classifies these cases as 1, complex; 2, fungoid; 3, cedematous; 4, varicose. As influenced by causes—1, essential; 2, syphilitic, 3, dartreux; 4, diphtheritic.—*L'Union Médicale.* X

* New Atlantis.

TWO CASES OF
FATAL OBSTRUCTION OF THE
INTESTINAL CANAL, BY PE-
RITONEAL BANDS.

(Read before the Abernethian Society, on
Thursday, February 14th.

By C. R. THOMPSON,
Late Honorary Secretary to the Society.

MECHANICAL occlusion of some part of the intestinal canal, by intra-abdominal causes, has lately attracted great attention among English surgeons; chiefly with regard to the propriety of performing gastrotomy, in the hope of finding and relieving the stricture.

Before any just conclusions can be arrived at on this point, more extended experience seems necessary, especially to determine whether we can predict with certainty the seat or nature of the constriction, during life: and whether the constriction is generally of such a nature that it might be relieved by an operation. This experience is only to be gained by the careful record of cases; and the exact report of the post-mortem appearances in such as terminate fatally. With this view, the two following cases, which have lately occurred in St. Bartholomew's Hospital, are offered to the Society.

The first is extracted from the registry of cases occurring in the Hospital, where it is reported by Mr. H. S. Webb; and the post-mortem appearances by Dr. W. S. Kirkes.

Case I.—Mary G——; a tall, dark-complexioned woman, aged 20, was admitted into Faith ward, November 13th, 1848.

Present condition.—Face dusky; eyes sunken, with livid areolæ around them; skin warm and dry; pulse 120, small, and rather sharp; lips dry; gums rather vascular; slight sordes about the teeth; tongue moist, coated on dorsum with very thick brown fur; clean at the edges. Bowels have not been open for seven days. Urine is said to be scanty, high-coloured, and depositing a thick sediment. Catamenia regular.

Complains of slight vertigo, and of nausea when she lies on the back. The abdomen is distended; generally dull on percussion; intolerant of pressure, especially in the umbilical region.

History.—Is a single woman, in domestic service; has always had good health until rather more than a fortnight ago, when she got wet feet, which checked the catamenial discharge. The next day she became very sick, and vomited whatever food she took, complaining also of a very severe gripping pain in the umbilical region. She suffered rigors, followed by great heat of the skin. She kept about the house for four days, although unable to attend to more than a part of her usual duties. Two days after the seizure she was seen by a chemist, who gave her some medicine, which was vomited. She was seen frequently by this chemist, but always vomited the medicine, as well as any food she took. Ten days ago she left her place, and saw a medical man, who gave her medicine which relieved her symptoms, and she got better, until a week ago, when she went out walking. On her return she went to bed, and during the night the pain in the abdomen and vomiting came on again, and with greater severity than before. She was bled, and has had leeches and fomentations applied to the abdomen during the past week, with the effect of relieving the pain. She has taken frequently aperient medicines, and enemata have been administered seven or eight times, without the desired effect.

Ordered.—Venesection to 12 oz. A dozen of leeches to the abdomen, to be followed by a poultice; a soap injection *per rectum*; to take calomel powder, ten grains, directly; and effervescing draughts, with one drachm of sulphate of magnesia, every sixth hour.

14th.—The bleeding was well borne; the blood not buffed; the pain seemed much relieved by the leeches; the powder was retained on the stomach; two soap clysters brought away no fecal matter. She passed a very restless, sleepless night; has vomited frequently a fluid of light yellow colour, and stercoraceous character; the expression is very anxious; features more sunken. Pulse 120, softer than yesterday. Abdomen very tense, and intolerant of pressure. Does not complain of actual pain, but of general discomfort.

Ordered.—Continue draughts; take of calomel three grains every sixth hour; repeat enemata; fomentations to abdomen.

15th.—Symptoms continue the same;

all nourishment given by the mouth is quickly returned; stercoraceous vomiting is unabated; complains of a sensation "as if something alive were moving" in her abdomen. The abdomen appears more distended, and particularly prominent to the left of the umbilicus; generally dull on percussion.

Ordered.—Continue draughts, with ten drops of laudanum added to each; continue pills. Brandy, two oz. Beef-tea, two pints, for two injections.

16th.—The first beef-tea enema was quickly returned, and followed in four hours by about a pint of dark bilious-looking fluid, containing abundant sediment of faecal odour and appearance: the second enema was returned unchanged. The vomiting and extreme restlessness continue. Tongue begins to dry. Pulse frequent, small, and jerking. Abdomen slightly resonant on percussion laterally; firm pressure on the abdomen causes nausea, but no pain. Takes arrow-root and brandy willingly.

Ordered.—Continue medicines.

17th.—She remains in the same condition; complaining more of abdominal pain. Continue medicines.

18th.—She is delirious and very restless; face very dusky and flushed; pupils contracted; skin hot and dry; pulse 124, small, and very feeble; vomiting of feculent matter continues; the enemata have brought away nothing; the abdomen is more tense, and intolerant of pressure.

She continued in this state until the evening, when she sank rapidly, and died at 7 P.M.

Post-mortem appearances 65 hours after death.—Limbs still slightly rigid; abdominal parietes discoloured from decomposition.

Brain not examined.

Lungs, with the exception of some emphysema, healthy. About half an ounce of clear fluid in the pericardium. Heart rather large; left ventricle contracted; not examined internally.

Peritoneum generally rather vascular; about four ounces of slightly turbid fluid containing flakes of lymph in the sac; some deposits of puriform matter at the bottom of the pelvis. Small intestine greatly distended; the peritoneal coat vascular and thickened; a broad line of redness ran along the border of the intestine where the coils lay in contact

with each other; a few slight shreds of lymph, here and there, held the coils slightly together at their surfaces of contact. Large intestine pale, contracted, and nearly empty. The transverse portion of the colon drawn down with the *right iliac region*, by a short narrow band proceeding from its lower border, and attached to the spine by means of the portion of mesentery corresponding to the last part of the small intestine. This band passed in front of the small intestine about an inch before its termination in the cæcum, compressing it against the spine, and constricting it so as to render it impervious. The portion of the colon from which the band originated was closely and rather firmly adherent to the constricted part of the small intestine, the thinner and more friable bands passing from one intestine to the other. The mesentery immediately surrounding the constricted part was inflamed, thickened, and matted around the intestine.

The entire length of the small intestine, as far as the constriction, was filled with liquid bilious matter, such as had been vomited during life. The mucous membrane was reddened and swollen in the neighbourhood of most of Peyer's patches, the individual glandulæ of which had burst, leaving spaces surrounded by a thickened border of mucous membrane. Each Peyer's patch thus presented a broad oval surface, the mucous membrane of which appeared worm-eaten, and as if irregularly removed by ulceration.

Here and there a swollen red solitary gland was seen. As the small intestine approached the seat of constriction, it became very vascular, and presented discoloured streaks and spots, as if from commencing sloughs. The piece of small intestine between the constriction and the cæcum was pale and contracted, and its mucous membrane irregularly ulcerated. Stomach distended with liquid bilious contents. Liver natural; gall-bladder distended with a quantity of thin dark bile; spleen natural; no morbid appearance of uterus or ovaries; kidneys large, heavy, and congested, but apparently of healthy structure.

CASE II.—Hannah R—, æt. 23, was admitted into Lucas ward, in the morning of February 7th, 1850.

Present condition.—Face very dusky and flushed; eyes dull, sunken, and surrounded by dark livid areolæ; skin

cold, moist, and clammy; pulse 140-50, very small and thready, at times barely perceptible; lips dry and fissured; tongue dry, coated with thick dirty white fur; bowels last open four days ago, but scantily; respiration natural, about 25.

She complains of pain from the most gentle pressure on any part of the abdomen, but especially in the hypogastric and iliac regions; the abdomen does not appear distended, and is generally soft, except about the umbilical region, where it is rather tense; there exists tympanitic resonance on percussion over the course of the colon, and in the epigastrium, but marked dulness in the umbilical and hypogastric regions. The patient is very restless; appears to lie indifferently on the back or either side, keeping the knees slightly drawn up. Complains of pain about the hypogastrium, and of constant nausea; vomits occasionally a light-coloured frothy fluid, having an appearance like working yeast, and of a decidedly feculent odour.

History.—She is a delicate married woman, a toy-painter by trade, which occupation she has pursued for years. Has been confined four times, and has two living children. The last labour occurred about five months ago, and was premature, at the seventh month of utero-gestation. Since that time she has never been perfectly well, suffering frequently from troublesome constipation, for which she has been under medical care; during the last three months particularly, the bowels have been generally costive, and she says the motions have been of unusually small calibre. The bowels were last open, after a constipation of three days' duration, on Sunday—i. e., four days ago, and then but very imperfectly. On Monday she complained of great pain in the abdomen, and stercoraceous vomiting commenced, as was learnt by a communication from her medical attendant. Since that day she has taken various aperient medicines; but the constipation, stercoraceous vomiting, and abdominal pain, continue unabated. Several enemata have been administered, and returned unchanged. The patient says she passed urine last night: catamenia regular.

A catheter was passed into the bladder, and about two ounces of thick, high-coloured urine drawn off. An elastic tube was introduced for some eight or ten inches by the rectum, and copious

enemata administered. It was determined in consultation that no operative proceeding was justifiable; and the patient was ordered wine, brandy, and beef-tea *ad libitum*, and to have large enemata.

Feb. 8th, 1 P.M.—She appears almost comatose, and fast dying; since 11 P.M. last night, she has taken four half-drachm doses of laudanum, to quiet the pain and restlessness, and has swallowed brandy and wine pretty freely; has vomited occasionally; abdomen more full, but soft; enemata came away unchanged: died at 4 P.M.

Post mortem appearances about 20 hours after death.

Thorax.—Heart and pericardium healthy; pleurae free from adhesion. Left lung healthy; infiltrated posteriorly with some bloody serum, but crepitant throughout, and floating in water. Right lung had lost its natural contractility, and presented a good example of extreme emphysema throughout its whole substance.

Abdomen.—On exposing the peritoneal cavity an extremely fetid odour was perceptible. The parietal layer of peritoneum was dotted with numerous small circular milk-white spots, more distinctly seen after exposure to the air; these spots were in the substance of the serous membrane, and not removable except by carefully dissecting it from the subjacent cellular tissue.

The great omentum was gorged with dark blood, and looked, at its lower part, like a bag of large distended veins. On raising it, it was found to be adherent to the subjacent intestines by recent lymph; on separating these adhesions carefully, there appeared an aperture in a mesenteric vessel of considerable size, freely giving exit to blood, of which about half a pint had escaped into the pelvis, probably during life. The contiguous surfaces of the intestines, both large and small, were also agglutinated together, and to the parietes of the abdomen, by recent lymph, which was easily broken down by the handle of a knife.

The greater part of the small intestine was of a deep red colour, very much distended in the upper part, and contracted below; about two feet of its length appeared more distended, and of a darker colour, than the rest of the intestine, almost black, or dark claret-

coloured: in the middle of this dark part was an ash-coloured slough, about three inches long, with well defined margin: the extremities of this congested fold of intestine were tightly girt by a firm band passing from the surface of another portion of intestine of the mesentery at the point of constriction: this band was very strong, and firmly attached at both its extremities.

The contents of the strangulated portion of intestine gave to the finger the sensation of hardened faecal matter, which was found to be caused, however, by the congestion and enlargement of the *valvulae conniventes*; for, on opening the canal, it contained only about one pint of dark fluid blood: the mucous membrane presented even a darker aspect than the exterior, and the internal surface of the ash-coloured spot was scarcely to be distinguished from the adjacent membrane: the mucous membrane was easily stripped off in various points. A considerable quantity of blood was also contained in the intestine, for a short distance above and below the stricture. The seat of constriction corresponded nearly to the umbilicus; but the coil of included intestine reached to the right lumbar region. It was a part of the lower end of the jejunum. Stomach healthy; also the large intestine, which contained near the caecal valve a few hardened scybala.

Pancreas and spleen of normal size and aspect.

Liver showed some portal venous congestion: otherwise healthy.

Kidneys rather large, but apparently normal.

These cases tend to confirm Rokitsky's opinion, that internal constrictions of the intestine are more frequent in females than in males; which, indeed, we should anticipate, on account of the great changes of position to which the abdominal viscera are subject in the former sex, in pregnancy, ovarian tumors, &c.

The same author has found abnormal peritoneal bands, or rents in the mesentery, to be the most frequent cause of internal incarceration. These bands are probably in most instances the result of some previous inflammation of the peritoneum; in many recorded cases we find a distinct history of mesenteric mischief; in the case related by Mr. Hilton, in *Med. Chir. Transactions*, vol. xxx., the patient, 20 years of age, was

the subject of mesenteric disease in early childhood, and, some time subsequently, suffered from symptoms which indicated peritonitis; here, from the history, the nature of the obstruction was accurately diagnosed, and found after death to be a band of false membrane, including a knuckle of the ileum. Mr. Druitt also records an instance where a boy, eleven years old, died after the operation of gastrotomy for the relief of the intestinal stricture; and old adhesions were found in the peritoneal sac, one of which was the cause of the constriction: the boy was stated to have suffered for eight years with occasional severe attacks of pain in the abdomen, subsiding under the use of medicine.

In the two cases which I have related, there was no evidence of former peritoneal mischief; indeed, from patients of this class it is always difficult to get a clear account of their ailments: it is curious that in both instances it was carefully observed that the abnormal band forming the constriction was the only one discovered of any strength or consistence, which seems to disprove the inflammatory origin of the bands, and raise a suspicion of their being congenital. Rokitsky mentions the frequent congenital occurrence of unusual length of the duplicatures of the peritoneum, or of supernumerary folds and pouches of that membrane; these pouches are chiefly found, he says, in the hypogastric, and more especially in the iliac and inguinal regions, and about the fundus vesicae; and communicate with the peritoneal cavity by a well-defined fissure or ring, which is frequently surrounded by a tendinous band, lying in the duplicature.

ON THE CONSTITUENTS OF RHUBARB.

M. GAROT gives the following as the results of his researches on the composition of rhubarb:—

1. That by treating rhubarb with nitric acid a peculiar acid is obtained, in the proportion of 8 to 10 per cent. in indigenous, and 15 to 20 per cent. in foreign rhubarb.

2. To this acid he gives the name of *erythroce*: it is soluble in alcohol and ether.

3. It combines with alkalies, and forms compounds which may be employed in the arts and in pharmacy.

4. The colouring property is greater in foreign than in indigenous rhubarb. — *Journal de Chimie Médicale*.

ON THE
OCCURRENCE OF A MUCO-PURU-
LENT DISCHARGE FROM THE
VAGINA IN SCARLATINA,
AND THE IMPORTANCE OF THIS SYMPTOM
IN RELATION TO FORENSIC
MEDICINE.

By ROBERT BARNES, M.D. LOND.
Lecturer on Obstetrics, Obstetric Surgeon to
the Western General Dispensary.

In the summer of 1843 I witnessed, in the wards of M. Chomel at the Hôtel Dieu, a case of scarlatina in a girl about 16 years of age. Owing to an unfortunate accident, by which I lost the greater part of the notes I made in the Paris hospitals, I cannot give a detailed account of this patient. She presented one symptom which, on account of its novelty, and its important relations to forensic medicine, I think is particularly interesting. After the decline of the eruptive stage, and when apparently convalescent, there occurred a muco-purulent discharge from the vagina, similar in character to the discharge which not uncommonly flows from the nares as one of the sequelæ of scarlatina.

There was evidence to show that this discharge did not exist at the time of the girl's admission, and that it was first observed at the period I have mentioned,—viz., after the decline of the eruption, when she had been some days in the hospital.

I believe that the discharge was not blennorrhagic, but a newly-observed feature in the pathology of scarlatina.

It is in the suspicion of blennorrhagia that lies the interest of this symptom in relation to forensic medicine. In this case, for example, an unmerited stigma might attach to the girl's character were it not admitted that the discharge might be the result of scarlatinal vaginitis. Numerous other charges of a grave nature might be founded upon such an appearance. Accusations of rape even might receive corroborative proof from its presence. About two years ago I saw a child of eleven years old who was recovering from scarlatina. It was then observed for the first time that she had a discharge from the vagina, and suspicions were immediately excited that she had been abused by a lad in the neighbourhood. This discharge might have been a

sequela of scarlatina. I believe it was.

The importance, then, of determining the possibility of a muco-purulent discharge as a consequence of scarlatinal vaginitis, is obvious. I am not acquainted with any author who notices this symptom. Dr. Tweedie has informed me that his attention has never been directed to such an occurrence, and that when a purulent discharge from the vagina has been noticed in scarlatinal patients he has concluded it to be blennorrhagic.

My colleague, Dr. Miller, whose elaborate work on scarlatina has thrown so much light on the pathology of the disease, has favoured me with the following interesting remarks:—

"I have never seen vaginitis during the eruptive period of scarlatina, nor even among the sequelæ. I am inclined to think that it is an uncommon occurrence. Still, during a smart attack of the eruptive fever, when the mouth, nares, and pharynx are implicated, it seems not unlikely that the inflammatory action should affect the vagina, there being a less abrupt merging of skin into mucous membrane (as in the mouth) than in other mucous outlets. Yet we know that the mouth, nares, and fauces, are specially implicated in this exanthem; and, were the other local affection more common, I think our attention would be drawn to it more frequently. In our dispensary patients, especially among strumous children, muco-purulent discharges from the vagina (slight vaginitis) are common; and I have never had reason to suspect gonorrhœa, though it has been alleged. It is essentially a scrofulous disease of the mucous surface (me judice), and occurs in connection with ascarides, accumulation in the large intestines, as a sympathetic affection, just as otorrhœa occurs during dentition in these subjects.

"Recurring to the vaginitis in scarlatina, I should rather expect it to occur where there had been a very full eruption and good peeling than in any other case. Did not the affection exist before on careful examination of the child's friends, we might safely set it down to scarlatina. But I cannot deem it a specific mark of the disorder, as the mouth, nares, and throat affection, will alone mark the disease without the cutaneous eruption."

I am quite prepared to accede to the opinion of Dr. Tweedie and Dr. Miller, that a muco-purulent discharge from the vagina in scarlatina is a *very rare occurrence*; but this very circumstance increases the importance of the symptom in relation to questions of legal medicine. The more rare the symptom the less likely is it to be recognised as an effect of scarlatina, and the more probable that it will be set down as a proof of contamination.

I will not dwell too much on the two observations I have cited, as I cannot expect they will carry the same conviction to others which they do to myself. I will therefore beg permission to refer to such points in the pathology of scarlatina as seem to give support to the view I have taken of these cases.

1. The inflammatory action in scarlatina attacks a wide extent of mucous membrane as well as skin. It invades a large portion at least of the alimentary tract: the mouth, fauces, pharynx, œsophagus, and, doubtless, sometimes the stomach and small intestines, are affected. The rectum and anus are commonly involved.

The respiratory mucous tract is not less extensively implicated. The nares, larynx, trachea, and even the bronchi, have been repeatedly observed to be inflamed.

The urinary mucous tract is equally affected: the bladder, the ureters, the pelvis of the kidneys, and even the tubuli uriniferi, are involved.

The only mucous tract remaining is the genital. Analogy forcibly points to the inference that this will not be exempted from an inflammation which invades every other mucous tract in the body. I see no reason why the vagina (and also the urethra of both male and female) should not be as frequently and as essentially inflamed as the rest of the mucous membranes.

2. *The discharge.*—A common sequela of scarlatina is an inflammatory thickening of the Schneiderian membrane, accompanied by profuse secretion of an acrid muco purulent matter.

Even before the febrile symptoms are abated, a similar secretion, but generally of a more viscid mucous character, is abundantly thrown out by the mucous membrane of the throat.

A viscid secretion, often containing flakes of lymph, is also frequently coughed up from the trachea. In short,

the obstruction of the throat, mouth and nares, by muco-purulent secretions, is one of the most distressing and alarming symptoms of scarlatina.

Scarlatinal inflammation, then, of portions of the alimentary and respiratory mucous tracts is attended by morbid products similar in character to those attending severe inflammation arising from any other cause.

Ordinary inflammation of the vagina and os and cervix uteri is most commonly productive of mucous or muco-purulent discharges. It is, then, reasonable to expect that scarlatinal vaginitis should produce similar results.

I further believe that a muco-purulent discharge from the vagina may also arise in connection with small-pox, and other exanthematous diseases which affect large tracts of mucous membrane as well as skin.

Such a discharge arising from scarlatina or small-pox may frequently be overlooked; or, when detected, be erroneously attributed to blennorrhagic infection.

I have another observation to make in reference to this subject. Like the discharge from the nares, the vaginal discharge may persist for a lengthened period after convalescence from the fever. It may constitute a new form of leucorrhœa, the true origin of which may never be suspected. In such a case the treatment would be simple. I have found nothing succeed so readily in inducing a healthy action of the Schneiderian membrane, when affected by scarlatina, as the application of nitrate of silver; and I have no doubt that a few injections of a solution of nitrate of silver into the vagina, so as to reach the os and cervix uteri, would effect a cure.

In conclusion, I freely admit that the occasional occurrence of a muco-purulent discharge from the vagina, as a consequence of scarlatina, has yet to be established. Observation alone can decide this.

Extensive inquiry will soon determine the existence of this affection, and the relative frequency of its occurrence. These points established, the practitioner may often experience the grateful opportunity of dispelling unjust imputations against female virtue, and of defeating false accusations of criminal violence.

Gloucester Terrace, Hyde Park, June 26, 1850.

MEDICAL GAZETTE.

FRIDAY, JULY 12, 1850.

Our readers will find, in another part of this number, the first, and we believe the only, instalment on medical legislation likely to be obtained during the present session of Parliament.*

We have great pleasure in giving publicity to this "Act for better regulating the Privileges of the Faculty of Physicians and Surgeons of Glasgow, and amending their Charter of Incorporation," for it breathes throughout a liberal spirit,—using this term in its legitimate, and not in its perverted sense. Under the third clause, the Fellows, Members, or Licentiates of any other Corporation or Royal College may practise in the Glasgow district, and enjoy the same privileges as the Fellows and Licentiates of the Faculty of Physicians and Surgeons of Glasgow. The conduct of the Glasgow Faculty in thus conceding a reasonable privilege, is strikingly contrasted with the illiberality of some other Scotch Corporations and Colleges. The London Apothecaries' Society have been strongly condemned for insisting on their privileges by prosecuting Scotch graduates who have practised in England and Wales without a license; but the following extract from a letter to a contemporary journal will show that illiberality, by which we are here to understand a very rigorous enforcement of questionable privileges, is not confined to the Corporations of England and Wales. If the Edinburgh M.D.s are prosecuted in England, the English Members and Licentiates are persecuted in Scotland. The writer of the letter states, that to his knowledge the Edinburgh University

"would not admit to examination even a member of the College of Surgeons of England, and licentiate of the Apothecaries' Company, and a student of three years' standing at Guy's Hospital, the largest medical school in England, who had also taken out six classes in the University of Edinburgh, and the hospital practice of the Infirmary for a year, and thus possessed certificates of attendance on lectures trebling those required by the senatus of the University. The London students of the same year as myself, who had certificates of lectures delivered at University College and King's College, London, were admitted to examination with only one year's attendance in Edinburgh, whilst the unfortunate Guy's man was thus driven to a northern University for his degree of M.D., unless he chose to stay in Edinburgh two years more. The Apothecaries' Company, in the same year, I know, admitted a friend of mine to examination solely on production of his Edinburgh diploma. I applied to the College of Surgeons of *Edinburgh* to admit me to examination on producing my diploma of the College of Surgeons of *England*. I was refused to be admitted to examination unless I possessed certificates of lectures on all the classes required by the Edinburgh College. Surely this is hardly an act of courtesy, when the London College admits licentiates of the Scotch College to examination solely on production of the Scotch license."

The statement of the writer shows, we think, not merely a want of courtesy on the part of the Edinburgh University and College, but a great want of liberality.

On what principle they draw a distinction between medical students educated at Guy's and St. Bartholomew's, and those educated at University and King's Colleges, we are at a loss to understand. There is no difference in the amount of education imparted, and, as fields of practice, the two great hospital schools, reckoning 1200 beds between them, are superior. This distinction has certainly neither justice nor liberality in its favour.

The Edinburgh Corporations have now a fair example before them in the

* See p. 65.

Act recently obtained by the Glasgow Faculty. If they fail to follow this example, so far at least as to concede a right of examination on the production of an English diploma or license, and still stand upon their ancient privileges, they must not complain if the Apothecaries' Society occasionally put in force *their* rights against the Edinburgh graduates. It is merely an enforcement of the *lex talionis*. The London Colleges have taken the initiative in liberally throwing open their examinations to the possessors of British licenses or diplomas. The Faculty of Glasgow have gone still further: they have instituted an *ad eundem* title; and we think there would be less envy and jealousy in the profession if this plan were more generally adopted. Under a fair dispensation of medical law, the license of one duly constituted College should suffice for a title to practise throughout Great Britain and Ireland.

ALTHOUGH the Assistant Surgeons in the Navy are without any professional representative in the House of Commons, yet their case is not lost for want of an advocate. It is greatly to the credit of some naval officers occupying seats in the House, that they will not allow the grievances of their medical brethren to go unredressed, so long as there is an opportunity of speaking in their favour. In answer to a question put by CAPTAIN BOLDERO on the 5th inst., respecting the steps which had been taken for providing accommodation for assistant-surgeons on board of ships of war, in conformity to the vote of the House on the 8th April last,

"Sir F. BARING said that he had stated, in the course of the debate, that a cabin was dedicated to the service of the assistant-surgeons. On inquiry, it appeared that, although the order was issued by the Admiralty, it was not carried into effect to the full extent. But it was intended that care should be taken that the order should be more

fully carried out. With regard to cabins for the assistant surgeons, the vote of the House would be attended to; but it was not possible by such a vote to add to the accommodation of a ship, and whilst it was the anxious wish of the Admiralty to give accommodation to all officers in the service, yet there were two limits: the first was the efficiency of the ship as a ship of war; and the next was, that they must attend to the accommodation of the crew. In some ships it would be possible to give additional room, and in those ships it was the wish and the direction of the Admiralty that such accommodation should be given to the assistant surgeons; but in others it was not possible, without infringing on the limits he had mentioned. As to the messing, it was his intention that assistant surgeons should be admitted into the ward-room after a certain period—three years of service. On the part of some officers there was more than a doubt as to the wisdom of the vote come to by the House; and, even amongst those who were anxious that additional accommodation should be given, it was felt extremely important for the service that assistant surgeons should at least serve some apprenticeship before they were admitted to the ward-room."

It thus appears that *three months* have elapsed and the order has not been carried into effect to the full extent. It was intended that the order should be more fully carried out. We considered, at the time the vote was taken, that there would be an attempt to evade it by those who, from their official position, would have to carry it into execution. We are, therefore, not surprised at the slow progress made, and the occasional necessity of applying some nautical stimulus to the Admiralty Board. It now appears that a separate cabin will be allotted where this arrangement is possible; while a three years' apprenticeship will be required before admitting the assistant surgeons to the ward-room. To us it appears, that if any novitiate be required for educated gentlemen, one year's service should suffice.

LECTURES
ON THE
TREATMENT OF DELIRIUM AND
COMA;

(IN SEQUEL TO THE LUMLEIAN LECTURES
FOR 1850;)

Delivered at King's College Hospital,

BY R. B. TODD, M.D., F.R.S.

(Reported by Mr. LIONEL S. BEALE, Med. As-
sociate K.C.L.)

LECTURE IV.

Termination of two cases of pneumonia reported in the last lecture—Delirium of bronchitis—Treatment of rheumatic and gouty delirium—Cases—The delirium of erysipelas—nature of erysipelas—treatment—Means of prevention of the delirium—Case—Advantage of the stimulant treatment in preventing secondary deposits—Cases—Sudden coma and death in erysipelas—Cases.

GENTLEMEN,—In my last lecture I spoke of delirium connected with pneumonia, and related to you a case in illustration of the good effects of the stimulating plan of treatment, which is the plan I advise you to follow when you meet with a case of delirium coming on in the course of pneumonia. The boy Hoesley got well under this plan; and after three days the hepatized lung had begun to resolve, and in seven days complete resolution had taken place, and on the tenth day he was restored to complete convalescence.

I mentioned a second case to you,—that of a woman named Darwin, in whom there was double pneumonia, with extreme prostration. We were very anxious about this case when we last met; but I am now able to say that the treatment has proved completely successful, and that the patient is now proceeding quickly to recovery. In this case the delirium did not go beyond a slight wandering at night; and I attribute the almost total absence of delirium to the fact that the stimulants were early and freely administered.

On the 12th of June we found this woman with complete hepatization of the lower two-thirds of the right lung, and slight pleuritic effusion, with a very feeble pulse of 130, and the respirations numbering 44. She was ordered beef-tea and chloric ether. The bowels were opened, and a blister applied to the right side. On the 13th the signs continued the same, with the addition of fine crepitation, which was audible beneath the mamma in front; indicating that the inflammation pervaded

the entire thickness of the lung from behind forwards. She was now ordered six ounces of wine daily. In the afternoon of this day she became so low, and sweated so profusely, that I ordered three grains of quinine in pill every fourth hour, and increased the wine to half an ounce every hour—namely, twelve ounces in the day.

On the 15th she began to expectorate very slightly. The physical signs were unchanged in the right lung; but on the left side, beneath the scapula, there were bronchial breathing and bronchophony, denoting hepatization of the lower half of that lung behind. A turpentine stupe was applied to the back, and half an ounce of brandy was given every hour.

On the 16th large crepitation began to be audible at the base of the left lung; and on the 18th returning crepitation was audible in both lungs. On the 20th the breathing in the left lung had become quite pure. On the right side there was nothing abnormal audible but slight blowing expiration at the base, which soon disappeared.

Thus, in this case, in three days the pneumonia had begun to resolve; and in eight days the lungs had become natural, excepting such a condensation of the right lung as produced the slight expiratory blowing.

The delirium of bronchitis should be dealt with in every way as the delirium of pneumonia. Depression of the vital powers favours the occurrence of delirium; and bronchitis is, generally speaking, a very depressing disease. If, therefore, you should be attempting to cut short the inflammation in a case of bronchitis by active antiphlogistic treatment, you may look upon the occurrence of delirium as the signal for you to alter your treatment, and to adopt a stimulating plan. There is a great tendency in the delirium of bronchitis and of pneumonia to pass into coma; and, unless prompt means are adopted by the physician, the patient dies. This tendency to coma is certainly greater when antiphlogistic remedies have been used. When coma comes on, you must not relax in giving support; indeed, the failure of the pulse or of the heart's action may induce you to be more active in the administration of stimulants; but, in addition to this, you will probably find great benefit from free counter-irritation, by mustard or blister, or both, to the back of the neck; and it may be necessary to shave the head, and apply a blister to the scalp.

Sometimes, but very rarely, the coma coming on suddenly in this way may arise from a sudden effusion of blood upon or into the brain. We had an example of this last week, in the case of a man of 60 years of age, who was admitted into

the hospital suffering under bronchitis, and in a state of active delirium, having been ill about four days. He was a man of intemperate habits, and had probably suffered from privation: however, the bronchitis alone would have been sufficient to have so far disturbed the balance of nutrition as to bring on active delirium. After he had been in the hospital a few hours he became suddenly comatose, and died in a state of profound coma. On examination, it was found that a considerable effusion of blood had taken place into the arachnoid sac, which had diffused itself over both hemispheres. There was also chronic valvular disease of the heart, and there were deposits in the vessels at the base of the brain.

This, then, was a case in which there was a combination of symptoms of a nature calculated to occasion a good deal of embarrassment to the practitioner. The man was intemperate; he had old heart disease, and also severe bronchitis, which greatly impeded the circulation through the lungs. This impeded state of the circulation through the lungs no doubt tended greatly to congest the venous system of the brain as of other organs. And his cerebral arteries being diseased and brittle, it was not difficult to explain their giving way under the increased pressure which they must have sustained from the retarded return of the blood to the right side of the heart.

The case afforded strong temptation for the use of the lancet, or for the abstraction of blood by some other means. But he came to us too late for the adoption of any such measure; and if it were likely that antiphlogistic means would have done him any real good, some change for the better would have ere this shown itself, for such means had already been adopted before his admission in very free purging, to which he was subjected for thirty-six hours. The delirium began on the third day of his illness, and continued until within three hours of his death.

Undoubtedly in this case there was congestion of the brain: but the cause of the congestion was not in the brain, but in the bronchitis; and the best way of relieving the cerebral congestion was to relieve the bronchitis, for which the best remedy was free counter-irritation and moderate support. It is plain that opium was inadmissible, in consequence of the pulmonary congestion.

On the whole, I do not know that we could have done better in this case than what Mr. Jordan did when the patient was admitted,—that is, to have abstained from bleeding, and given moderate support, and applied counter-irritants to the chest. Had he been bled, with the rapid weak pulse

which he had on his admission, he would still probably have died of the coma which arises from a deficient supply of blood, or from the supply of a watery blood.

Rheumatic and gouty delirium.—I shall now proceed to speak of the treatment of the rheumatic and the gouty forms of delirium.

Rheumatic delirium generally comes on suddenly in the course of rheumatic fever, and is frequently *coincident* with pericarditis and endocarditis. I say it is *coincident* with the cardiac affection, and not necessarily related to it as effect to cause; because the amount or severity of the delirium bears no proportion to the intensity of the inflammation; for we may have slight peri- or endocarditis with severe delirium, or we may have slight delirium with extensive inflammation of the heart; and we know well that the cardiac inflammation often exists without the delirium. The severity of the delirium is, however, in proportion to the debility of the patient. When you have a very watery state of the blood, or where the blood is deficient in colouring matter, and when, also, perhaps, the blood is much charged with the rheumatic poison, you have a condition highly favourable to the production of delirium.

The rheumatic delirium undoubtedly occurs more frequently in those cases of rheumatic fever which have been treated by bleeding; and probably because bleeding makes the blood watery, and diminishes its red particles. When I was in the habit of bleeding in every case of rheumatic fever, as I used to do some years ago, a much larger portion of my patients were attacked with delirium than now, when I never bleed in this disease: indeed, now I find it difficult to meet with a case of this form of delirium to show you.

Now let us suppose that you are asked to see a patient who had been labouring under rheumatic fever, and who has suddenly become delirious, with or without difficulty of breathing. From what you now know of the clinical history of the disease, you would at once examine the state of the heart, whether there were any symptoms referable to that organ or not. Let us suppose that you find, as in the majority of cases you will do, a rubbing sound, showing the existence of pericarditis, or a bellows sound, indicative of valvular disease or of extreme anæmia. What are you to do in such a case as this? The patient is quite delirious, tosses about the bed-clothes, talks wildly, tries to get out of bed, and will not be restrained.

Although there is inflammation of the

heart here, and none of the brain, yet I can assure you that the symptom which demands most urgently your first attention is the delirium; and that whatever may be your theory as to the relation of the delirium to the affection of the heart, the interests of your patient demand the instant adoption of means to quiet the state of excitement into which he has passed. Important and serious as is the affection of the heart, as compared with the delirium, it is just now of no moment; for if the delirium be allowed to continue, there is great danger of the patient dying suddenly from exhaustion.

Fortunately, however, we have in opium a remedy equally applicable to the cardiac, as well as to the cerebral affection. In these cases you must give opium to produce sleep, just as you would do in traumatic delirium or in delirium tremens. Opium thus given will quiet not only the brain, but the heart also; and it no doubt exercises a favourable influence on the inflamed serous membrane likewise, checking the spread of inflammation and promoting its resolution.

At the same time, you will find it useful to apply a blister to the region of the heart, and to promote a free discharge from the blistered surface. But avoid bleeding, whether general or topical, if you wish to get your patient through without untoward symptoms, and reserve all other depressing treatment until the symptoms of delirium have passed away. During delirium you must also be careful to administer support, because the very existence of delirium makes great calls on the powers of the patient, and he therefore requires to be freely supplied with nutritious matter easy of digestion, and with stimulants. You need not be deterred from giving opium, and administering support and stimulants, by the fear lest such treatment should increase the cardiac inflammation; the experience of many cases convinces me that under this plan the heart becomes less irritable, the pulse slower, and the cardiac inflammation becomes circumscribed, and it tends to terminate by resolution or adhesion rather than by the effusion of water. On the other hand, if you persist in an antiphlogistic plan, you keep up or increase delirium, you exhaust the powers of the patient, and effusion is apt to take place into the pericardium, whereby the heart's action may become seriously impeded.

If this treatment be commenced early, and pursued effectually, it is rare indeed for the patient to go wrong.

It may, however, be objected to this plan, that patients have been bled, and nevertheless have done well. Undoubtedly this is

the case, and I could refer you to several cases of this description: but although the antiphlogistic plan has been pursued up to a certain point, you will generally find that the obvious exhaustion of the patient has suggested, sooner or later, a change of treatment. But how many cases are there which have ended fatally under the antiphlogistic plan! I might easily collect a considerable number. The first case which arrested my attention as to the inefficacy of the antiphlogistic treatment was one of a young and handsome girl, who had severe rheumatic fever, in the course of which she became highly delirious, and at the same time showed the signs of pericarditis. I treated her for acute arachnitis: the head was shaved, cold applied, mercury freely given, leeches applied to the temples, and the full force of an antiphlogistic treatment was brought to bear upon her. She died; and on examining the head I found the membranes of the brain free from all signs of inflammation, but pale and bloodless.

The treatment of this form of delirium by antiphlogistic means is, in my opinion, as inexpedient and as dangerous as that of delirium tremens by a similar plan; and we are not justified in assuming that the practice is safe or desirable because cases have got well under it.

I have but little to say upon the delirium which accompanies acute gout, because the remarks I have made upon the rheumatic delirium are equally applicable to it. I shall content myself with stating that, while the delirium is essentially of the same nature as the rheumatic, it has a greater tendency to pass into coma. While, therefore, our treatment ought to be in all other respects of the same kind, we must not be so ready to administer opium as in the rheumatic delirium; we should wait to observe whether there is any marked tendency to coma. I had a well-marked example of this form of delirium in one of the College porters, a stout lusty man, who used to attend the gata. After having had several attacks in the great toe, he had a very severe attack of general gout, resembling very much rheumatic fever, but without any inflammation of the heart. He became violently delirious, so much so as to require restraint. I was called to him in this state, and prescribed stimulants, and treated him much in the same way as if it were a case of delirium tremens; but I did not give him opium. He took hyosciamus, and after a time, as there appeared to be no tendency to coma, I ordered him small quantities of opium. He soon improved, and got rapidly well.

In the delirium of rheumatism and gout we occasionally meet with cases of sudden

death from exhaustion: the patient, after an effort, suddenly falls back dead. Hence, when delirium shows itself during rheumatic fever or acute gout, the attendants of the patient should be warned of this danger, and distinctly instructed to watch the patient closely, so as to prevent as much as possible all exertion, and to keep him in the horizontal posture.

One point I wish particularly to impress upon you is, that as in pneumonia, and in cases of severe injury, you may prevent the delirium, so also in rheumatic fever and in acute gout you may keep it off by avoiding extreme antiphlogistic measures, under the idea of cutting short the disease. I am convinced it is an erroneous notion, to which careful clinical observation gives no countenance, that such a disease as rheumatic fever may be cut short by a bold stroke of the lancet. There are mild cases as there are severe ones, and it is not easy to distinguish the one from the other at their onset. The practitioner, therefore, who bleeds as a matter of routine, will often meet with cases which will *appear* to have been cut short by such a practice. And even if you do cut short the disease by the sudden abstraction of a large quantity of blood, it will be at a tremendous cost to the patient, involving years of subsequent delicate health.

Our object in the treatment of these cases should be, not so much to cut short the disease by debilitating the patient, but carefully to guide him through the malady, promoting the elimination of the morbid poison, and at the same time protecting his powers against its depressing influence. By this mode of proceeding we do not pretend to cut short the disease, but we shall guide our patient safely through it, and shall much shorten the period of his convalescence.

The delirium of erysipelas.—The next subject to which I shall call your attention is the mode of treating the delirium of erysipelas. I know no disease the pathology and treatment of which are more worthy of the careful consideration of the practical physician or surgeon than erysipelas, as it is a malady of very frequent occurrence, and very severe and destructive in its nature. The question of the treatment of the delirium of erysipelas involves the consideration of the treatment of erysipelas itself. I refer here of course particularly to the idiopathic erysipelas, which comes under the observation of the physician; but the remarks I have to make will apply equally to the phlegmonous form which you will meet with in the surgical wards, for the two forms of the disease are of essentially the same nature,

Let me begin by stating what appears to me to be the most reasonable view of the nature of erysipelas, and the most consonant with its clinical history. Erysipelas is produced by the introduction into the system of a morbid poison, generated in the body under certain circumstances, or derived from another individual affected with the disease. A patient who has received an injury may generate the poison of erysipelas in his own person by exposure to certain deleterious influences; or he may, under similar circumstances, generate this poison in his own body, even when he has not received any wound, in which case we call the erysipelas *idiopathic*. Or whether wounded or not, he may imbibed the poison from some one labouring under the disease, even without actual contact.

The poison of erysipelas, like that of measles, of small-pox, of scarlet fever, &c., has its special habitat,—that is, it shows a proneness to affect certain tissues to the exclusion of others; and it attacks specially the skin and the gastro-pulmonary mucous membrane, sometimes affecting the throat first, and travelling outwards to the skin; at other times taking the contrary course. This poison produces other effects than mere cutaneous inflammation; its introduction into the blood causes a fever of a very depressing nature, and this amount of depression bears no constant relation to the extent of the cutaneous inflammation. A large dose of this poison may kill a patient in a few hours, causing at the same time only very trifling local disturbance, such as a trifling redness of the throat. Generally speaking, however, it requires many days for its elimination, during which time the patient exhibits the local and constitutional symptoms with which you are familiar.

Like the other morbid poisons to which I have alluded, the poison of erysipelas exhibits secondary and tertiary effects: secondary effects in the formation of abscesses in various parts of the body, and even in parts which may not have been affected by the cutaneous inflammation; and tertiary effects in chronic inflammation and induration of various glands, and the deposition of scrofulous matter.

What is the most reasonable and the best treatment of erysipelas? have we any means of eradicating the poison by a bold stroke at the onset of the disease? Slight cases—or, in other words, cases which have imbibed only a small dose of the poison—will quickly recover, with little or no medical treatment; and such cases, when treated early by a bold antiphlogistic measure, will appear to have been cut short. But, in the majority of cases, when a large dose of the poison has been imbibed, I believe that

the disease must pass through a certain course, that the poison will produce its specific effects, and that the principal business of the practitioner is to uphold the powers of the patient so as to limit as much as possible the destructive influence of the poison. In such a case you can no more think of extracting the poison by some active stroke of treatment, than you can think of eradicating the poison of small-pox or scarlet fever under similar circumstances. Any one who would, now-a-days, think of such a thing in these maladies, would be set down as having lost his senses. We have no means of extracting the poison; we must, therefore, endeavour to guide our patient through the various stages of the fever it excites, into port, or we must find some antidote for it, which, when introduced into the blood, may counteract its depressing and destructive effects.

There is no treatment which appears to me to be so generally applicable and so safe in cases of erysipelas, as that which we call the stimulating treatment—by support by means of nutritious food in the liquid form, and by bark, or ammonia, or chloric æther; nor is there anything which seems, in some cases most strikingly, to act so much as an antidote to the erysipelatous poison, as alcohol, given either as brandy or wine, or beer, but more especially as the first.

In all cases of erysipelas, then, my practice is first to evacuate the bowels, when necessary, by such means as will not occasion any undue depression, and then to give nutritious food, easily digestible, with stimulants, as brandy or wine. I adjust the quantity of these according to the urgency of the symptoms, keeping, however, to this rule—namely, to give small quantities very frequently, and at short intervals, rather than larger quantities administered occasionally. This is, if I may use the expression, the staple of my treatment: to it I add such medicines as ammonia, bark, &c.; but if the stomach be irritable, or the patient opposes or is disgusted with the administration of so many things, I prefer giving up the drugs and trusting to food and brandy only.

Patients treated in this way from the beginning do not often become violently delirious. When delirium sets in in erysipelas, it may be taken as an indication that our patient wants more support and more stimulus; and the more quickly and freely you give them under such circumstances, the sooner you will subdue the delirium. If freely given just at the beginning of the delirium, you may check it completely. Of this I had a remarkable example last summer, in the case of a

clergyman, a robust man who came to town with his family to see the lions. He caught cold, as he thought, from sitting one hot day, with a north-east wind, near an open window in an omnibus. That evening he had a severe rigor, and vomited. I saw him soon after, and found some redness of the mucous membrane of the nose, spreading outwards on the skin. I immediately suspected erysipelas, and the next day confirmed my suspicions, as the redness had extended over the nose. He was moderately purged, and to his surprise I allowed him two or three glasses of port wine at once. Under this treatment he seemed to go on well for a couple of days. One night I was led almost by accident to visit him at a very late hour; and to my surprise he was looking strange, talking rather wildly, and wanting to get up and go out. I immediately called for the brandy bottle, and gave him a large dose of it, which seemed to quiet him; and I left instructions with the nurse to give him smaller quantities at short intervals through the night. Next morning I found that he had slept through the greater part of the night, and that the delirium had completely ceased.

In cases which show a tendency to coma, it is often advisable to shave the head and apply blisters freely to the scalp or back of the neck. Nor need you be deterred from applying blisters, by the fear lest the blistered surface shall become the seat of new erysipelas. As a rule, it is not desirable to give opium in the delirium of erysipelas, unless the delirium is decidedly of the active and wakeful kind; on the whole, however, the other narcotics, as henbane, camphor, hop, are safer than opium.

The stimulating treatment in erysipelas not only saves the patient from delirium, or cuts it short when it appears, but it shortens the period of convalescence, and, what is very important, it seems to diminish the tendency to secondary deposits. We have had many cases in the hospital which exemplify this remark. You may recollect a very severe case of erysipelas last winter in Lonsdale ward: the patient was quite delirious, and there was afterwards a tendency to coma; nevertheless, this patient quickly recovered, and had no secondary abscesses: she was treated by stimulants from the beginning. In Augusta ward, also, we had a similar case about the same time, which was treated in the same way with a like result. The clergyman's case, to which I just now alluded, afforded another example of the absence of secondary deposits under stimulating treatment, although the erysipelas extended over the whole scalp. His wife, on the other hand, who caught the disease from him, had a

very irritable stomach, and could not take stimulants; and she had an abscess in one of the upper eyelids. In the case of a young lady whom I attended in the spring, along with Mr. Bowman, with severe erysipelas, there were no secondary abscesses; she was treated by brandy and nourishment from the beginning; and I could enumerate several other instances in which this desirable result followed the stimulating treatment.

Now and then it will happen that an erysipelatosus patient will rapidly become comatose, and die in spite of all our remedies. There was a man in Sutherland ward a short time ago, affected with erysipelas, who appeared going on very well for some time, but suddenly he became comatose, and died, and we could not make out any reason for his death. The only reasonable explanation that I can offer of the sudden change for the worse which sometimes occurs in these cases, is derived from the well-known tendency they have to form pus, which, accumulating at some point, may find its way through the ulcerated coats of some small vein, and thus enter the circulation, producing coma and complete prostration.

I shall conclude this lecture by referring to two cases as examples; the one to show the rapidly fatal influence of the poison of erysipelas, and that the extent of the cutaneous affection bears no relation to the effects of the poison on the system; the other to illustrate the good effects of an early stimulant treatment.

The first case is that of a man named Collins, *æt.* 27, who was admitted into the hospital about three weeks ago. He had always been temperate in his habits, and enjoyed good health, with the exception of an attack of erysipelas two years previously.

On the 28th of April he noticed a small pimple on his nose, which increased rapidly in size, and on the first of May, having been exposed to cold, he was seized with shivering. He entered the hospital on the 3d of May. The inflammation had spread on the nose and great part of the face and the eyelids, but not so as to close the eyes. He was ordered five grains of carbonate of ammonia every three hours. On the 4th he was evidently much lower; several pustules full of pus had formed upon the nose; but the erysipelas had not spread. Brandy was ordered to be given freely, and chloric ether added to the ammonia. That night, however, he began to breathe quickly, and became comatose, and died on the fourth day from the rigor. It is very probable that had stimulants been administered from the first in large quantities, this patient might have been saved. I confess

that when I first saw him I was not impressed with his being in so low a state.

Upon examination we could find no evidence of inflammation or of any other morbid state of the membranes of the brain; but there were recent adhesions on the right side of the chest, and, as commonly happens, there were signs of recent inflammation of the mucous membrane of the bronchial tubes. This case shows you how rapidly a man may be knocked down by a large dose of the poison, although the external signs by which the disease is distinguished may be very slight.

The second case to which I shall refer is that of Jones, *æt.* 44, which terminated favourably, although in some degree placed under less favourable circumstances than the former.

This man's habits were intemperate. When admitted, he had very extensive erysipelas of the head and face; the eyelids were closed; the tongue dry and brown. This was on Saturday, the 27th of April. He was immediately ordered chloric ether; beef-tea, two pints, and brandy, two ounces per diem. On the 28th and 29th no material change had taken place. The brandy was increased to six ounces on the former day.

April 30.—On the evening of the 29th he became extremely restless and delirious, and it was with great difficulty that the nurse and some of the convalescent patients in the ward could keep him in bed. His allowance of brandy was doubled, half an ounce to be taken every hour, and half a drachm of chloric ether every two hours.

The next day (May 1) it was reported that the pulse had fallen to 92 from 100, and that the delirium had ceased. On the 2d of May he was reported to have passed a very good night without any opiate, and was perfectly rational. The pulse had fallen to 76: this was a most favourable sign. It is especially auspicious when the pulse falls in frequency while the patient is taking stimulants.

From this time this patient recovered rapidly, without the occurrence of any purulent deposits.

This case shows how, by increasing the amount of stimulants, you may subdue active delirium and shorten convalescence; for on the 4th of May this patient was well enough to eat meat. The attack began on the 27th of April, so that in seven days he passed through the various stages of a severe attack of erysipelas, and became convalescent.

At our next meeting I propose to consider the treatment of the delirium of typhus fever, and the delirium of hysteria and of epilepsy.

Rebiews.

The Types of Delirium Tremens, their Pathology and Treatment. By JAMES BIRD, A.M., M.D. Pamphlet, 8vo. pp. 29. London. 1850.

THE treatment of delirium tremens has been attended with difficulty and uncertainty, because it has not been sufficiently borne in mind that it is a disease susceptible of considerable modifications, and often complicated with various and even opposite states of the system. Dr. Bird's object is to point out the varieties thence resulting, and the practical application of their distinction in the treatment of the disease, as observed by himself in the European General Hospital at Bombay, during a residence of four years.

The author describes four types, or forms.

1. *The simple*, or "purely nervous variety, depending on mere erythsm of the nervous system, either of centric or peripheral origin, which runs a certain course, and has a tendency to subside of itself in a given time."

Dr. Bird's experience leads him to fix from four to six days as the average duration of this form of the disease.

2. *The inflammatory*, "marked by a degree of vascular determination to the brain and its connections, or to some of the remote organs, either the stomach or lungs, which may act sympathetically on the nervous system."

3. *The pyrexial*: "occurs more generally in the malarious seasons of the year, from July to October, is frequently met with during the other months, in constitutions rendered irritable by long residence in warm climates, or in persons previously subject to intermittent and remittent fevers."

4. *The epileptic*, "attended with convulsive twitching of the muscles, passing into sudden loss of consciousness and sensation, accompanied by clonic spasms of the muscles, recurring in paroxysms, which terminate in recovery of consciousness and sensations, or in apoplectic annihilation of the cerebral functions."

Tabular statements, illustrative of these several forms of the disease, are given by Dr. Bird. The author also adds observations on the causes and diagnosis of this disease. It appears from the latter, that in India it is not unfrequently mistaken for poisoning by

Datura, the symptoms of which, in some points, resemble those of delirium tremens. Dr. Bird observes on this subject:—"The diagnosis of the simple erythsmal type of delirium tremens, from like cases of narcotic poisoning, will be found in the history and development of the two diseases from their respective causes; in the persistent *insomnia* of the one, contrasted with the somnolency, or, rather, *somnambulism*, of the other; in the manner of the patient, which is busy and active in the former, sluggish and inactive in the latter; in the *greater power of controlling the thoughts* for a moment, which persons labouring under drunken cerebral erethism possess over those who are narcotised by stramonium; and in the *more busy character of the delirium* of the first, compared with the drowsy, low muttering, and catching at objects of the last."

The author's remarks on the subject of treatment are highly judicious. They are directed to the abolition of the indiscriminate employment of opium, and teach the importance of following the indications of the pathological conditions of each case.

Handbuch der allgemeinen und speciellen Gewebelehre des menschlichen Körpers, für Aerzte und Studierende. Von Dr. JOS. GERLACH. Dritte Lieferung. 8vo. Mainz. 1849.

THE part now before us concludes the publication of this very excellent compendium of general and special human anatomy. The subjects comprised in this portion of the work are, the generative organs, the nervous system, and the organs of the senses. Having on former occasions spoken well of the two preceding portions, we need only now reiterate our commendations. We can safely recommend Dr. Gerlach's Handbook to every student who may desire to gain a knowledge of the present state of physiological anatomy. As an acquaintance with German medical literature will henceforth be a *sine quâ non* to a complete professional education, we would advise the student to take Dr. Gerlach's work as his companion in his study of German medical authors. While acquiring a knowledge of the language, he will at the same time derive from this work a large amount of valuable information on the topics with which it is more immediately occupied.

Proceedings of Societies.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, June 25, 1880.

THE PRESIDENT IN THE CHAIR.

A Case of Ileus and Strangulated Hernia occurring in the same Individual. By SAMUEL SOLLY, F.R.S., Senior Assistant-Surgeon to St. Thomas's Hospital.

THE patient was a fine young man, aged twenty-one. Mr. Solly found him, at half-past ten A.M., writhing on the floor in great agony. He had an inguinal hernia on the right side. The account which he gave was, that the swelling and pain occurred simultaneously on going to the water-closet immediately after breakfast. About ten days previously he had obstinate constipation, for which he took some strong aperient medicine, which, when acting, produced a good deal of pain, and left considerable irritation of the bowels, that lasted some days, the evacuations being tinged with blood; but this all passed away, and on the Sunday before the Wednesday on which he was seen by Mr. Solly he took a walk of fifteen miles without feeling any fatigue. On the Tuesday night he made a full supper of stewed beef, and on the Wednesday morning again ate of the same dish, and so heartily that he acknowledged to his friend that he had eaten too much. As he left his home after breakfast, at half-past eight A.M., and was descending the steps of his house, he complained to his friend of a violent pain in his belly, having at that time a swelling in the groin; this pain was so severe that he was obliged to ride up to town, and by the time he reached his destination, in Leadenhall Street, he was deadly pale, and his lips blue with pain. On his arrival he went to the water-closet, and passed some faeces, and in so doing the swelling first appeared in the groin. He says it came down without any pain. The taxis proved unsuccessful, and the hernia was so very sensitive, that he was removed to bed, and ordered a warm bath, and in the meantime ice to be applied. In an hour's time, the ice having produced no effect, the taxis was again tried, but very gently, without avail. The warm bath was next used, but with the same result. Mr. Solly now determined to operate, just four hours after the descent of the rupture. The external and internal rings were divided. The external abdominal ring, which was very tight, was divided without opening the sac, but as this did not release the intestine,

the sac was opened; in it there was a knuckle of gut, which was forced over the external ring, and was of rather a dark colour, and another portion of intestine, at the lower part of the sac, was almost healthy. The internal ring was next divided, and the intestine returned without difficulty into the abdomen. The wound was dressed in the usual way. The post-mortem examination was made the morning after his death, at 9 A.M. The abdomen only was examined. The external and internal rings were found quite free; there was about a foot and a half of small intestine, enormously distended, and nearly black, lying in the pelvis; on raising this, it was found girt by a band about an inch and a half long, which was connected with the cæcum on the one side, and with the peritoneum covering the iliacus internus muscle, close to the internal abdominal ring, on the other: this band was round and firm, and seemed to be either one of those bands of old adhesive inflammation which are occasionally met with in the abdomen, or the remains of the omphalo-mesenteric duct. It must have existed a long time. It was not so tight as to have strangulated the intestine in its undisturbed condition. Mr. Solly believed that this was the cause of the previous obstinate constipation, and also the cause of the subsequent irritation of the bowels, though it all subsided before the fatal strangulation took place which terminated his existence.

ROYAL SOCIETY.

May 30th, 1880.

THE EARL OF ROSS, PRESIDENT, IN THE CHAIR.

On the Structure of the Dental Tissues of Rodentia. By JOHN TOMES, Surgeon-Dentist to the Middlesex Hospital. [Communicated by WILLIAM BOWMAN, Esq. F.R.S.]

THE author of the paper bearing the above title relates the results obtained from an extensive series of investigations on upwards of sixty of the more typical members of the order Rodentia. He finds that not only are the teeth of animals of this order distinguished by strongly-marked structural peculiarities (hitherto not recognised) from other mammalian teeth, but also that the teeth of the several larger groups are distinguished from each other by modifications in what may be called the rodentia type of dental tissues. Mr. Tomes pointed out, in a paper published in Part 2, for 1848, of the Philosophical Transactions, that in the teeth of marsupial animals the dentinal tubes are continued into the enamel. In

the present communication he shows, that the structural peculiarities which characterize and are confined to the teeth of rodents are also mainly resident in the enamel.

The earlier pages of the paper are devoted to a description of those structural conditions which are common to the teeth of the whole order.

Amongst these the author finds, that the extremities of the dental tubes, which in the lower part of the tooth communicate with the pulp-cavity, become, in the extruded portion, sealed up by the development of a layer of non-tubular tissue, which is formed at and near the apex of the pulp-cavity. This closure of the dental tubes is not, however, confined to the teeth of *Rodentia*, but occurs in all teeth in those parts exposed to wear. A similar condition is found to obtain in the osseous tissue which forms the antlers of the deer tribe. If a portion of an antler, previous to its losing its periosteum, be examined, ordinary Haversian systems are found; but if an antler which has been shed be examined, each of the larger Haversian systems will be found to be lined by a layer of transparent tissue, destitute both of lacunæ and canaliculi.

The author considers these conditions to indicate the existence of a general law, viz. that dense tubular tissues, when about to be exposed to wear, or about to be cast off, are first deprived of their connection with the nutrient fluids by the development of a layer of non-tubular tissue between them and the vascular surface, with which they were originally connected, and from which they derived their nutriment.

In order to facilitate description, Mr. Tomes proposes to use several terms as indicative of the number and arrangement of the component tissue of teeth. Thus a tooth which has one central pulp-cavity, from which the tubes radiate to all parts of the dentine, he terms a *dental system*; and if this be coated with enamel, partially or wholly, a *denticle*. A tooth composed of several dental systems, each of which are more or less invested with enamel, and the whole united into a mass by cementum, he describes as a tooth composed of *denticles*; but if the dental systems are united without the intervention of enamel or cement, as a tooth composed of *dental systems*. If the systems are united to each other throughout their length by a narrow band of dentine, the tubes of which belong as much to the one as to the other adjoining system, the tooth is said to be made up of *confuent dental systems*; but if each system is partially coated with enamel, of *confuent denticles*.

The tooth of the *Labyrinthodon* is mentioned as being composed of confluent

dental systems, with an external investment of enamel common to the whole. He objects to this tooth being described as divided into compartments by inflections of the cementum, because the division into dental systems takes place internal to the enamel, external to which the cementum would be placed if there were any in this part of the tooth.

A minute description of the dental tissues of upwards of fifty species of rodents forms part of the paper. They are taken in the order proposed by Mr. Waterhouse in his arrangement of rodentia, published in Johnston's Physical Atlas. The author has followed this arrangement because he finds that it accords with the modifications of the dental tissues. The incisors are described as possessing the most strongly-marked and constant characters.

The enamel of the incisors of rodents has been described by Prof. Owen as composed of two layers—an inner placed next the dentine, and an outer, which extends towards the surface of the tooth. Mr. Tomes objects to this description, on the ground that the fibres of the inner and outer division are continuous. This change of direction and arrangement does not occur in the hare tribe.

The anterior surface of the incisors of many rodents has a deep brown colour, which has been attributed to the presence of a layer of coloured cementum. The author states, that the enamel fibres can be traced through this coloured part to the surface of the tooth, and hence regards the colour as a stain resident in the terminal ends of the fibres, and denies the presence of cementum in this part of the tooth.

The great distinguishing structural feature in this order of quadrupeds consists in a lamelliform arrangement of the fibres of the enamel in the inner division of that tissue.

In the *Sciuridae*, the enamel is composed of parallel layers of fibres, and each layer of a single series, which, in the contiguous layers, pass in contrary directions, so as to give a square pattern to the inner or lamelliform portion, when seen in a transverse section of the tooth. In the outer part of the enamel the decussation ceases; the fibres suddenly change their course, become parallel, and proceed upwards in straight lines to the surface, but without any indication of lamination. In a longitudinal section, the laminae are seen to lie at right angles to the surface of the dentine. This peculiarity of position the author finds to be constant in the squirrel tribe and marmots, but he has failed to find the same angle preserved in the members of any other group.

In the rat family, *Muridae*, a similar lamelliform arrangement of the fibres is present in the inner enamel, but the laminae lie

surface of the dentine, and, as seen in a longitudinal section of an incisor, have well-marked serrated edges. The fibres are generally oval in their transverse section, but when exposed in their length have slight serrations from what may be regarded as their upper and lower surfaces. These little processes occupy the intervals left by the oval figure of the fibres of the layers situated immediately above and below.

The incisors of the Gerboa (*Dipus Egyptianus*) and Dormouse (*Myoxus avellanarius*) show the serration of the laminae, but indistinctly, and are otherwise peculiar in having the layers of the enamel fibres in the lower incisors extended in the length, while those of the upper teeth are placed across the tooth, just as in the squirrel and in the more typical rats. Hence in these creatures a longitudinal section of a lower corresponds in appearance with a transverse section of an upper incisor.

In the rootless molar teeth of the members of this extensive family of rodents, the enamel corresponds in its lamelliform arrangement with that of the incisors; but in the rooted molars, the laminated character is seen near the terminal edge of the enamel only. In the molar teeth of the Gerboa, the dentinal tubes are continued into the enamel, in the same manner as in the teeth of marsupial animals.

In the members of the Hystricine family, Mr. Tomes finds that the enamel presents an appearance very different from that observed in the teeth of the preceding groups. The fibres take a serpentine course, and describe several uniform curves in the inner part of the tissue, the number and size of the curves varying in the different species. Notwithstanding this peculiarity, the fibres are disposed in laminae, which run transversely across the tooth.

In a longitudinal section of an incisor, the laminae appear thick and confluent; while in a transverse section parallel to the course of the fibres it is seen that the curves of the fibres of contiguous layers take an opposite direction, but that those of alternate layers follow the same course. It is moreover seen, that the curves of the fibres, when regarded in the whole breadth of the enamel, are arranged in series, which commence at the surface of the dentine, and instead of following the surface of that tissue in colour, lines proceed obliquely outwards, and cross out near the surface, where the fibres become straight and parallel, and lose their lamelliform arrangement.

This curved disposition of the fibres gives to the enamel, as seen in the longitudinal section, an extremely complex appearance.

In the molar teeth of Hystricine rodents, the author finds a similar arrangement of the enamel fibres, but with the characters

of group less strongly marked than in the incisors, and subject to modifications in the different species.

The *Brathyrgeus maritimus* is found to have teeth structurally similar to the Hystricine family, with which it has been placed, but is now separated, and located in the last section of the *Muride*.

The *Helamys Capensis*, though placed in the rat family in the arrangement which the author has followed, appears, in its dental structure, to be closely related to the Hystricine animals, and in the minor degree to the hare tribe.

In the *Leporidae*, a family containing forty-six species only, and therefore numerically unimportant as compared with the preceding groups, which collectively contain 558 species, Mr. Tomes finds that the incisors differ in structure from those of the preceding families, in uniformly possessing vascular dentine, and also in having the fibres of the enamel continued in an uninterrupted course from the surface of the dentine to the surface of the tooth, and without the slightest indication of an outer and inner division similar to that which forms so constant a character in the incisor teeth of the preceding groups. The lamelliform arrangement of the enamel fibres is broken and indistinct, and their decussation, as seen in a transverse section, is irregular.

In the molar teeth of the hares, the enamel fibres, where the tissue is tolerably thick, proceed through the first part of their course in straight and parallel lines, but afterwards describe one or two large curves, and with an irregular lamelliform arrangement.

The author remarks, that the enamel in the teeth of the *Leporidae* bear a close structural resemblance to that of the teeth of the Wombat, described and figured in his previous communication to the Society, and that the structure of the dental tissues generally is more closely related to those of the *Hystricidae* than to any other rodential family.

The paper was illustrated with numerous figures, drawn from the more typical species of rodents.

It is stated at the conclusion, that the author purposes, in a future communication, entering into the development and special adaptation of the peculiarities of the dental tissues which it has been the business of this paper to lay before the Society.

ACADEMY OF MEDICINE, PARIS.

June 25th, 1850.

Medico-legal Researches on Dried Cerebral Substance.

M. ORFILA read an essay on this subject, with reference to a late assassination. M.

Orfila, with M. Jules Bares, had been requested to state whether a certain substance, adhering to the *blouse* of the accused, consisted of dried cerebral matter. From the smallness of the quantity of this matter, and the want of recorded investigations on this question, the inquiry was beset with considerable difficulty. From their experiments M. Orfila submitted the following conclusions on this very important medico-legal investigation :—

1. Among the organs of the human body the brain has a peculiar and specific reaction with sulphuric and hydrochloric acids.

2. The pancreas, digested in concentrated sulphuric acid, imparts in a day or two a violet tint, analogous to that presented by cerebral matter; but this violet tint has been preceded by a yellowish-brown and then by a red hue, which do not occur with cerebral substance. Besides this, the pancreas imparts to hydrochloric acid a dull slate-grey colour, without any violet tint; this also does not occur with cerebral substance.

3. Muscle imparts a violet tinge to sulphuric acid, at the end of a day or two, but this has likewise been preceded by a red tint. With hydrochloric acid the reaction is similar to that of pancreas.

4. The substances which, adhering to garments or to weapons, might be mistaken for cerebral matter, are, for the most part, white of egg, butter, soft cheese, gelatine, and fat. But these cannot be confounded with cerebral matter if submitted to the action of sulphuric and hydrochloric acids.

5. Albumen imparts a violet colour to concentrated sulphuric acid, but with hydrochloric acid a blue colour is produced, as deep as that of ammoniaco-sulphate of copper; exposed, however, to heat, a violet colour results, which passes into deep (purple) brown. The cerebral matter, on the contrary, does not dissolve in hydrochloric acid, even after twelve days; at the end of several days it presents a slate-grey tint, passing faintly into violet, and then to a red, without exhibiting any trace of blue.

Cheese produces a violet colour when dissolved in sulphuric acid, but it may be distinguished from cerebral matter by the ready precipitation of the latter on the addition of water. Cheese, also, is precipitated of a black colour by chloride of nickel; while cerebral matter is thrown down of a green colour, by this reagent, from its solution in sulphuric acid.

Dissolved in hydrochloric acid, cheese that has been dried by the sun presents almost immediately a clear rose, then a violet, and lastly a dull grey colour; whilst

brain produces no discoloration for a long time, and then passes to a grey with a very slight violet tint.

6. It was not possible to detect the phosphorus of cerebral matter in so minute a quantity as was presented for examination.

7. Acetic acid was of no use in the experiments.

8. Sulphuric and hydrochloric acids suffice to distinguish cerebral matter from albumen, cheese, &c.

The microscope (of 580 to 600 diameters) detected a very minute quantity of cerebral substance, and confirmed the chemical analysis. The blood-globules may also be distinguished by the microscope if the substance, or article to be examined be first treated by a concentrated solution of sulphate of soda.

. The chemical differences here assigned require further investigation before they can be adopted in practice. This remark especially applies to the action of hydrochloric acid on the cerebral substance.

Influence of Vaccination on Population.

M. BOUSQUET read a paper in which he refuted the assertion of M. Carrot, that vaccination is the cause of typhus, phthisis, &c. in youth; and that consequently, in saving the lives of infants, it only preserves them for certain death at a more advanced age.

SURGICAL SOCIETY OF PARIS.

June 19th and 26th, 1850.

THE discussion on M. Chassaignac's proposed new treatment of abscesses was concluded. M. CHASSAIGNAC submitted his plan as the result of his experience in numerous cases, and invited the members of the Society to make their own experiments thereof.

Amputation of the Tonsils.

M. GUERRANT presented Fahnestock's new instrument, which permits the performance of this operation with such facility and rapidity, that it may be performed upon the most intractable child without assistance.

MM. LARREY, CHASSAIGNAC, and LENOIR, considered that the usual mode of amputating the tonsils is preferable to the employment of so complicated an instrument as that submitted by M. Guerrant.

New Forceps to supersede Museux's.

M. LARREY presented the forceps invented by M. Luër, and which have been

used with advantage by MM. P. Dubois and Maisonneuve. Instead of terminating as crotchets, the ends of the branches presented two rings, which joined on the blades being closed. A toothed rack was attached to maintain the branches in a fixed position.

*** We are at a loss to comprehend from this description the mode of application, and the superiority of these forceps.

ACADEMY OF SCIENCES, PARIS.

June 24th, 1850.

The Diet of the Miners of Charleroi.

M. MAGENDIE read a letter from Dr. Charpentier, of Valenciennes, in which it was stated that the account of the diet of the miners of Charleroi, given by M. Gasparin on a previous occasion, was not in accordance with his own observations.

It is true, observed M. Charpentier, that the miners subsist on the coffee and bread-and-butter diet, described by M. Gasparin, when in the shaft; but they do so because a more substantial diet is digested with difficulty in the painful and constrained posture they are obliged to maintain in the galleries. At other times these men make use of a meat diet, with beer, brandy, &c.

With reference to the health of these workmen, Dr. Charpentier quoted the report of the Inquest by the Belgian Government,—that they manifest all the usual effects of the excessive use of some muscles and the total disuse of others; besides which, their constitutions are so impaired, that at the ages of thirty or forty years they become the subjects of fatal disease. The superiority which certainly exists in the health of the miners of Charleroi over that of the labourers in the mines of Mons, Liège, and Anzin, M. Charpentier traced to the difference in their mode of ascent and descent to the mines. In the last-named mines the workmen are extremely fatigued, and fatal affections of the heart are induced by the method of ascent and descent.

Dr. Charpentier added that he had investigated this subject because the statement of M. Gasparin appeared so contrary to all that chemistry has taught, and because he attributed the frequency of phthisis, &c., to the low vegetable diet of the labouring classes in France. Dr. Charpentier accounted for the greater amount of labour of which English workmen are capable, by the superiority of their diet.

Some points in the History of Fecundation in the Lower Animals.

M. QUATREFAGES read a paper in which

he stated that the experiments of Spallanzani, and of Prévost and Dumas, on the spermatozoa and ova of Batrachians, are few in number, and from their having been performed on fresh water animals, the results do not admit of generalization.

M. Quatrefages had endeavoured to supply the deficiency by a series of researches on *Annelida* and *Mollusca* living in salt water. The author had studied the influence of increase and decrease in the saltiness of the water,—the action of acids, bases, and salts. The following are some of the results at which M. Quatrefages had arrived:—

The spermatozoa of the Hermaphrodites bear a decrease, better than an increase of the saline matters of sea-water. The bases exert a less influence than the acids. Organic poisons act much less energetically than mineral poisons. Among mineral poisons the nitrate of copper and acetate of lead exert an equal influence; while corrosive sublimate acts more energetically upon the spermatozoa of the *teredo* than on those of others. In all aquatic animals having an external fecundation, a certain degree of dilution is required for the fecundating liquid to attain the maximum of its power. In the Batrachians, beyond a certain limit the number of fecundated ova is in proportion to the number of spermatozoa. The immediate contact of the ovum and of the spermatozoon is necessary to fecundation. The spermatozoa, which have been apparently killed by some noxious agent, are capable of restoration to activity. Contrary to that which obtains with Batrachians, the ova of the *teredo* are capable of fecundation after long residence in water not containing any spermatic fluid. A slight increase in the saline ingredients of sea water prevents fecundation in the ova of *teredo*. The addition of a small quantity of fresh water favours fecundation and development of the larvæ. The action of poisons is less energetic upon the larvæ than upon the ova and spermatozoa.

FRACTURE OF THE SKULL.

THE uncertainty of the trephine, as a means of cure, was demonstrated in the case of an unknown man, who died a few hours after admission. The fracture was in the base of the skull; but the principal coagulum, which was of great size, was on the opposite side, and in the upper and middle portion of the cerebrum.—Dr. Hartshorne, in *American Journal of Med. Sciences*, 1850.

Hospital and Infirmary Reports.

KING'S COLLEGE HOSPITAL.

Hernia—Stricture—Taliacotian operation.

In a former report from this hospital (see MEDICAL GAZETTE, May 31), was a case in which Mr. Bowman had operated for femoral hernia: we mentioned that, on the 25th of April, fecal matter was coming away from the opening made to relieve the stricture. On May 1, the powers of the patient had begun to give way; she was beginning to lose flesh; she had a troublesome cough; the tongue had become furred and dry; pulse 100, feeble; patient lying in a sleepy condition: she, however, took nourishment well, and did not vomit. The wound looked ill: the sac commenced to slough; and a portion of the intestine could be seen exposed, and in a gangrenous condition. Free stimuli and nutritious broths were ordered.

May 4th.—The patient is in a somewhat better condition; the tongue is moister; the pulse is still about 100, small and feeble; the slough is separating from the wound; and less fecal matter comes away by the artificial anus. The patient takes her nourishment well, which consists of fish, beef tea, and wine.

7th.—Slough has entirely separated from the wound, which is beginning to granulate; pulse still continues small and weak; tongue moister and cleaner; very little fecal matter comes away by the wound, most of it coming through the natural passage.—Ordered wine freely, and a mutton chop.

10th.—Patient doing well; the wound is granulating in a healthy manner, but still a large quantity of feces come away through it.—To go on.

15th.—She has remained in a similar state since the last report, if anything, however, the patient is lower; she still lies in a sleepy condition; pulse very small; faeces somewhat dusky; she does not vomit; nearly all the fecal matter comes away by the wound. She is plentifully supplied with stimulants and nutritious food.

25th.—This old woman still continues in a curious condition: since the last report, she has fluctuated much; at different intervals she has been very low, and as though she were about to sink, but during the last few days she has improved, her countenance has become more cheerful, her tongue cleaner, and she has taken her nourishment well. As the faeces came almost entirely from the wound, and the opening in the intestine, which was at

the upper part of the wound, appeared to be valvular, Mr. Bowman applied careful pressure on the upper part of the artificial orifice, and this appeared to act with efficacy for two or three days, as less fecal matter came away; but at the end of this time vomiting occurred, and it was found necessary to omit the pressure. Now, however, much less comes by the wound, and the patient has motions "per vias naturales." She is supported by a large quantity of stimuli and fluid nourishment.

June 1st.—Since the last report matters have taken an unfavourable turn with the patient: she gradually began to sink, although no symptoms of any inflammatory mischief presented themselves; the pulse continued feeble, varying from 90 to 100; the tongue remained slightly furred and dry; vomiting occurred at intervals; some of the faeces continued coming through the opening, and some by the rectum. She was supported by a liberal amount of stimuli, but she gradually got worse, and died on the 31st of May.

On making a post-mortem examination, nothing particular was found which could account for death. There was some slight effusion of lymph on the peritoneal surface of the intestines. On looking at the parts from within, it was found that a complete artificial anus had been formed, and so effectually as to prevent the effusion of matters into the peritoneal cavity; the portion of the small intestine which had been opened by ulceration was firmly attached by adhesive matter to the circumference of the opening.

This case must be looked upon as one of extreme interest, both to the practical surgeon and pathologist, for it was here shown by the post-mortem examination how well nature's efforts had succeeded both in remedying an accident which had occurred after the operation, and in preventing the disastrous results which would have ensued from the escape of the contents of the intestine into the general cavity of the peritoneum. A complete artificial anus had been formed, and the intestine was so firmly and closely adherent on all sides to the opening on the inner surface of the abdominal wall, that no escape of fecal matter into the cavity could take place.

Herein was beautifully shown how well nature works in such instances; and had there been sufficient strength of constitution in this patient, the ultimate result would, in all probability, have been favourable: but, unfortunately, the subject was old, and exceedingly fat, which condition of things is at all times unfavourable to the success of a severe operation.

It is somewhat interesting to ascertain

how it was that the intestine should have become perforated in this instance; for, as it was mentioned in the former report, that portion of the gut which was found lying in the sac at the time of the operation, although it was highly congested and dark as chocolate, did not present any features of gangrene. The cause of the subsequent gangrene, in all probability, arose from without, for it was seen that sloughing had attacked the hernial sac lying in the wound, and then a portion of the intestine was discovered to be in an equally diseased condition. Most probably the sloughing had extended from one to the other—the sac first, and afterwards the gut.

In our report of the same date, we mentioned a case of impermeable stricture of the urethra complicated with stone, for which Mr. Fergusson operated: we shall now continue the history of this interesting case up to the present time. For a few days after the operation this patient seemed to improve very much, the urine came freely away from the opening in the perineum, and much less from the sinus in the abdominal wall, and his appetite increased; but about a week after this proceeding, the patient began to go back, and he was seized with vomiting, became jaundiced, and had some hæmorrhage from the gums; he also complained of considerable pain in the abdomen, particularly on the right side, which was very tender on pressure, and on examination the liver was found to be enlarged and painful. Calomel and opium was ordered, and hot fomentations were applied over the abdomen, which gave the patient considerable relief.

May 2d, twelve days after the operation. —His countenance is somewhat anxious; he vomits occasionally; and complains of considerable pain over the abdomen, more particularly when the region of the liver is pressed; pulse 100, feeble, but irritable; countenance still slightly tinged, but not so jaundiced as it was. Dr. Todd saw this patient to-day, and ordered one grain of opium to be taken three times a-day, and hot fomentations, with laudanum, to be applied over the abdomen; fluid nourishment to be taken.

4th.—Feels and looks much better; the opiate treatment has given very great relief; there is now no jaundice, and the countenance is much more cheerful; tongue clean; pulse 100, still very irritable. As he has been so ill, Mr. Fergusson has not meddled with the wound, and has not attempted to pass any catheter. The urine still comes freely away by the wound, and he is much less disturbed than he used to be by the irritability of his bladder.

7th.—Improving; has not much pain in

the belly; liver is still large and somewhat painful; countenance much clearer and more cheerful; tongue clean; pulse 100; urine still keeps coming freely away through the wound. Takes some wine, and nourishing broths, &c.

10th.—Still doing well. Mr. Fergusson attempted to-day to pass a small catheter through the strictured portion of the urethra, which involves the greater portion of the corpus spongiosum, but it was perfectly impermeable to the catheter.—Potassa Fusa was therefore applied to the face of the stricture.

15th.—The Potassa Fusa has been again applied, but no instrument whatever can be carried through the strictured portion of the canal. The urine still comes freely away by the hernial opening, and a large gum catheter can be easily passed through it into the bladder. Very little water comes from the sinus in the abdominal walls. The patient, although much better than he has been, still is very weak.

May 25th.—This patient has lately been improving so much, that he is enabled now to get out of bed, and put his clothes on. He feels very much the benefit of the operation which has been performed upon him: the urine comes away freely by the perineal opening, which has been kept in free communication with the bladder by the passage from time to time of a short elastic catheter: there is comparatively little irritability of the bladder. Still, however, little progress has been made with the stricture in front of the opening: it is so long and tough, that the Potassa Fusa appears to have made scarcely any impression upon it, as no instrument whatsoever could be got down. The general health is much improved, although it continues in a weak condition.

29th.—Mr. Fergusson, at his visit to-day, made a very careful and patient attempt to get a small wire through the stricture, but although after a long trial he was enabled to get some way into the stricture apparently, he could get nothing through it. He therefore divided it from within by an instrument of his own invention, consisting of a long director, with a deep groove in it, and a long narrow knife, with a cutting edge at its extremity, of about one inch in length. He first passed the director down to the stricture, and then passed the knife into the groove, which is so deep that the cutting edge of the knife does not come into contact with the healthy urethra, but only cuts the stricture. He was enabled by this means freely to divide it on either side. An attempt was now made to pass a catheter, but it was not successful.

June 1.—Mr. Fergusson was able, at his visit on the 29th, to pass with some little

difficulty a No. 2 catheter fairly along the urethra, into the bladder, and to-day he passed, without difficulty, a No. 4, and the patient has suffered but little from the cutting operation lately performed.

We shall continue the report at a future time of this extremely interesting case.

In a former report we related an instance in which the Taliaacotian operation had been performed on a young man by Mr. Fergusson; at that time the first steps only of the operation had been done,—that is to say, the integument of the forehead had been detached, and drawn over the old nose.

Almost complete union took place on either side, where the raw surfaces were in apposition; but unfortunately the patient was seized with a most severe attack of erysipelas, whereby his life was placed in imminent danger. The symptoms were so severe a few days after the commencement of the attack, that it was deemed necessary to apply restraint: the man became violently delirious, and remained in a state much resembling acute mania, for several days; in this state he was removed to the physician's hands, where Dr. Todd saw him, and under his superintendence the patient gradually recovered. His nose was in great danger of being pulled off during the maniacal attacks under which he suffered, but he was carefully watched and restrained, and fortunately the union of the wounds was not prevented.

He was permitted to go out for some weeks to recruit his health; he returned at the beginning of last month, and on examination there was found to be a very firm union of the new nose, on either side, with the exception of a small portion near the apex; Mr. Fergusson therefore carefully pared the edges, and having separated the narrow connection of the nose to the skin of the forehead he brought them accurately together by sutures. The patient, fortunately, has not suffered from any erysipelatous attack this time; perfect union of the whole of the new nose with the face has taken place, and the patient altogether makes a very respectable appearance, and certainly is a much less disagreeable object than he was before the operation was done upon him. It is remarkable to observe how little disfigurement there is on the forehead, for although a very large raw surface was left after the first operation, the parts have cicatrised so well, that very little is there to be seen, and this little the patient will be able to cover with his hair.

Medical Intelligence.

THE CHOLERA ON THE CONTINENT.

Vienna and Venice.—This disease has recently made its appearance at Vienna and in Venice, and has occasioned numerous deaths.

Prague.—Accounts from Prague of the 26th June, state that the cholera has been very severe in that city during the last three weeks. From the 16th to the 23d June there were 57 cases, of which 28 proved fatal.

Algiers.—The cholera has reappeared on the northern coast of Africa. It destroyed in one day, at Tunis, 150 persons.

Malta.—Letters from Malta of the 2d inst. state that there is now no doubt of the cholera being in the island. Several cases occurred on board the fleet, some of which proved fatal. Among the troops the disease was confined to the 44th Regiment, quartered in the low, ill-aired buildings at Florian. There were, exclusive of the men of both services, 39 cases and 34 deaths in the city. A postscript, written at post hour, mentions the total number of cases among all classes at 82, and deaths 60. Cholera, though not of a dangerous character, was raging among the children.

London.—In the Registrar-General's return for the week just completed, there is an account of one death from (English) cholera. This was the case of a boy of seven years residing in Lambeth. The disease proved fatal in fourteen hours.

ALLEGED PROTECTIVE POWER OF SYPHILIS AGAINST CHOLERA.

M. PROVENCAL, of Montpellier, has addressed a communication to the President of the Republic, claiming the honour of the discovery of the immunity of venereal patients from cholera, owing to the protective power of the syphilitic poison. The same physician proposed the inoculation with syphilitic poison, and demanded a national recompense for the said discovery.

The President of the Academy, M. Velpeau, however, and several other members, stated that they had seen many syphilitic patients die from cholera. So that the protective power of syphilitic virus, or of the therapeutic mercurial action, does not exist. The practice has been officially reprobated and authoritatively forbidden.—*L'Union Médicale.* X

MILLBANK PRISON — STATISTICS OF MORTALITY, &c., FOR 1849-50.

THE recently printed report on this prison

announces that it is intended that about 700 cells shall be used for the detention of prisoners passing their probationary period in separate confinement previously to being sent to penal labour or public works, but the period of separate confinement in Millbank, owing to its peculiar local circumstances and the unfavourable influence they exercise on the health of the inmates, will not generally be continued beyond eight or nine months, instead of twelve months, which is fixed as the period of detention at Pentonville and other prisons of that class. The number of convicts in the prison on the 1st of January, 1849, was 1,144 (1,058 men and 86 women), and on the 1st of January last, 1,093 (955 men and 138 women). The deaths in the year were 94, or 8·2 per cent. The expenses of the establishment were £31,821. 18s. 8d., and the net amount of profit by prisoners' earnings in the year £3,760. 15s. 6½d. The smallness of the earnings arose from the breaking up of the establishment on account of the cholera which prevailed last year.

GREAT MORTALITY FROM FEVER, AT RIO JANEIRO.

ACCORDING to the latest intelligence from Rio Janeiro, we learn that the fever had somewhat abated on shore, but amongst the shipping it still continued its ravages: 14,000 of the inhabitants are reported to have fallen since January, out of which about 120 are English residents and 150 British seamen, including 8 masters and 18 mates of merchant ships; a still greater mortality occurred amongst the foreign shipping. Her Majesty's ship Tweed had lost 18 officers and seamen; Her Majesty's steamer Comorant, 12 officers and seamen; the Portuguese ship-of-war Vasco de Gama had lost 150 of her officers and crew; and the constitutional Brazilian frigate, 100 officers and men. At Bahia and Pernambuco the fever still continued, but not with such virulence.

OBSTETRICS IN SPAIN.

ONE of the leading journals of the Spanish capital has the following extraordinary announcement regarding the expected delivery of the Queen of Spain. It has evidently been penned by a *lunatic*. It is to the effect that "the delivery will probably take place this week. As the moon is well known to exercise an influence over such events, the confinement of Her Majesty is generally expected, either on Tuesday, the 2nd, or on the 9th, the day of the new moon."

. It is our belief, that the moon has no such influence in the British empire!

STATISTICS OF THE POPULATION OF PARIS.

In 1848 there were in the city of Paris—

Births	32091
Deaths	30088

Excess of deaths over births 2003

Births.

Males	16028
Females	15968

Excess of males over females 955

Of the births there were—

Legitimate children (born in wedlock)	22068
Illegitimate (born out of wedlock)	10823

Deaths.

Males	15822
Females	14266

Excess of deaths of males 1556

Of these, 564 bodies were exposed at the Morgue,—namely, 51 females, and 513 males.

HONOURS CONFERRED ON DR. HASTINGS AND CARSWELL.

AT the levee held on the 3d instant, Her Majesty conferred the honour of knighthood on Dr. Hastings, of Worcester, well known as the founder and President of the Provincial Association; and on Dr. Carswell, formerly Professor of Pathological Anatomy in University College, and now Physician to Leopold, King of the Belgians. Such honours are so rarely conferred in this country on medical men, or men of science, that this is an event which deserves to be recorded. There are some other professional men whom we could name, who, by the distinguished position which they have won for themselves in the ranks of the profession, are deserving of the honour of a baronetcy, or of knighthood.

VIOLENT CRAMPS CAUSED BY GREEN TEA.

A YOUNG man, twenty-eight years of age, of intemperate habits, took one evening, after being fatigued, as much as a quart of strong green tea, which he drank in cups one after the other. After several hours of disturbed sleep, his skin was covered with a profuse perspiration, and he exhibited symptoms of congestion of the brain. His pulse was frequent, but soft. Suddenly severe and fearful paroxysms of general convulsions, and tonic cramps, occurred. Consciousness was lost; the patient tossed about, tore his bedding, and gnashed his teeth. Opisthotonos, emprosthotonos, and trismus, alternated. Scarcely did one paroxysm subside before another followed. This state lasted three days, and was succeeded by a long and severe attack of delirium tremens, from which, however, he recovered. —*Casper's Wochenschrift.*

THE GLASGOW FACULTY.

An Act for better regulating the Privileges of the Faculty of Physicians and Surgeons of Glasgow, and amending their Charter of Incorporation. 13 Vic. c. 20.

WHEREAS the Faculty of Physicians and Surgeons of Glasgow were incorporated by royal charter granted by His Majesty King James the Sixth of Scotland, under the Privy Seal of that kingdom, on the twenty-ninth day of November, one thousand five hundred and ninety-nine, which Charter was ratified by an act of the Scottish Parliament passed upon the eleventh day of September, one thousand six hundred and seventy-two. And whereas by the said Charter the said Faculty were empowered to call before them and examine all persons practising surgery within the city of Glasgow, and the counties of Lanark, Renfrew, Dumbarton, and Ayr; to admit and grant licenses to such of the said persons as they should find qualified; and to debar all others from exercising the profession of surgery within the limits aforesaid; which powers the said Faculty have from the date of the said Charter exercised, and still enjoy. And whereas the city of Glasgow, and the said counties of Lanark, Renfrew, Dumbarton, and Ayr, over which the privileges of the said Faculty extend, comprehend a populous, wealthy, and important district of Scotland. And whereas it would be of advantage to the public, and also to the medical profession, if the exclusive privileges enjoyed by the said Faculty were so relaxed and amended that all persons found qualified and licensed to practise surgery by any corporation authorised by law to grant such licenses might have right to practise within the said district, and the right of all persons found qualified and licensed by the said Faculty to practise beyond the said limits were better defined, and if the members of the said Faculty were hereafter designated "Fellows of the Faculty of Physicians and Surgeons of Glasgow." And whereas the said Faculty have raised and established a fund of provision for the widows and children of the members thereof, and it has hitherto been considered obligatory upon all persons becoming Members of the said Faculty to become also contributors to the said fund; and it is expedient that such obligations should cease; but these objects cannot be effected without the authority of Parliament. May it therefore please Your Majesty that it may be enacted; and be it enacted by the Queen's most Excellent Majesty, by and with the advice and consent of the Lords Spiritual and Temporal, and Commons, in this present Parliament assembled, and by the authority of the same, that the present Members of the said

Faculty of Physicians and Surgeons of Glasgow, and such persons as shall be hereafter admitted into the said corporation, shall be known by the name and style of "Fellows" thereof; and shall, except in so far as hereby otherwise provided, possess, exercise, and enjoy the same powers and privileges as those heretofore possessed, exercised, and enjoyed by the Members of the said Faculty.

II. And be it enacted, that the Fellows and Licentiates of the said Faculty shall respectively enjoy the same status and privileges in the practice of their profession, and be equally eligible to the same offices in connection therewith, throughout Her Majesty's dominions, as if the said Faculty had been specially authorised by law to grant licenses or diplomas in surgery conferring the same status and privileges as those conferred by any other corporation or royal College in Scotland which now is or may hereafter be authorised by law to grant such licenses or diplomas: provided always that nothing herein contained or authorised shall interfere with any exclusive privileges heretofore granted by competent authority to any other corporation or royal College, so far and so long as such exclusive privileges remain in force and unrepealed.

III. And be it enacted, that the Fellows and Members, or Licentiates, respectively, of any other corporation or royal College which now is or hereafter may be authorised by law to grant licenses or diplomas in surgery, shall, within the city of Glasgow, and counties of Lanark, Renfrew, Dumbarton, and Ayr, enjoy the same status and privileges in the practice of their profession, and be equally eligible to the same offices in connection therewith, as the Fellows and Licentiates respectively of the Faculty of Physicians and Surgeons of Glasgow.

IV. And be it enacted, that no person who shall hereafter be admitted a Fellow of the Faculty of Physicians and Surgeons of Glasgow shall be obliged to become a contributor to the fund raised and established by the said Faculty for the widows and children of the Members thereof as aforesaid; nor shall any such person, or the widow or children of any such person, have any interest in the said fund, unless such person shall voluntarily become a contributor thereto, according to the regulations thereof in force for the time being.

V. And be it enacted, that neither the Faculty of Physicians and Surgeons of Glasgow, nor its office-bearers, nor the Fellows of the said Faculty, in consequence of their admission into the said corporation, shall have any claim to or interest in any part of the said fund, as presently vested in or under the management of the trustees

thereof or contributors thereto; but the same shall remain the sole property, and be under the exclusive management and control, of the contributors to and trustees of the said fund for the time being; and the said trustees shall have full power to demand, sue for, uplift, and discharge all sums owing to or invested for behoof of the said fund, without the interference or concurrence of the said Faculty, or its office-bearers or Fellows, and to manage, and from time to time to re-invest the same in the name of the trustees for the time being of the said fund, for behoof thereof.

VI. And be it enacted, that nothing herein contained shall prevent the Faculty of Physicians and Surgeons of Glasgow from altering the rules and regulations heretofore made by the said Faculty under the powers contained in the said Charter, or from making such new rules and regulations as may be necessary for carrying into effect the purposes of the said Charter and of this act; provided always that such new or altered rules and regulations shall not be inconsistent with this act or with the laws of the realm.

VII. And be it enacted, that this act shall commence and take effect from and after the passing thereof.

VIII. And be it enacted, that this act shall be a public act, and shall be judicially taken notice of as such.

CASE OF THE LATE SIR R. PEEL.

It was ascertained, on a slight examination, that there was a comminuted fracture of the left clavicle. Our contemporary, the *Lancet*, states that a swelling as large as the hand was subsequently formed below the fractured clavicle, which pulsed to the touch synchronously with the action of the heart. When examined carefully by the eye, it was found that the movement of this tumor corresponded with the contractions of the auricle, and was, in some respects, similar to the pulsations observed in the veins of the neck in very thin persons, and in certain forms of venous regurgitation. It was evident, from these signs, that some vein beneath the clavicle, probably the subclavian, had been wounded by the broken bone at the time of the fall; and that the subclavicular swelling consisted of blood effused from the wounded vessel. It was also evident that the swelling was in this way connected with the heart, forming what might be called a diffused false venous aneurism. The severe pain suffered by the deceased has been attributed to the rupture of some nerves connected with the axillary plexus. It might be accounted for, however, by the fractured rib (an accident discovered after death), and the general concussion pro-

duced by the fall. This must have been very severe in order to have produced a comminuted fracture of the clavicle. It is not improbable that there was injury to some of the internal organs. We are informed that there was complete suppression of urine during the two days which the deceased survived.

There is no ground whatever for supposing that Sir Robert fell from his horse owing to a sudden epileptic attack. All the facts concur to prove that the fall was the result of accident.

CAUSE OF THE GREEN COLOUR OF THE WATER OF THE SERPENTINE.

At a recent meeting of the Botanical Society of London, Dr. A. Hassall read a paper on this subject. In this communication it was shown that the periodical and coppery-green colouration of the water of the Serpentine is due to the presence of a minute plant belonging to the tribe of *Algae*, of which the writer gave a detailed and critical description, and which he named *Thompsoni*. The development of this plant takes place early in the spring, out of sight and at the bottom of the water, and it is only on the approach of the warm weather of summer that it diffuses itself through the water, deeply colouring it, and that part of it rises to the surface, forming a scum or pellicle of a bright æruginous or coppery-green colour. The whole of the water of the Serpentine is not usually coloured at one time, but different portions of it at different times, according to the strength and direction of the wind, which drives the plant before it. At one time it is found to collect at the Hyde Park extremity; at another it is present in the Kensington division; sometimes in the north and at others on the south shore—the remaining parts of the Serpentine being entirely free from the plant. This variable distribution which, unexplained, would be apt to occasion surprise, accounts for the fact that the observer may sometimes visit the Serpentine and not see a trace of the plant in question; and hence he might be led to form an erroneous conclusion as to the condition of the water. The observer, therefore, who wishes to come at its real state, should make the tour of the whole of the Serpentine. Considered in a sanitary point of view, Dr. Hassall is of opinion that the plant, when actually introduced into the system, as when swallowed in bathing, would not be productive of effects injurious to health, and regards it as a test of impurity and as an evidence of the very bad condition in which the water of the Serpentine now undoubtedly is. Dr. Hassall concluded the communication by observing that the colouration of large pieces of water by means of conferva is by no means un-

frequent, and cited as a remarkable instance of it the Red Sea, which derives its name and colour from the presence of a minute plant diffused through the water of a blood-red colour.

With due deference to the learned lecturer, the Red Sea is commonly considered to derive its name from "Edom, which signifies 'red.'" It was formerly called the sea of Edom. Travellers who have sailed down this Sea have assured us that there is nothing red about it.

THE MEDICAL PROFESSION AND PAWNBROKERS.

It is not commonly known that the three gilt balls displayed by pawnbrokers as a sign of their craft, are three gilt boluses, the heraldic distinction of the celebrated Medici family of Florence.

The founder of the Medici family was of the medical profession, and probably took his name from this, as the progenitors of the Smiths, Coopers, Bakers, and Turners of our own country took their names from their respective trades. The first members of the Medici family added to their gains by usury: they thus acquired wealth, and rose to distinction. They subsequently threw off the profession, but the family escutcheon retained the boluses; and this symbol of usury and money-lending is now considered an indispensable sign of the pawnbroker's trade.

IRISH LUNATIC ASYLUMS.

ACCORDING to a return to Parliament (just issued) there are 329 governors of lunatic asylums in Ireland. There are 426 officers and servants belonging to the district lunatic asylums, whose annual pay amounts to £8,994. 17s. 9d. In three years, ending the 31st December last, there were 674 urgent cases admitted. The total number of patients in the period was 7,320: the number of curable cases was 2,938, and of incurable cases 4,382.

ROYAL FREE HOSPITAL.

At the quarterly meeting of the governors of the Royal Free Hospital, on the 10th inst., the report stated that during the previous three months 183 in-patients, and 6889 out-patients, had been relieved. Regret was expressed that one of the large wards, containing fifty beds, was still unavoidably closed from want of funds.

DERBY COUNTY ASYLUM.

DR. HITCHMAN, of the Hanwell Lunatic Asylum, has just been appointed Superintendent Physician of the newly erected Derby County Asylum.

PROFESSORSHIP OF SURGERY AT UNIVERSITY COLLEGE.

THE Council of University College have conferred the office of Professor of Sur-

gery and Surgeon to the Hospital, rendered vacant by the resignation of Mr. Arnott, on Mr. J. E. Erichsen, assistant-surgeon to the hospital.

ELECTION OF FELLOWS AT THE ROYAL COLLEGE OF PHYSICIANS.

THE following gentlemen have been recently elected Fellows:—Dr. Basham, Dr. Peacock, Dr. Herbert Davies, Dr. G. Johnson, Dr. Acland (Oxford), and Dr. Ormerod. The censors chosen for the year are—Dr. Mayo, Dr. Barker, Dr. Barlow, and Dr. Jeaffreson.

ROYAL COLLEGE OF PHYSICIANS—LICENTIATES ADMITTED JUNE 1880.

At the last quarterly examination held at the Royal College of Physicians, the following gentlemen were admitted licentiates:—Drs. John Anthony, Spencer Pratt, Tyler Smith, John Snow, and William Wood.

COLLEGE OF SURGEONS—ELECTION OF MEMBERS OF COUNCIL.

THE annual election of members of the Council of this institution took place on the 4th inst., in the theatre of the College, when a large assemblage of Fellows attended from all parts of the country, for the purpose of electing from among their own body three gentlemen to the vacant seats in the Council, occasioned respectively by the death of Mr. Andrews, the resignation of Mr. Grainger, and the retirement in rotation of Mr. Pilcher. The President having taken the chair, delivered an address to the Fellows on the importance of electing only those gentlemen who were calculated to uphold the honour and dignity of the institution. The election was by ballot; at its conclusion the President, Professor Joseph Henry Green, declared that Messrs. George Pilcher, Gilbert Wakefield Mackmurdo, and Francis Kiernan, were duly elected members of the Council. The votes were distributed as follows:—

	Ayes.	Noes.
Mr. Pilcher . . .	74	61
Mr. Mackmurdo . .	107	16
Mr. Kiernan . . .	106	15

Messrs. Percy and Simpson were first nominated, according to the College regulations, in place of Messrs. Mackmurdo and Kiernan. The votes were as follows:—

	Ayes.	Noes.
Mr. Percy . . .	60	73
Mr. Simpson . . .	25	108

ROYAL COLLEGE OF SURGEONS.

GENTLEMEN admitted members on the 28th ult.:—B. J. Wetherell—S. Beswick—J. Furse—G. F. A. Drew—J. Marsh—R. D. Kidd—J. H. Parkinson—J. G. Kent—C. E. Playfair—E. M'Sorley—T. Hardinge—C. R. Thompson—C. Purdey.

Admitted on the 5th inst.:—H. Madge—J. H. Trounce—H. H. Massey—W.

Adams—G. Pound—R. Fowler—A. Forster—J. Y. Bown—R. McCormick—G. W. New—R. Neale—J. R. Reynolds.

Admitted on the 9th inst.:—C. Warden—J. H. Seville—T. L. B. Barwis—T. H. S. Pullin—H. Manley—H. E. Fox—H. T. Cornelius—J. Higgs—C. W. Hammond—T. Clarke—E. J. Blyth.

APOTHECARIES' HALL.

NAMES of gentlemen who passed their examination in the science and practice of medicine, and received certificates to practise, on Thursday, 4th July, 1850:—Thomas Clarke, Bradford, Yorkshire—John Craven, Dockroyd, Keighley, Yorkshire—David Grant McPherson, Bristol.

OBITUARY.

On the 4th inst., at Charing, Kent, after an active and useful life, respected by all who knew him, Thomas Prescott Wilks, Esq., surgeon, in the 79th year of his age.

PREVENTION OF PHTHISIS.

ONE of the most important means of preventing the development of phthisis is the free admission of air into dwellings. Our state governments should prohibit the erection of buildings that do not meet the requirements for the maintenance of a healthy race of operatives. Not only their own interests, but those of other classes, reacted upon by them, demand such a prohibition. It has been recommended that those of the poor who marry—and the same advice might be given to others—should select habitations that contain apartments which are large, and to which the air has free access. A vigorous offspring cannot be had by unhealthy parents. The progeny may be numerous, but will be unhealthy. The early marriages in this country are a great evil. The mother, for the most part with a fragile constitution, is worn out by numerous births; and, by the care and attention given to the children, becomes a mere drudge. In society she is rarely seen, and her influence is unfelt—she too often in a few years exhibits the marks of premature old age.—*Dr. Hallowell*, in *American Journal of Medical Sciences*, Jan. 1850.

BOOKS & PERIODICALS RECEIVED FOR REVIEW.

Cases of the Cure of Consumption and Indigestion. By G. Galvert Holland, M.D. Eighteenth Annual Report of the Trustees of the Perkins Institution and Massachusetts Asylum for the Blind.

Second Report on Idiocy. By Dr. S. G. Howe, Massachusetts.

Gout: its Causes, Cure, and Prevention, &c. By Abraham Toulmin, M.D.

Boston Medical and Surgical Journal. June 1850.

Parish Maps, and other Matters. By C. R. Walsh, M.R.C.S.

Comparative View of the different Anæsthetic Agents. By G. Hayward, M.D. Comptes Rendus. Nos. 24 and 25; 17 and 20 June.

The Chrono-Thermalist. No. 5; July.

BIRTHS & DEATHS IN THE METROPOLIS

During the Week ending Saturday, July 6.

BIRTHS.	DEATHS.
Males... 640	Males... 406
Females... 607	Females... 388
1847	794

CAUSES OF DEATH.

ALL CAUSES	794
SPECIFIED CAUSES	791
1. Zymotic (or Epidemic, Endemic, Contagious) Diseases...	169
<i>Sporadic Diseases, viz.:</i>	
1. Dropsy, Cancer, &c.	38
2. Brain, Spinal Marrow, Nerves, and Senses	112
4. Heart and Bloodvessels	38
5. Lungs and organs of Respiration	70
6. Stomach, Liver, &c.	37
7. Diseases of the Kidneys, &c.	9
8. Childbirth, Diseases of Uterus, &c.	8
9. Rheumatism, Diseases of Bones, Joints, &c.	10
10. Skin	6
11. Old Age	30
12. Sudden Deaths	7
13. Violence, Privation, Cold, &c.	25

The following is a selection of the numbers of Deaths from the most important special causes.

Small-pox	6	Convulsions	38
Measles	17	Bronchitis	31
Scarlatina	20	Pneumonia	28
Hooping-cough	28	Phthisis	121
Diarrhoea	40	Lungs	2
Cholera	1	Teething	7
Typhus	35	Stomach	3
Dropsy	12	Liver	11
Hydrocephalus	25	Childbirth	6
Apoplexy	20	Uterus	2
Paralysis	19		

REMARKS.—The total number of deaths was 83 below the average mortality of the twenty-seventh week of ten previous years. The only circumstance calling for notice is the progressive increase of deaths from diarrhoea. The deaths from this cause were 40, while the decennial weekly average is 24.

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer 29.78
 Thermometer 59.6
 Self-registering do. Max. 85.2 Min. 37.
 * From 12 observations daily. * Sun.

RAIN, in inches, 0.72.—Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was 1° above the mean of the month.

NOTICES TO CORRESPONDENTS.

Communications have been received from Dr. Robinson, Newcastle; Dr. Pearson; Mr. Rose; Mr. Jennette; Dr. J. C. Warren, U.S.; Mr. Galloway; and Dr. Barclay.

The papers of Mr. Marson, Mr. Smith, and Mr. Moore, next week.

We are again compelled to postpone Mr. Harvey's Obstetric Cases. They will be published in the following number.

Lectures.

LECTURES
ON INFLAMMATION,
(Delivered in the Theatre of the Royal
College of Surgeons of England).

BY JAMES PAGET,
Professor of Anatomy and Surgery to the
College.

LECTURE VI.

Nature and Causes of Inflammation. Limitation of the term 'increased action' as applied to the process of inflammation. Indications of defective nutrition, and defective exercise of the vital forces; in the effects described in the preceding lecture, and in the low organic characters of the inflammatory products, and in the conditions preceding the process. Peculiarities by which the inflammatory mode of nutrition is generally distinguished from hypertrophy, atrophy, and the production of new growths, such as tumors.

Proximate causes of inflammation traced to qualitative changes of the necessary conditions of normal nutrition; as, 1. of the Blood-vessels: 2. of the Blood; especially inoculable diseases, and specific inflammations; explanation of the localization of a general disease of the blood: 3. of the Nervous force, as affecting the size of the blood-vessels, and as having a share in the plastic forces: 4. of the proper tissues of the inflamed part.

An examination of the nature of the process of inflammation may best be made in the form of a comparison of its effects with those of the normal process of nutrition. And this comparison may be drawn with two principal views; namely, to determine, 1st—how the effects of inflammation differ, in respect of quantity, from those of the normal process; and 2nd, how they differ from the same, in respect of quality or method.

The decision on the first of these points may seem to be given in the term 'increased action,' which is commonly used, as synonymous with inflammation. As used by Mr. Hunter, this term was meant to imply that the small vessels of an inflamed part are more than naturally active, in formation or absorption, or in both these processes. This is, probably, the meaning

still generally attached to the term by some; while, as employed by those who believe the vessels are only accessories in the work of nutrition, the expression 'increased action' may be used to imply merely increased formation, or increased absorption. In either, or in any, meaning, however, the term seems to involve the idea of an increased exercise of the vital forces, i. e. of those forces through the operation of which the various acts of organic formation are accomplished. But, if 'increased action' is to imply this, the description of the process and effects of inflammation shows that the term cannot be properly used, without some limit or qualification.

If we consider the quantity of organic formation effected during the inflammatory process, in the proper substance of the inflamed part, it is evidently diminished. All the changes described in the last lecture are signs or results of diminished or suspended nutrition in the tissues of the inflamed part: they are all characteristic of atrophy, degeneration, or death. The tissues become soft or quite disorganized; they are relaxed and weakened; they degenerate, and remain lowered at once in structure, chemical composition, and functional power; or else, after degeneration, they are absorbed, or are disintegrated, or dissolved, and cast out; they die in particles or in the mass. During all the process of inflammation, there is no such thing as an increased formation of the natural structures of the inflamed part; they are not even maintained; their nutrition is always impaired, or quite suspended. It is only after the inflammation has ceased that there is an appearance of increased formation in some of the lowly organised tissues, as the bones and cellular tissue.

So far, then, as the proper substance of the inflamed part is concerned, there appears to be decreased action; that is, decreased formation. There may be, indeed, an increased absorption; but this is also, in one sense, characteristic of decreased exercise of vital force; since all absorption implies a previous degeneration of the part absorbed. Nor can we justly call this, in any sense, 'increased action,' till we can show how absorption is an act of vessels.

On the whole then, we may conclude, thus far, that one of the constituents of the inflammatory process, one of the characters in which it differs, in respect of quantity, from normal nutrition, is a defect in the nutrition of the proper substance of the inflamed part.*

* This has been clearly maintained by Dr. Carpenter. See Br. and For. Med. Rev. July, 1844.

But it is characteristic of inflammation, that, while the inflamed part itself suffers deterioration, there is a production of material which may be organized. Here, therefore, may be an evidence of increased formation, of increased action.

Doubtless, in relation to the productive part of the inflammatory process, the expression 'increased action' may be in some sense justly used; for the weight of an inflamed part, or of the material separated from it, may be much increased by the formation of organized matter. But the quantity of organized matter formed in an inflammation must not be unconditionally taken as the measure of increase in the exercise of the vital forces: for it is to be observed, that the material formed presents only the lowest grades of organization, and that it is not capable of development, but rather tends to degeneration, so long as the inflammation lasts.

It may be but a vague estimate that we can make of the amount of vital force exercised in any act of formation; yet we may be sure that a comparatively small amount is sufficient for the production of low organisms, such as are the fibrinous and corpuscular lymphs of inflammation. The abundant production of lowly organised structures is one of the features of the life of the lowest creatures, in both the vegetable and animal kingdoms. And, in our own cases, a corresponding abundant production is often noticed in the lowest states of vital force; witness the final inflammations, so frequent in the last stages of granular degeneration of the kidneys, of phthisis, of cancer, and other exhausting diseases. In all these, even large quantities of the lowly organised cells of inflammatory lymph may be formed, when life is at its last ebb. And with these cases correspond those that show the most rapid increase of tubercle and cancer, and other lowly organised tumors, when the health is most enfeebled, and when the blood and all the natural structures are wasting.

From these considerations, we may conclude that the productive part of the inflammatory process is not declaratory of the exercise of a large amount of vital, or organising, force; and this conclusion is confirmed by observing that development, which always requires the highest and most favoured exercise of the powers of organic life, does not occur while inflammation lasts. The general conclusions, therefore, may be, as well from the productive, as from the destructive, effects of the inflammatory process, that it is accomplished with small expenditure of vital force, and that even when large quantities of lymph are formed, such an expression as "increased action" cannot be safely used, unless we can be sure that the

defect of the formative power exercised in the proper tissue of the inflamed part is more than counterbalanced by the excess of power manifested in the production and low organisation of lymph.

It may be said that the signs of inflammation are signs of increased action. But these are fallacious, if, again, by increased action be meant any increased exercise of vital force. The redness and the swelling of an inflamed part declare the presence of more blood; but this blood moves slowly; and it is a quick renewal of blood, rather than a large quantity at any time in a part, that is significant of active life. An abundance of blood, with slow movement of it, is in no case characteristic of activity in a part; it more often implies the contrary, as in the erectile tissues, and the cancellous tissue of bones.

The sign of heat in the inflamed part is equally fallacious. The source of the locally increased heat cannot, I believe, be satisfactorily explained. This phenomenon of inflammation is involved in the same difficulty as are all those that concern the local variations of temperature in the body: difficulties which the doctrines of Liebig, however good for the general production of heat, are quite unable to explain. But, from the fact that the general supply of heat in our bodies is derived from oxydation or combustion of wasted tissues or of surplus food, we may assume that, in local augmentation of heat, the source is rather from some similar destruction of organic substances, than from increased formation of them. This can, indeed, be only assumed; but, if there be little evidence for it, there is as little for any assumption that the increased heat of an inflamed part is an indication of an increased formative action. The full heat of an actively growing part may be compared with the high temperature of one which is the seat of "determination" of blood, or of "active congestion;" for, in both cases, the heat is high because the blood, brought quickly from the heart, is quickly removed; but, in an inflamed part, the blood is not so renewed; it moves more slowly.

In thus endeavouring to estimate the difference between the normal and the inflammatory modes of nutrition, in regard to the quantity of formative or other vital force exercised in them respectively, I have also stated the chief differences between them in relation to the quality or method of nutrition. The most general peculiarity of the inflammatory method, in its simplest form, is the concurrence of these two distinct and independent, though usually coincident, events: namely, 1st, the impairment or suspension of the nutrition of the proper

substance of the inflamed part: and 2nd, the effusion, from the blood, of a material more than sufficient in quality for the nutrition of the part, but less than sufficient in its capacity of development.

The predominance of one or the other of these components is the ground of some of the chief varieties in the forms of inflammation; distinguishing, especially, the adhesive and suppurative inflammations, in which the formative part predominates, from the ulcerative and gangrenous, in which the destructive part prevails. But in all cases, the two components of the process are in certain measures combined, and their combination establishes the chief differences between the inflammatory and every other mode of nutrition in a part. Thus, from all the forms of mere atrophy or degeneration, the inflammatory process is distinguished by the production of the lymph, which may be organising, even while the proper tissue of the inflamed part is in process of atrophy, degeneration, or absorption. So far as the tissues inflamed are concerned, some inflammations might be classed with atrophies or degenerations; but the concurrent production of lymph is distinctive of them.

On the other side, the inflammatory mode of nutrition is distinguished from all the forms of hypertrophy by the failure of the nutrition of the inflamed part itself. So far as mere production and formation of organisms are concerned, some inflammations might be paralleled with hypertrophies; but the organisation of the lymph falls short of that proper to the part in which it is exuded; and the substance of the part, instead of being augmented, is only replaced by one of lower organisation.

And, lastly, from the production of new growths, such as tumors, the inflammatory process is distinguished by this;—that its organised products, though like natural tissues of the body, are usually infiltrated, fused, and interwoven into the textures of the inflamed parts; and that, when once their development is achieved, they have no tendency to increase in a greater ratio than the rest of the body.

I am well aware that these can be accepted as only the generally distinguishing characters of the simplest inflammatory process. Cases might be easily adduced in which the border-lines are obscured; inflammations confounded on one side with atrophies, on another with hypertrophies, on a third with tumors. But the same difficulties are in every department of our science; yet we must acknowledge the value of general distinctions among diseases even more alike than these are.

The case that I have chosen for illustrating the general nature of the inflammatory

process is one representing the disease in its simplest form and earliest stage, manifesting only the formation of lymph, and such a change as the softening or absorption of the inflamed part. This is but the beginning of the history; but, if the inflammation continues, or increases in severity, all that follows is consistent with this beginning; all displays the same double series of events, the same defective nutrition of the part, and the same production of low organisms. But these additions are observed; the part is more and more deteriorated, and perishes in the mass, or in minute fragments; the newly-organised products, not finding the necessary conditions of all nutrition, partake in the degenerative process, and, instead of being developed, are degenerated into pus, or some yet lower forms, or perish with the tissues in which they are imbedded.

Respecting, now, the causes of inflammation, I shall not say more of its exciting causes than that, from the external ones, which alone we can at all appreciate, we may derive a confirmation of the opinion I have expressed concerning the nature of the process. They are such as would be apt to produce depression of the vital forces in a part; all being, I think, such as, when applied with more severity, or for a longer time, will lead, not to inflammation, but to the death of the part. If a certain excess of heat will inflame, a certain yet greater heat will kill: if some violence will inflame, a greater violence will kill: if a diluted chemical agent will only irritate, the same concentrated will destroy the part. The same may be said, I think, of cold, mechanical injury, and all the other external exciting causes of inflammation. I am aware that other explanations of their action are given; but none seems to me so simple, or so consistent with the nature of the process that follows them, as this, which assumes that they all tend (as it may be said) to depress the vital forces exercised in the affected part: they may be stimulants or excitants of the sensitive nerves of the part, but they lead to the opposite of activity in its nutritive processes.

The proximate causes of inflammation appear to be various perversions of the necessary conditions of healthy nutrition in a part; that is, morbid changes in either the supply of blood, the composition of the blood, the influence of the nervous force, or the condition of the proper substance of the inflamed part. Any one or more of these four conditions of nutrition being changed in quality, the result appears to be an inflammation. A change in quantity more usually produces either an excess or deficiency of nutrition in the part, or

some process different from inflammation. Thus, a diminution or withdrawal of the blood, without alteration of its quality, is usually followed by atrophy, degeneration, or death: a mere increase of blood in a part may produce hypertrophy, or something more nearly resembling inflammation, yet falling short of it. Similar effects may ensue from a mere increase or decrease, or abstraction, of nervous force. Change in the quality, whether with or without one in the quantity, of the conditions of nutrition, appears essential to the production of the phenomena of inflammation.

I will endeavour now to show that inflammation may follow such perversion or qualitative change in each of the conditions of nutrition, even though all the rest of them remain in their normal state: selecting, for this purpose, such cases of inflammation as we may trace proceeding, in the first instance, from the uncomplicated error of a single condition of nutrition.

1st. Inflammation may perhaps be produced—it certainly may be in some measure imitated—by changes in the blood-vessels; changes attended with alteration of their size, or their permeability, or the other qualities by which they affect the supply of blood to a part. This may be concluded from the similarity to some of the phenomena of inflammation which may be observed in certain cases of mechanical obstruction to the venous circulation. In a case of ascites from diseased heart or liver, the peritoneum often contains coagula of fibrine floating free in the serum, though no organ may present appearances of having been inflamed. In such a case, moreover, I have found the fibrine developing itself in the form of nucleated blastema, even while floating free. In another case of mechanical dropsy, I have found the fluid of anasarca in the scrotum containing abundant lymph-corpuscles, like those in the fluid of an inflammatory effusion. Such as these are the cases through which mechanical congestions of blood connect themselves with inflammation. And if to these we add the constancy of increased vascularity among the phenomena of inflammation, they may be sufficient to make us believe, that disturbances in the circulation of a part may produce some of the principal phenomena of inflammation, even though all the other conditions of nutrition are, in the first instance, unchanged. But I know no other good evidence for the belief; and I think we should not lay much stress on these cases, since they display an imitation of only one part of the process of inflammation, namely, the production of organised matter. The nutrition of a part with obstructed circulation suffers but a

trivial disturbance, in comparison with that which would accompany an inflammation with an equal amount of hindrance to the movement of the blood. I should therefore be cautious of regarding these effusions in mechanical obstructions of blood as more than partial imitations of the inflammatory process. So far as the effusion in an inflammation depends on the altered mechanical relations of the blood and vessels of a part, so far may similar alterations produce effects imitating those of inflammation; but I much doubt whether any change whatever in the circulation of a part, however produced, can alone produce or alone maintain the phenomena or effects of inflammation. I believe that the disturbances of the circulation are no more adequate to the explanation of inflammation, than the normal movements of the blood are adequate to the explanation of the ordinary process of nutrition.

2. We may speak much less equivocally of the influence of the state of the blood itself in determining inflammations; for there can be little doubt that a very great majority of the so-called spontaneous or constitutional, as distinguished from traumatic, inflammations, have herein their origin. We might anticipate this from the consideration that, in normal nutrition, the principal factors are the tissues and the blood in their mutual relations: but we have better evidence than this, in cases of local inflammations occurring in consequence of general diseases of the blood. Some instances of this are clearly proved, as, *e. g.*, in the cases of eruptive fevers, when the presence of morbid materials in the blood is proved by the effects of their transference in inoculation. Scarcely less thoroughly demonstrated are the cases of rheumatism and gout, of lepra, psoriasis, herpes, eczema, erysipelas, and other such affections, whose constitutional nature—in other words, whose primary seat in the blood—all readily acknowledge in practice, if not in theory. Now, in all these cases, local inflammations are the external signs of the general affection of the blood: and I apprehend that if any difficulty be felt in receiving these as evidences that the morbid condition of the blood is the cause of the local inflammation, it will be through doubt whether a general disease of the blood—a disease affecting the blood sent to every part—can produce peculiar phenomena of disease in only certain small parts or organs. But this local effect of a general disease of blood has its illustration in some of the sure principles of physiology; especially in this—that the presence of certain materials in the blood may determine the formation of appropriate organisms, in

which they may be incorporated.* Thus, when one kidney is removed or destroyed, the other will acquire a greater size, sufficient for the discharge of the necessary quantity of urine. Now we know that the principal materials of urine exist ready-formed in the blood; that, being taken from the blood, they are incorporated in the secreting cells of the kidney, and are for a time enclosed in them, and enter into their composition; and that, finally, they are discharged into the excreting tubes from these cells, not by mere filtration. We may, therefore, safely hold that, when one kidney is lost or spoiled, more renal cells are formed in the other, *because* more of the constituents of urine are in the blood: in other words, that the presence of these constituents in the blood that is carried to every part determines the formation of appropriate organs in one part of the body; in which organs these constituents may be incorporated. Nor is this the only case. Certain medicines, especially diuretics, are separated from the blood by only certain organs; they must, for this separation, be embodied in those organs, and while embodied they may excite inflammation; as cantharides, turpentine, and the like, do. Abundant hydro-carbon principles in the blood will bring about the formation of abundant fat cells, in which they may be enclosed. The accumulation of sap in the branch of a tree that has been *ringed* will determine the organisation of abundant fruit; and many similar cases might be cited.

It is in exact parallel with these facts in physiology, that in certain general diseases of the blood, organs are formed, as the products of inflammation, within which the specific morbid material is incorporated. Thus, in small-pox, cow-pox, primary syphilis, and whatever other diseases may be transferred by inoculation, the morbid material from the blood is incorporated in the fluid and corpuscular products of inflammation, which are enclosed within the characteristic vesicle or pustule; just as, in the cases already cited, the constituents of urine or of medicines are incorporated in the renal cells, which are formed within the substance of the kidney;

* This principle was fully discussed in the Lectures on Nutrition delivered in 1847, and published in the *MEDICAL GAZETTE* of that year. And it appears to be capable of even much wider extension than was then assumed. For example, it supplies the best theoretic expression of the origin of cancerous, and similar tumors. The cells or the tissues of these growths, we may believe, are formed so as to comprise or enclose specific morbid materials generated or inserted in the blood. That each such morbid material should have, for its appropriate habitation, cells and a mass of peculiar form and appearance, is only consistent with normal rules of formation.

or just as the constituents of sap are incorporated in fruit.

In the cases of disease produced by a demonstrable virus, we have all the evidence that can be necessary to prove the principle I am contending for—namely, that a general disease of the blood may determine a local inflammation in one or more circumscribed portions of a tissue. And the analogy is so close, that I think we need not hesitate to receive the same explanation of other inflammations, which I have cited as occurring during morbid conditions of the blood. For although we cannot, by inoculation, prove that a specific morbid material of such a disease as herpes, or rheumatism, has been incorporated in the inflammatory products, yet we find great probability hereof in the many analogies which these diseases present to the inoculable diseases, in their whole history, and, especially, in the decrease of general illness which ensues on the full manifestation of the local inflammation.

If it be asked why a morbid material is determined to one part or tissue rather than another, or why, for example, the skin is the normal seat of inflammation in small-pox, the joints in rheumatism, and so on, I believe we must say that we are, on this point, in the same ignorance as we are concerning the reason why the materials of sweat are discharged at the skin, those of urine at the kidneys, of bile at the liver, or why the greater part of the fibrine is incorporated in the muscles, and of gelatine in the bones. We cannot tell why these things are so; yet we believe them, and our belief has practical advantages. So may the belief that a great majority of the so-called spontaneous local inflammations are the consequences and manifestations of certain morbid conditions of the whole mass of blood; and that when such conditions depend on the presence of any specific material in the blood, a portion, or the whole, of that material is usually incorporated in the products of the inflammation, and in them is separated from the blood.

But, again, it may be said, that if this be granted, still we need some explanation of the fact that the morbid condition of the blood does not influence the whole extent of any given tissue, but only portions of it. In the secretion of urine, it may be believed that the whole kidney is affected and works alike; but in the assumed separation of the virus of small-pox, only patches of the skin are the seats of pustules; in vaccinia and primary syphilis, only a single point; in secondary and tertiary syphilis, a certain, but often disorderly, succession of various parts, and so on.

It must be admitted that many of the facts here referred to cannot yet be ex-

plained; but such difficulty of explanation affords no warrant for a denial of the theory, especially since we are able, consistently with this theory, to point out some of the conditions that determine the locality in which a general disease of the blood will manifest itself by inflammation. In some instances, it is evident that the localisation of a general disease of the blood is determined by a previous condition, such as we may call a weakened or depressed condition—a state of already impaired nutrition—in some one part. For instance, suppose a stream of cold air is impelled on some part, say the shoulder, of a person disposed to rheumatism, it determines, as a more general exposure to cold might do in the same person, the rheumatic state of the blood, with all its general symptoms; but it determines, besides, the part in which that rheumatic state shall manifest itself first or alone. The depressed nutrition of the chilled shoulder makes it more liable than any other part to be the seat of inflammation excited by the diseased blood.

Or, again, when a virus is inserted, as in all cases of poisoned wounds, the local inflammation produced by the disease with which the whole blood is infected will commonly have its seat in the wounded part. The virus must have produced some change in the place in which it was inserted, as well as in the whole mass of the blood. These cases are, probably, only examples of the general rule, that a part whose natural force of nutrition is in any way depressed, will, more than a healthy part, be liable to become the seat of chief manifestation of a general blood-disease. Thus, a part that has been the seat of former disease or injury, and that has never recovered its vigour of nutrition, is always more liable than another to be the seat of local manifestation of blood-disease: it is, as they say, a *weak part*. Thus the old gouty or rheumatic joint is apt to receive the brunt of the new attack. And the same may happen in a more general way. A man was under my care with chronic inflammation of the synovial membrane of his knee, and general swelling about it: he was attacked with measles, and the eruption over the diseased knee was a diffused bright scarlet rash. A patient under Dr. Budd's care had small-pox soon after a fall on the nates: the pustules were thinly scattered everywhere, except in the seat of former injury, and on this they were crowded as thickly as possible. Thus, too, when a part has been injured, and it may be, is healing, a disease having begun in the blood will manifest itself in this part. Impetigo appears about blows and scratches in unhealthy children; erysipelas about the same in men with unhealthy blood. Such facts as

these appear to be sufficient evidences that morbid conditions of the blood are most probably the causes of the great majority of so-called spontaneous local inflammations; of such as cannot be traced to the direct influence of any external force, but appear, rather, as having an internal origin.

It may be added, that the state of the blood may determine not only the locality, but also the degree, and the form, and probable termination of the inflammation. But on this I need not dwell, having devoted a large portion of a former lecture to demonstrate that the products of even the same form of inflammation excited in the same tissue, may be different in different persons, according to the several peculiarities of their blood.

3. Respecting the disturbance of the third condition of healthy nutrition, namely, the due influence of the nervous force, as a constituent of the phenomena of inflammation, I have already spoken in a former lecture.* I now resume the subject, in the hope of showing that the disturbance of this condition may be one of the causes of inflammation.

To test the influence of the nervous force in engendering the inflammatory process, we must not, as is commonly done, take cases of the effects of external injury. Such an injury, or the presence of a foreign body, is supposed to excite inflammation by stimulating the nerves of the part, and by changing, through their influence, the state or action of the blood-vessels. This may be true; but we should remember that when a common injury is inflicted, it acts not only on the nerves of the part, but also on its proper tissues; and it may so affect the state of these tissues, that the changes produced in them may be the excitant of inflammation, independent of the affection of the nerves. All such cases as these are, thus, ambiguous. For a better test, we must select cases in which the excitant of inflammation acts (at least in the first instance) on the nervous system alone. Such cases are those already referred to. When the conjunctiva is inflamed after overworking of the eye, we cannot suppose that the light, by its direct contact, has affected the vessels, or the nutritive act, in the conjunctiva: it can, probably, affect either of these only through an influence reflected from the retina. So, when irritation of the urethra excites inflammation in the testicle; when the irritation of teething excites it in any distant part; when, as in a case quoted from Lallemand, by Dr. Williams, inflammation of the brain followed the application of a ligature to part of the brachial plexus;

* Lecture I. p. 12.

in these and the like cases—such as I mentioned in the first lecture,—we cannot but refer to the disturbance of the nervous force as the initiator of the phenomena of inflammation.

Now, for the explanation of such cases as these, there appear to be two chief theories—1. It may be that the nerves distributed to the minute blood-vessels of a part may be so affected that these vessels may dilate, and their dilatation may produce the other phenomena of inflammation; or, 2. The disturbance of the nervous force may more directly interfere with the process of nutrition, inasmuch as this force exercises always some influence in the nutrition of each part, and is (as one may say) one among the plasturgic forces.

The first of these theories has lately acquired a dominant place in systems of pathology, especially in those of Germany. The principal form of it, which has been maintained most prominently, by Henle, has enlisted the approval of even Rokitsansky, and is largely received, professing to explain all inflammations, and passing by the name of “neuro-pathological,” to distinguish it from the “humoral” and all other theories of inflammation. This theory may be thus briefly stated. The exciting cause of inflammation, whether an external cause, such as an injury of a part, or an internal one, such as diseased blood, acts, in the first instance, on the sensitive centripetal or afferent nerves of the part. These it affects as a stimulant, producing in them an excited state, which state, being conveyed to some nervous centre, is thence reflected on the centrifugal or motor nerves of the blood-vessels of the same, or some other related, part. This reflection, however, is supposed to bring about a kind of antagonistic sympathy, such that, instead of exciting the motor forces of the blood-vessels to make them contract, it paralyses them, and is followed by their dilatation or relaxation. This dilatation being established, the exudation, and other phenomena of inflammation, are assumed to follow as natural, and most of them as mechanical, consequences.

The eminence of those who have supported this hypothesis makes one hesitate in rejecting it; and yet I cannot help believing it to be groundless. If we remember that parts may present some of the phenomena of inflammation, though they have no nerves, as the firmest tendons and articular cartilages; that the degrees of inflammation in parts bear no proportion to the amounts of pain in them when inflamed; that the severest pains may endure for very long periods with only trivial, if any, phenomena of inflammation; that the

phenomena of the so-called reflex paralysis are rare, equivocal, and altogether insufficient for the foundation of a law or general principle; we may well think that there can be no sufficient ground for the invention of such an hypothesis as this. And, if we add that, even admitting the dilatation of bloodvessels as a possible consequence of the stimulus of sensitive nerves, yet the phenomena of even simple inflammation would be no necessary consequences thereof; that the varieties of inflammations would be quite unintelligible as results of similar mechanical disturbances of the circulation; and that the dilatation of bloodvessels, in any mechanical way produced, is followed by only feeble imitations of a part of the inflammatory process; then we may think that the hypothesis, if all its postulates be granted, will yet be insufficient for the explanation of the facts.

I believe that, if we would have any clear thoughts respecting the influence of the nerves in initiating inflammations, we must first receive the theory, referred to in the first lecture, that a certain exercise of the nervous force is habitually and directly engaged in the act of normal nutrition. If we admit this, there can be little difficulty in believing that the perturbations of the nervous force may engender the inflammatory mode of nutrition; especially when we see the normal and inflammatory modes connected through such intermediate instances as the increase of secretions when the nerves of a distant part are stimulated.

Now, that the nervous force has some other influence in normal nutrition than can be explained by referring to it only the government of the size of bloodvessels, we have, I think, ample evidence; and I cannot but wonder at the steadfastness with which some maintain or imply that the nervous force can manifest itself in nothing but impressions on the mind, and muscular contraction-force. So limited a view of the convertibility of nervous force, is such an one as the older electricians would have held, had they maintained that the only possible manifestations of electricity were the attractions and repulsions of light bodies, or that the electric force could never be made to appear in the form of magnetism, of chemical action, or of heat. We are too much shackled with these narrow dogmas of negation. The evidence of the correlation and mutual convertibility of the physical forces might lead us to anticipate a like variety of modes of manifestation for the nervous and other forces exercised in the living body.* We might

* For the suggestion I thank my esteemed friend whom I am glad to know correlated of the vital illustrated.

anticipate, too, that, as the nervous force has its origin in the acts of nutrition by which the nerve-substance is formed, so, by reciprocal action, its exercise might affect the nutritive acts. As (for illustration sake) the completed blood affects all the processes by which itself was formed, so, we might suppose, would the nervous force be able to affect all the acts of which itself is the highest product.

But we need not be content with these suppositions of the direct influence of the nervous force on the nutritive act, while we remember such facts as these. The mind can affect all nutrition, but it can do so only by affecting, in the first instance, the nervous force; and its effects are such changes of nutrition as cannot be referred to mere changes of the size of the blood-vessels. Abstraction of the nervous influence from a part by division, or other profound injury, of its nerves, is, as a general rule, followed by serious impairment of its nutrition, and by such and so various impairments as cannot be accounted for by paralysis of the bloodvessels. The direct influence of the nervous system on both the quantity and the quality of secretions is a fact even commonly noticeable; and secretion and nutrition are so similar processes that we may be sure they are accomplished by the same forces similarly exercised.*

We seem, then, to have sufficient evidence that the nervous force is one of those which (at least in the highest animals) are engaged in the normal act of nutrition; and sufficient evidence that inflammation may ensue in consequence of disturbance of the nervous force, even when that force cannot have acted in the first instance on the bloodvessels of the inflamed part. I think, therefore, the expression is justified, that the inflammation of a part may have its origin in disturbance of the nervous force which is normally exercised in the nutrition of the part, and which is exercised directly, not merely through the government of the contractile coats of the blood-vessels.

The last of the necessary conditions of normal nutrition in a part is the healthy condition of the part itself. If a part be at present diseased, it will be apt to remain so, because of the continuance of the same diseased method of nutrition. Now, it appears highly probable that a disturbance of the healthy state of a part may introduce the phenomena of inflammation. This is probable for many reasons; as, first, from analogy with normal nutri-

tion. Generally, the principal conditions of nutrition are the relative and mutual influences of the elements of the tissues and the blood. More particularly, the condition of the tissues determines, at least in great measure, both the quantity and the rate of movement of the blood supplied to them, the changes of the tissues, whether in growth or decrease, just preceding the adapted changes in the supply of blood. So we may believe a change in a part anyhow engendered may, by altering its relation to the blood, alter its mode of nutrition; and some of the changes may produce the inflammatory mode of nutrition, together with the altered supply of blood, and other characteristic signs.

Secondly, we may judge the same from the analogy between inflammation and the process of repair. Certainly it is the state of the injured part—i. e. of its proper tissues, not of its nerves and blood-vessels—which determines the process of repair: and some of the processes of repair are so like those of inflammation, that they are commonly identified, and are, perhaps, not capable of even a refined distinction.

And thirdly, the influence of the condition of the proper tissues of a part in initiating inflammation in it, is illustrated by more direct facts; such as, that injuries of parts that have no vessels or nerves are followed by altered modes of nutrition in them, these modes being more or less exact resemblances of inflammation. Thus, e. g. it is in the cornea, lens, vitreous humour, and the like, after injury.

On the whole, I think we may conclude that inflammation may have its origin in disturbance of the normal condition of the proper tissues of a part,—in such a disturbance as may be produced by injury, or by the proximity of disease. To this source, indeed, I should be disposed to refer nearly all inflammations that originate in the direct application of local stimuli, whether mechanical or chemical. It is true, that, in most cases, the stimulus affects at once the proper elements of the part, its nerves, and its bloodvessels, so that we cannot say how much of the disease is to be ascribed to the affection of each; but the fact that a process resembling, so far as it goes, that of inflammation may ensue after injury in parts that have neither vessels nor nerves, may make one believe that, in parts that have both, the inflammation depends mainly on injury, or other affection, of the proper tissue.

I have thus endeavoured to show that inflammation may take its rise, may have its proximate cause, in a disturbance of

* The subject is more fully illustrated in the Lectures on Nutrition, in the *MEDICAL GAZETTE* for 1847.

any one of the conditions of nutrition. In the examination of different cases, we find that, even while any three of the four chief conditions may be normal, yet a qualitative error of the fourth may bring in the phenomena of the inflammatory process and method of nutrition. In the necessity of choosing pointed cases, I may seem to have implied that it is usual for inflammation not only to begin, but to be maintained, by an error in one of the conditions of nutrition: but this is improbable. Rather we may believe, that many of the existents of inflammation may affect at once more than one of the conditions of nutrition; and, as I stated in the first lecture, it is nearly certain that in every inflammation, after a short continuance, all the conditions of the nutritive process are alike involved in error.

P.S.—I cannot but fear, lest, having seldom referred, in these Lectures, to the works of those who have written on Inflammation before me, I may seem unready to acknowledge my obligations to them, or desirous to receive unmerited praise for originality of observation or of thought. Nothing is further from my intention, or, I believe, from my custom. The truth is, when I began to prepare for the delivery of the lectures, it seemed impossible to study their subject-matter by both reading and personal investigation: its extent seemed almost equally boundless in books and in nature. I therefore determined to occupy my time almost exclusively with personal inquiries and reflections; and to add the results of these to whatever I found true in the knowledge previously acquired by reading or by earlier examinations. If I had had time to study the vast literature of inflammation, I would gladly have endeavoured to assign to its proper author whatever fact or opinion of any value these lectures may contain; but this was impossible: and now, though conscious of being largely indebted, I am quite unable to say to what creditor each debt is due. But I gratefully acknowledge that my greatest obligations were incurred, many years ago, in reading the works on Inflammation of Hunter, Lawrence, Tweedie, James, and Macartney; and that, more recently, I have borrowed both facts and suggestions from the writings of Mr. Travers, Dr. Carpenter, Dr. C. J. B. Williams, and my valued friend and former pupil Mr. Humphry. Nor are my obligations less to Rokitsansky, Henle, Virchow, Reinhardt, and, it may be, to many others from whom I may have derived such knowledge that I ought never to have forgotten whence it came.

CASE OF MOLLUSCUM FUNGÖIDES, OR TUBERCULOSA STYPHILOIDEA. BY M. LUCIEN CORVISART.

THE patient, a female, thirty-one years of age, was admitted into the hospital, Beaujon, under the care of M. Huguier. Her general health had always been good, and she was the mother of three healthy infants. She had resided two years in Paris, in the most unfavourable hygienic circumstances—in a damp room, in the midst of rags and old clothes, in which she carried on a brokerage. In the month of April, 1848, there appeared on the left side of her neck a prominent bluish-red tumor, of firm consistence. When first noticed it was as large as a pea, and in the course of six weeks acquired the size of a bean. Three months afterwards, this tumor was destroyed with Vienna paste. Fifteen days afterwards, eight other tumors appeared on the abdomen, which, however, disappeared in a short time without treatment. Towards the end of September she was seized with paralysis of the right side of the face. In December, ulceration of another tumor on the loins occurred. Fresh tumors also shewed themselves on the legs, thighs, abdomen, chest, and arms. At the same time, numbness and partial paralysis of the right forearm supervened; and lastly, in the beginning of January 1849, a tumor appeared on the middle of the forehead.

When admitted into the hospital, this patient had upwards of a hundred tumors, varying in size from that of a pea to that of a large nut, and in colour from that of a rose tint to deep red, adhering by broad bases, and presenting on their summits a slight elevation of cuticle from beneath, which exuded a scanty yellowish plastic serosity. Some of these tumors were ulcerated, presenting a pale excavation, with depressed or elevated base, granular and firm, the edges generally everted: some were covered by a layer of plastic reddish lymph. Besides these, large whitish-opaque *bullæ* were seen on various parts of the body, as the ends of the fingers, on the inside of the thumb and index finger, and on the back of the wrist. These *bullæ* were surrounded with indurated inflamed bases, and contained pus.

The disease did not yield to remedies, and shortly the patient died.

The autopsy showed that the tumors were composed of fibro-plastic tissue, with great vascularity of the superficial laminae of the skin. Beneath the frontal tumor, the periosteum was thickened, and in one place removed, leaving the bone exposed. Ulcerations of the palate were found. Two fibrous tumors were found in the substance of the right ventricle of the heart, also several in the uterus and ovaries. The liver was fatty to a great degree. The os uteri rated.—*L'Union Médicale.*

Original Communications.

ON THE
INDUCTION OF PREMATURE
LABOUR;

WITH CASES.

BY GEORGE HARVEY, SURGEON,
Castle Heddingham, Essex.

ANY interference with the natural process of uterine gestation, more particularly any attempt to produce premature labour, must be considered unjustifiable, except under very peculiar circumstances, endangering the life both of the mother and child,—such as profuse uterine hæmorrhage occurring in the latter months of pregnancy, or the previous knowledge of such deformity of the bones of the pelvis that a fœtus at the full period could not possibly pass entire. Under these circumstances an attempt to produce premature labour may be not only justifiable but advantageous, as I hope the following cases will clearly prove: but it must be confessed that, after having used all available means, we cannot possibly predict the time at which labour will commence.

CASE I.—On Saturday, April 11th, 1846, at about 8 o'clock p.m., I was summoned to Mrs. H—, who was that evening suddenly attacked with hæmorrhage from the uterus to a large amount (probably two pints). Has met with no accident; was previously in good health, and knows no cause for this occurrence. Considers herself wanting full six weeks of her time. Suffers very little pain. Bowels costive; pulse weak and fluttering.—*R. Magnes. Sulph. ʒij.; Infus. Rosæ (c. duplici portione Acidi), ʒvj.; sumat cochl. maj. ij. 4tis q. h. To be kept cool, and in a recumbent posture.*

12th, 10 o'clock p.m.—Hæmorrhage ceased; no pain; bowels relieved twice during the night: says she can feel the movements of the child.—*Rep. Mist. sine Magnes. Sulph.*

April 13th.—No hæmorrhage; more cheerful: says she can feel the child. I thought I could hear the child's heart by the stethoscope just above the crista ili, on the right side, but am not certain.

15th.—Again summoned at 6 o'clock this morning. Hæmorrhage returned to an alarming amount. No pain; very faint: says she can feel the child. On examination per vaginam, I found the os uteri very high up, and undilated, the cervix about half an inch in length. However, I succeeded in inserting the tip of my forefinger just within the entrance of the os uteri; then passing a flexible female catheter, with a perforation at its extremity, along my finger, it readily entered the cavity of the uterus. I now passed through the catheter a stilette with a sharp point, 1-8th of an inch longer than the catheter, and punctured the membranes: a large quantity of liquor amnii flowed readily through the instrument, and the hæmorrhage almost immediately ceased. I then gave her a scruple of ergot of rye, which I repeated at 8 o'clock, no perceptible effect having been produced. 10 o'clock.—Very comfortable: no hæmorrhage. Liquor amnii discharged at intervals. Slight pain in the abdomen and loins. Os uteri in the same state: ordered to remain in bed and be kept quiet.—*R. Pulv. Secale, ʒj.; Spt. Ether. Nitrici, ʒij.; Sacch. albi, ʒj.; Aquæ, ʒvj. Mix. Sumat cochl. maj. ij. 4ta q. hora.* 2 o'clock p.m.—Slight pains in the loins and abdomen: no hæmorrhage. I can perceive with the stethoscope a confused murmur on the right side near the crista ili, and an obscure pulsation synchronous with the pulse at the wrist, probably the iliac artery; but nothing of the kind perceptible on the left side. 10 o'clock p.m.—Slight pains still continue at intervals, but no hæmorrhage. Os uteri in the same state. Complains much of faintness, and wishes for a little beer, which I have permitted.

16th.—Very comfortable; rested well last night; took half a pint of beer, and some bread and cheese, with great relish. Very desirous of getting up, which I have forbidden. Os uteri remains the same. Still slight pains, accompanied with a discharge of liquor amnii. The hæmorrhage having ceased, I have discontinued the ergot, lest the constant contraction of the uterus should destroy the child, which the mother is still confident of feeling: to have broth and gruel, a little beer, but no medicine.

17th, 10 o'clock a.m.—Pains increased in strength: no hæmorrhage. Os uteri soft and slightly dilated. Presentation apparently the head. No attachment

of the placenta discoverable: but I did not interfere much, lest I should do mischief. 2 o'clock p.m.—Labour proceeding slowly: os uteri moderately dilated; presentation natural. Let well alone. 7 o'clock p.m.—A messenger to say that the pains were very severe. Found the os uteri fully dilated, but no attachment of the placenta to be detected. At 8h. 30m. a male child was born, which immediately cried lustily. After a few minutes, the pain returning, I grasped the uterus through the abdominal parietes, and the placenta was speedily expelled, without any traction by the cord, and without any hæmorrhage beyond what is usual.

On carefully examining the placenta, I found the cord inserted about an inch from its edge. At this part, and on either side, the chorion was separated from the edge of the placenta nearly half its circumference, and in width about half an inch. In this part there were numerous coagula; but I could discern no evidence of the spongy portion having been separated from the uterus previous to the birth of the child. I was unable to obtain possession of the placenta, which I was desirous of preserving. Did the hæmorrhage proceed from that part of the placenta from which the chorion was separated?

18th, 10 o'clock a.m.—Mother and child quite comfortable. Ordered the child to be put to the breast. The recovery of the mother was speedy and complete.

CASE II.—On the 20th of June, 1847, the mother of L. C. called to inform me that her daughter, whom I had delivered with the crotchet rather more than a twelvemonth since, was again pregnant, and requested my attendance. She could not say when it might be expected, having never menstruated since her last delivery, but thought she must be more than half her time. I now informed both her mother and herself that the difficulty experienced in her last labour arose partly from the large size of the child, but principally from the unfortunate deformity of her own person (a very short woman, with the promontory of the sacrum projecting to little more than two inches of the pubis). I therefore advised her to allow me to induce premature labour two months before the full period: to this she made not the slightest objection.

I then desired her to endeavour to recollect the time at which she had quickened, informing her that twenty weeks from that time would amount to the full period, and that I wished to bring on labour eight weeks before the regular time.

August 7th.—Being in attendance on a neighbour, I called on her, when she informed me that she was very uncomfortable, on account of the great size and weight of her body, and that she considered she had barely two months before her full period would be completed. This was about noon. I promised to see her in the evening, and desired she would summon her mother, and have everything prepared, as I should endeavour to bring on labour, with the hope of saving the child, and without risk to herself. 5 o'clock p.m.—Found no difficulty in reaching the os uteri with the forefinger of my left hand, on which I introduced a flexible female catheter, perforated at its extremity, and passed it fairly through the os uteri till it met with some resistance. I then passed a stilette, and the liquor amnii flowed freely through the catheter. I desired her to remain in bed, and to send to me when labour pains commenced. 11 o'clock p.m.—Her mother called to inform me that she had felt a few very slight pains, and that the waters were constantly dribbling away. Desired to be called if the pains increased either in strength or frequency.

8th, 12h. 30m. noon.—Pains rather more frequent, but very slight.—Secale Cornuti, \mathfrak{ss} . statim. 7 o'clock p.m.—Pains stronger; os uteri fairly dilated; head presenting, but advances very slowly. Half-past ten: delivery completed 29 hours and a half after puncturing the membranes. Child, a son—still-born; several patches of ecchymosis on different parts of its body, particularly one extending over the whole of the right shoulder, and another covering the entire scalp. This I consider to have been caused by the pressure of the uterus after the evacuation of the liquor amnii; and this pressure, although the pains were never violent, may have caused the death of the fœtus, which does not appear to have advanced beyond the sixth month; therefore the woman must have made an erroneous calculation. I would not, except in cases of hæmorrhage, or some other extraordinary emergency, advise premature

delivery earlier than the thirty-second week of utero-gestation, which was the time represented to me in the present instance.

CASE III. — December 24th, 1849. L. C. this morning called to inform me that she was again pregnant, and in her twenty-seventh week: that she was confident of the correctness of her calculation, and begged that I would again deliver her before the full time. I enjoined her to pay strict attention to the state of her bowels, and to let me know if at any time she had pain or uneasiness, and desired her to have everything prepared, as I intended to be with her eight weeks before the full period.

Thursday, January 24, 1850. — I visited her this morning, informed her that I intended to be with her on the following Monday, January 28, gave her some aperient medicine, and engaged her husband's mother to be in attendance.

28th. — On examining per vaginam I could just reach the os uteri, but not sufficiently to introduce a catheter with certainty; therefore gave her *Secale Cornuti*, ℞j. at 12 o'clock. 5 o'clock P.M. — Has had slight pain in the abdomen, and vomited twice. I can now reach the os uteri with ease, and therefore introduced the catheter and punctured the membranes, as in her former labour. Desired her to remain in bed, and send for me if the pains increased.

29th, 9 o'clock A.M. — But little increase of pain. Liquor amnii has been freely discharged. Os uteri soft, but very slightly dilated. 5 o'clock P.M. — Much the same. Resolved not to repeat the ergot; as in her former labour I suspected it had, by exciting the uterine action, caused the bruises and consequent death of the child: the dose given yesterday, with the view of dilating the os uteri and bringing it within reach, could have no such effect, as the membranes were not then ruptured.

30th, 11 o'clock A.M. — Pains increasing in strength and frequency; but I cannot discover any presenting part. She assures me that the child is living, but I cannot ascertain it by the stethoscope. Bowels relieved this morning. 4 o'clock P.M. — Os uteri more dilated. I can just feel what I believe to be the head resting on the brim of the pelvis. 11 o'clock P.M. — Pains rapidly increasing, and very severe. I can now clearly

distinguish the head just entering the pelvis. 12 o'clock. — Head gradually advancing. At 10 minutes before 1 o'clock in the morning, during a very severe pain, the child was expelled, about 56 hours after puncturing the membranes. The head was much flattened and elongated, but the child breathed, and cried feebly: wrapped it in a warm flannel, and placed it in a woman's lap before the fire, the night being intensely cold. In about half an hour, having given it a teaspoonful of weak gin and water, it began to cry strongly, and to breathe with freedom. After having given the usual directions for the care of the mother, to whom I gave thirty drops of *Tinct. Opii*, I directed the child to be washed and wrapped in a warm flannel, and, on account of the extreme cold, to be placed in a warm bed with the woman who first received it, but not with the mother, lest she should be disturbed by its presence, and not to be dressed before my visit in the morning.

31st, 10h. 30m. A.M. — The mother has passed a tolerable night, and is comfortable this morning. The child breathed freely, and cried loudly till about 9 o'clock this morning, when the mother, awaking from a sound sleep, found it dead by her side, whither it had been conveyed contrary to my positive orders.

February 1st. — Very comfortable.

3rd. — As well as women usually are. Bowels relieved naturally this morning.

CASE IV. — Mrs. Elizabeth C —, Feb. 22nd, 1850, sent this evening in great haste to inform me she was in labour. On my arrival I found that, according to her own reckoning, she wanted full ten weeks of her full time, but was alarmed by a sudden attack of hæmorrhage unattended with pain. The hæmorrhage was slight, but, as she was greatly alarmed, I remained with her about two hours. No pains coming on, I desired her to remain in the recumbent position, to be kept cool, and have no stimulants. I then gave her twenty drops of *Tinct. Opii*, and left her.

23rd. — Slept well during the night. No pain. Hæmorrhage continues, in a fluid state. No coagula; no heat or tenderness of the abdomen. Says she feels the child very clearly. Pulse firm, and not too frequent. On examination per vaginam, with the intent of rupturing the membranes, I found that I could

not reach the os uteri: I therefore gave a scruple of ergot, hoping to excite the action of the uterus, and, as soon as I could get my finger introduced, intended to perforate the membranes with the catheter. The ergot produced sickness and a little pain in the abdomen. After about an hour, on examination, I could just reach the os uteri, but not sufficient to introduce my finger. The hæmorrhage has nearly ceased; and, as I suspect the placenta is situated near or over the os uteri, it will be hazardous to try the catheter, which might pierce the placenta and increase the hæmorrhage.—*R. Secale Cornuti, ʒi.; Aquæ, ʒviij. Coque ad ʒvj. et cola. Sumat cochl. maj. ij. 4 v. 5. q. h.*

24th.—Much the same. Mixture caused sickness, but no pain. To remain in bed, take nourishing food, but no stimulants.

25th.—Hæmorrhage nearly ceased. Is very desirous of getting up, which I have strictly forbidden.

26th.—Contrary to my orders, she came down stairs this morning, but, becoming very sick and faint, she was obliged to return to bed. No increase of hæmorrhage, but complains of constant nausea. Bowels relieved this morning.—*R. Sodæ Carbon. ʒij.; Tinct. Cardam. comp., ʒij.; Infus. Gent. comp., Aq. Ment. Pip. aa. ʒijj. Mix. Sumat cochl. ij. ter in die.*

27th.—Considers herself better to-day. Very little hæmorrhage; no pain; appetite good. The neighbouring gentry are exceedingly kind in sending supplies of food; but I have strictly forbidden wine, spirits, or beer.—*Rep. Mistura ut heri.*

March 1st.—The membranes ruptured spontaneously last night about 12 o'clock, when I was immediately summoned. Found a very copious discharge of liquor amnii, and some increase of hæmorrhage, but no coagula, and very slight pain. On examination, the os uteri was easily felt; but I could only just introduce the extremity of my finger. The cervix uteri is about an inch in length, and very flabby.—*Capt. Pulv. Secalis Cornuti, ʒj. statim.* After remaining with her about an hour, finding no increase of pain or any indication of labour, I left her, desiring to be sent for if pain or hæmorrhage occurred.

2nd.—Liquor amnii, deeply tinged with blood, continues to dribble away,

but without pain. Vomited after I left her yesterday, and still complains of nausea and faintness: pulse 84, and of good strength; bowels regular.—*Rep. Mist. c. Infus. Gent., as prescribed Feb. 26th.*

5th.—Much the same; no indications of labour; os uteri in the same state. Complains of being so long confined to bed; but I explained to her friends and herself the danger of allowing her to get up. Says her medicine comforts her.—*Rep. Mist.*

10th.—Received a message from Mrs. G—, a lady residing in the neighbourhood, requesting that I would summon any assistance if I considered it necessary, offering to pay the expense, even if I thought proper to summon either of the London practitioners in midwifery.

11th.—No alteration. I now determined to avail myself of the very liberal offer of Mrs. G—, considering that if anything unfavourable should occur I should be deservedly censured for not calling further assistance. I therefore called on my friend Mr. Carwardine, of Earl's Colne, and gave him a full statement of the case; on hearing which, he replied that he had been so many years unaccustomed to midwifery practice, that he did not feel sufficient confidence to act in the present case, but considered that my treatment hitherto had been very judicious, and that at present I was not warranted in further interference. However, he recommended me to state the case to his neighbour Mr. Taylor, of Earl's Colne, a general practitioner, about my own standing in the profession. We immediately went together to Mr. Taylor's house, and, on stating the case, Mr. Taylor agreed with Mr. Carwardine and myself, that until more urgent symptoms presented themselves the case had better be left to nature. However, he accompanied me to the patient's house, and after a patient examination per vaginam, and repeated questions relative to her former and present feelings, decided that further interference was not required. We therefore desired her to keep perfectly quiet, and endeavoured to comfort her by assuring her that we all considered it best not to interfere; that although true labour pains might be daily or hourly expected, it was possible that she might complete the full period.

April 1st.—A hasty messenger about

6 o'clock this evening, informing me of a sudden gush of blood to a very considerable amount had just taken place. On my arrival the hæmorrhage had nearly ceased: no pain. On examination the os uteri presented the same closed condition as before: I therefore enjoined perfect quiet, the application of cold vinegar and water to the pubes, and left her.

2nd.—Hæmorrhage nearly ceased.

4th, 7 o'clock p.m.—A sudden accession of pain, but without hæmorrhage. On examination I found the os uteri well dilated, and flaccid. Breech of the child presenting: pains strong, and at short intervals. I had fully expected a presentation of the placenta, but on the most careful examination can discover nothing of the kind. At 45 minutes after 8 o'clock, full five weeks after the rupture of the membranes, the child, a female, was expelled, apparently wanting 7 or 8 weeks of the full period: after gasping with attempts at respiration for about three minutes, it expired. The placenta, which was situated at the back part of the uterus, near the fundus, was thrown off about ten minutes after the birth of the child; but no appearance as though any part of it had been torn or prematurely detached could be detected.—*Sumat Tinct. Opii, gtt. xxx.*

5th.—Very comfortable, and as well as women usually are, and very thankful for my attention. Her complete recovery was not interrupted by any untoward circumstance.

Castle Hedingham, May 20th, 1850.

TREATMENT OF BURNS. BY DR. REESE.

AMONG the most numerous cases brought into the surgical wards of charity hospitals everywhere, may be reckoned the injuries received by burns and scalds, which, when extensive, are too often fatal. In the treatment of these injuries, we have had great experience and uniform success, when the patients were brought in soon after the injury. No fatal case of recent burn or scald has occurred in the hospital, although several have been extensive and severe. The universal treatment of all such cases is to cover the parts with wheaten flour thrown over the wounds by a dredging-box, which, if thoroughly done so as to exclude the air, and prevent its temperature from reaching the suffering tissues, will afford instant relief from pain, and allay all that nervous irritation which is

the chief source of immediate danger in all cases of extensive burns. We have had opportunity to test this practice in terrible burns occasioned by explosions of gunpowder, in scalds from the bursting of steam-boilers, in examples of persons while drunk falling into the fire, and others in which the clothes were burnt off the body by the combustion of spirit gas, &c. In all these cases,—and in some of them scarcely any portion of the body had escaped,—and notwithstanding, in a few of them, the integuments were literally baked, so that extensive and deep-seated suppuration and sloughing were inevitable, and had afterwards to be endured,—the external application of the flour was in the first instance our only remedy, and this was continued for one or more days while the acute effects of the injury demanded it. The superficial portions of the burn or scald would often heal under this application alone; and the solutions of continuity, more or less deep, which remained open and discharging, were then dressed with lime-water and oil, by means of a feather, to which creasote was added if the granulations were slow, or the sloughs tardy in becoming loose. Under this dressing the most formidable burns have been healed; and, even when the face has been involved, there has been scarcely any considerable deformity. In one of our patients, the face being horribly burned by an accidental explosion of gunpowder, the grains of powder having been embedded in the skin, very great apprehensions were indulged that the discolouration thus produced would permanently disfigure and deform the countenance. But, after the persistent application of the flour for three successive days, and until the tumefaction of the face and head had subsided, it was found that, with a few applications of the lime-water dressing, the cicatrization was complete, and even the discolouration was removed.

If this simple remedy were resorted to in the severe scalds sometimes occurring from explosions of steam-boat boilers, &c., there can be little doubt that the fatality of such burns would be very rare; while the popular and mischievous methods of applying raw cotton, oil, molasses, salt, alcohol, spirits of turpentine, sugar of lead water, ice, &c., to *extensive and deep burns*, are all of them injurious, and often destructive to life.—*American Journal of Med. Sciences*, 1850.

. When the burn is superficial, one of the best applications is to cover the skin with a varnish of dextrine applied in successive layers. *Probatum est.*

A CASE OF
DISEASE OF THE SPINAL CORD,
IN WHICH THE SYMPTOMS
SUPERVENED UPON A RECENT COLD:
SMALL-POX HAD PRECEDED ONE YEAR,
AND INJURY TO THE SPINE
TWELVE YEARS.

BY J. F. MARSON,
Surgeon to the Small-Pox and Vaccination
Hospital, London.

(*Read at the Westminster Medical Society,
May 11, 1850.*)

THE subject I am desirous of bringing under your notice this evening, is a case of disease of the spinal cord, the particulars of which I will endeavour to relate to you as accurately as I can, just as they appeared during the progress of the patient's illness, and after death on dissection.

Joseph Thomas, a black, a native of Nova Scotia, 22 years of age, was attacked, in the beginning of March 1845, with symptoms such as are produced by a common cold. He had been employed as a servant in the wards of the Small-Pox Hospital for about twelve months, subsequently to being a patient there with small-pox, which disease he had in a mild form. He was an active, intelligent man, of middle size, and rather slight form. During the first few days of his illness he was allowed to sit up and amuse himself, without doing any work; but the symptoms of his disease increasing, he was, with some reluctance, prevailed on to confine himself to bed: his pulse at that time was soft, and from 90 to 100 in the minute. He had thirst, a furred tongue, and copious perspirations. At the end of a week he complained of stiffness of his neck, and pain on attempting to move his head from side to side; and he stated that he had had flying pains in his neck and shoulders at night, occasionally, for a month or six weeks past. After being confined to his bed for about a fortnight, he began gradually to lose the use of his arms, and subsequently of his lower extremities. This was followed by retention of urine, and his stools were passed involuntarily. His bladder was emptied night and morning with the catheter for ten days, when he passed

his water also unconsciously; and from this time there was a constant dribbling of urine, the bladder remaining in a contracted state. He had complete loss of voluntary motion in all parts supplied with nerves below the fourth pair of cervical nerves. Sensation, however, did not appear to be in any respect altered, either by increase or diminution. Increased sensibility is considered by Ollivier* a sign that the membranes of the cord are inflamed;—diminished sensibility, a sign of inflammation of the cord itself. The patient's respiration was easy and free, his deglutition good, and he was without pain in any part of his body, except in his neck, and only there on motion. His appetite was good, and he slept well. He had no pain on percussion over the spinous processes of the vertebrae, no rigors, and no headache. His intellect was in no way affected all through his illness, nor was there any indication of his brain being implicated. Amongst the most common and the most striking symptoms described by Ollivier and Abercrombie, in their works on diseases of the spinal cord and membranes, are muscular rigidity, twitchings, or convulsions: yet there were no such signs to guide us in the case now under our consideration;—no movements could be excited in either the upper or lower extremities by tickling the soles of the feet or palms of the hands, or other parts of body, with a feather.

It was, however, tolerably evident, from the symptoms detailed above, that there was some mischief going on insidiously in the spine, most likely of an inflammatory nature; but from the softness of the patient's pulse, and the profuse perspiration, it was not judged to be proper to employ general bleeding; and there was no pain nor tenderness on percussion over the spinous processes, to indicate where local bleeding might be of service. Purgatives and saline medicines were administered; the patient was kept in bed, and on low diet. Bleeding not seeming to be admissible, he was very early brought under the influence of mercury, so as to make his gums slightly tender, which was continued for more than a fortnight; and a narrow blister was applied from the nape of the neck to the lumbar region. The greatest care was taken

* *Traité de la Moelle Epinière, et de ses Maladies*, 2me édit. t. ii. p. 598.

to keep his bed as dry as possible, by the use of the urinal, &c.; but from his constantly lying in one position, without the power of moving in any way, he soon had a sore formed over the sacrum. Directly that this sore was discovered, he was placed on an India-rubber collar or crescent; but a large slough formed, which appeared to be the proximate cause of death, after an illness of seven weeks. Not the least pain was ever felt in the wound over the sacrum, although sensation was good in all other parts of the body. I may perhaps be allowed to state here, that, owing to the great pain experienced in moving the head forward, I predicted, three weeks before the patient's death, that there was pus in front of the cervical portion of the spinal cord, but that in all likelihood the cord itself was not materially diseased in this part; because the muscles of the chest and abdomen were used in breathing, which would most likely not have been the case had the cord been seriously diseased in the upper part of the spine. This prognosis proved to be correct on dissection.

Post-mortem examination.—Lapse of time after death, twenty-four hours. The spine was carefully examined in its whole course. There was purulent deposit on the anterior surface of the theca vertebralis, occupying about 5-6ths of the whole of the cervical portion of the canal; but the cord itself in this part was not diseased. About six inches of the membrane of the dorsal portion of the cord was much congested, and its cellular tissue infiltrated with a gelatinous effusion, opposite to the lower dorsal vertebrae. The entire cord was somewhat softer than natural; portions of which, of variable extent, were completely disorganised, and reduced to a semi-fluid condition. The fluid found in the vertebral canal could not be said to be in unusual quantity. The medulla oblongata and pons varolii appeared to be quite healthy. Some years ago, when a boy about ten years old, this man received an injury from a horse, by being thrown down and trampled upon; but there was no breach of surface, the parts at the time appearing only to have been bruised: but now, two of the right transverse processes of the seventh and eighth dorsal vertebrae were discovered to have been fractured opposite to the softened and

congested part of the cord and membranes; and in the areolar tissue surrounding these transverse processes there was slight, but evident purulent infiltration. The processes were loose, and the separated or disunited ends in a state of ulceration: they did not appear ever to have united after the fracture: but of this it is impossible to speak with certainty. Ample evidence being found at once, on opening the spine, to account for all the symptoms of disease shown during the fatal illness, other parts of the body were not examined.

I looked upon this case at the time as one of those untoward occurrences and intricate forms of disease, that we meet with every now and then in practice, the approach of which it is perhaps impossible for us to foresee, or, if foreseen, to prevent. Considerable mischief has usually been done in such cases, and the disease has made great progress before the symptoms of it are such as to guide us to a decision as to what is probably going on. The patient in this instance had been at work just before his illness appeared, cleaning the balusters of a staircase, and being an industrious man he had worked hard, and perspired freely, on a cold windy day in the spring, with a window open at the top of the staircase, and another at the bottom. This I look upon as a most dangerous position for a person to be working in: I had remonstrated with him for doing so; and I have remonstrated with others since repeatedly for the same thing, but all to no purpose: they seem to enjoy the breeze, and disregard the caution given. It is, however, likely that the mischief thus done would have passed away as a common cold, but for the predisposition to disease arising from the injury received from the horse some years previously, influenced somewhat, probably, also by his late attack of small-pox. Injured parts are disposed to take on morbid action when brought under exciting causes. Of this we must all have seen examples. A striking instance came under my notice a few years ago:—A sailor, on jumping from his ship to the pier, lost his balance, and fell back on the ship, striking the calf of his leg against the side of the vessel. No harm came of the blow at the time; but several months afterwards he caught small-pox, and in the

progress of the disease a deep abscess formed in the calf of the leg, between the gastrocnemius and soleus muscles, which I had occasion to open, and had to cut completely through the gastrocnemius muscle of a strong muscular man before I reached the pus.

It may perhaps be thought by some, that this man with disease of the spinal cord ought to have been bled at the onset of his illness; and I am not prepared to say, if we could then have been quite sure what was going on, and what would be the result, that it might not have been serviceable: but I have stated that there were reasons against bleeding—namely, a soft pulse, profuse perspirations, and the constitution of the black. Blacks do not, in this country, so far as I have observed, bear bleeding so well as the Englishman does. Besides, I will state to you what our distinguished surgeon, Sir B. Brodie, says, in an excellent paper on disease of the spinal cord arising from injury, on the subject of bleeding.* He says to the effect, that in injuries of the spine, followed by inflammation of the membranes, it will be proper to take blood from the arm, and perhaps to repeat the bleeding; but it should be remembered, that in cases of injury to the spine the patients do not bear bleeding so well as after injuries of the head. "It is, however," he continues, "if my experience has not much misled me, a great mistake to suppose that blood-letting is always proper. In the majority of cases the state of the pulse is such as actually to contra-indicate the abstraction of blood; and the blood, when drawn, does not in general present those appearances which are supposed to mark the existence of inflammation. I have no reason to believe that blood-letting arrests the process of softening and dissolution of the spinal cord; and, indeed, I have usually found that the symptoms which mark the existence of these changes make a more rapid progress in proportion as a larger quantity of blood is taken away." Now if bleeding be considered improper in some cases of direct and perceptible injury to the spine, it may well be employed with hesitation in cases involved in great doubt, where there has been no known injury done. That there are exceptions to the use of bleeding I have no doubt, and I believed, and

still believe, this case one; at the same time, I think, as a general rule, it will be well to employ bleeding, general and topical, at the *very beginning* of the illness, if we can make up our minds that there is inflammation going on in the cord; but when the disease has existed for some time—say a fortnight or three weeks—it may then fairly be questioned, whether bleeding is a judicious measure to adopt or not.

And here I feel that our best powers of perception and diagnosis will be required to guide us to a correct decision. The part diseased is surrounded by a strong bony covering, so that pressure upon it does not produce pain. Mr. Copeland* has suggested an admirable means of assisting us in our diagnosis of these intricate cases: he says, when there is no pain on pressure over the spine, "he has frequently detected the seat of the disease by the part being more susceptible of the stimulus of heat: a sponge wrung out of hot water and carried down the spine, will often give a very acute degree of pain while passing over the part where disease is going on." This test, however, I need scarcely remark, will be of no use to us in those cases where sensation, as well as voluntary motion, has been lost.

When similar cases come under our care, we should watch them with the very greatest vigilance at the commencement, in order to make out if possible, and as early as possible, the exact seat of the inflammation; for inflammation I suppose it is, in nine cases out of ten, in its origin. I feel convinced, upon reflecting on these cases, that our only chance of doing much good will be at the beginning; our early remedies will be those of the greatest use, and they should be employed as boldly and decisively as circumstances will permit; bearing in mind that it is only in the early stage that the progress of the disease is much under our control. Let a fortnight or more go by, and then our depletory measures will most likely do more harm than good. It is, it appears to me, one of those cases in which the rule *principiis obsta* is most essential to observe and act on. When, however, the disease is in a chronic state, I can readily understand that repeated bleedings will only tend to weaken the body without relieving the disease, and only hasten the

* Med.-Chir. Trans. vol. xx. p. 162.

* Obs. on the Symptoms and Treatment of the Diseased Spine, p. 35, 1815.

termination; as stated by Sir B. Brodie. But then it is not our custom generally to use repeated bleedings, nor do we expect to cure other forms of chronic inflammation by bleedings: we resort to different means of relieving it, mercury and counter-irritation for instance; and the same rule of treatment will, I take it, apply here as in other forms of chronic inflammation: but this does not militate at all against bleeding in the early stage of the disease, when, as is not uncommon, there are active symptoms. At the same time, it should be remembered, there are forms of the disease of an asthenic character from the first, scrofulous, perhaps, assuming a low form of inflammation, but still inflammation, although not such as would justify us in employing bleeding. Whilst expressing my belief, earnestly as I have done, that bleeding and other active measures should be employed at the commencement of the disease, I would again say,—and especially with reference to bleeding,—but let them be resorted to only after the most mature deliberation. And I feel that I ought to quote a few words from the admirable work of Abercrombie, on disease of the spinal cord: he says,* “Several very remarkable cases have occurred to me which presented all the characters of disease of the spinal cord, while nothing could be found, either in the brain or the cord, that could in any degree account for the symptoms.” He then describes carefully four cases, having paralysis and other symptoms of disease of the spinal cord, in persons in whom no disease could be detected after death, either in the cord or brain: still they had died, and we may fairly presume not without a cause. These cases are followed by the relation of several others, with similar symptoms, which recovered.

Sir B. Brodie,† Andral,‡ and Rostan,§ are of opinion that softening of the brain and spinal cord may take place independently of inflammation. Abercrombie,|| Ollivier,¶ and Lallemand,**

on the contrary, believe that inflammation at some period has always been the cause of it: and I confess myself to have coincided with the latter opinion, until I read an account of the investigations on this subject, made with the microscope, by Dr. Hughes Bennett* of Edinburgh, who has shown that the most important changes may take place in the cerebral substance, inappreciable to the naked eye, but clearly discernible with the microscope. He states “that two kinds of cerebral and spinal softening exist—an inflammatory and a non-inflammatory—which may always be distinguished from each other by means of the microscope: that inflammatory softening is characterised by the presence of exudation-corpuscles and granules, whilst in non-inflammatory softening these bodies are never found: that the nature of inflammatory softening consists in the formation and development of nucleated cells in exuded blood plasma; whilst the nature of non-inflammatory softening consists in the mechanical destruction or maceration of the nervous tissue in serum, or is the result of putrefaction: that the inflammatory and non-inflammatory softenings have been confounded together by morbid anatomists, it being impossible to distinguish one from the other, with any certainty, by the naked eye.”

I have not brought this case before you simply for the purpose of detailing what may perhaps be termed an anomalous case of disease of the spinal cord and membranes, or at least so in some of the points belonging to its history; but for the purpose of connecting it with like diseases of these parts, in which the symptoms are much the same at the onset of the illness, so that we may discuss, and make up our minds, so far as may be proper to do so, what we would do in the way of treatment in similar instances. At the time when bleeding, and other active remedies, would probably be of the most service, the nature of the disease is often unfortunately veiled in great obscurity, and it is only after it has made considerable progress that its true character becomes distinguishable.

One other feature in the case remains for me to bring prominently before

* Pathological and Practical Researches on Diseases of the Brain and Spinal Cord, 3d edit. p. 415.

† Med. Chir. Trans. vol. xx. p. 137.

‡ Précis d'Anatomie Pathologique, t. ii. p. 807, 1839.

§ Recherches sur le Ramollissement du Cerveau, 2me edit. p. 169.

|| O. cit. p. 24, et seq.

¶ O. cit. pp. 545, 608.

** Recherches Anatomico-Pathologiques sur l'Encephale et ses Dependances, t. i. p. 81, 1824.

* Pathological and Histological Researches on Inflammation of the Nervous Centres (Edinburgh Medical and Surgical Journal, vol. lx. p. 398).

you. There is the possibility of this disease of the spinal cord having been the result of, or in some way owing to, the attack of small-pox. Such an occurrence has not before come under my knowledge, and I confess that the probability of its being the cause of the disease of the spinal cord in this instance had not crossed my mind (as the man had been to all appearance entirely well from the small-pox for nearly twelve months), until I was asked lately whether I had ever known such an effect to follow this disease, as two cases, apparently to be ascribed to that cause, had lately taken place: there was not, however, any *post-mortem* examination of either of them. On looking over the able treatise of Ollivier,* on the Spinal Cord and its Diseases, I find that three cases are alluded to by him of disease of the cord, two occurring after petechial, and one after intermittent, fevers. He, however, did not see one of them: two of the cases are by Bréra, the third by Petronelli,† this case having been treated at the garrison of Barcelona. There is a fourth case related in the work of Racchetti,‡ of a girl ten years old, who had inflammation of the spinal cord and membranes, and puriform matter about the *canda equina*. Reflecting on the subject, it appears surprising that such cases are not presented to our notice more frequently, when we call to mind how often we meet with disease of the brain. All of my hearers are aware that great constitutional disturbance takes place on the approach of the exanthemata: in small-pox more especially, there is nearly invariably, in the early stage of the disease, great pain in the dorsal region of the back, apparently to be ascribed to congestion and swelling of the vessels of the spine, affecting particularly that part where there is least room for such enlargement, and where it consequently gives most pain, as first observed and stated by Mr. Earle,|| with reference to other forms of spinal disease.

* Op. cit. pp. 564, 657.

† Obs. de Bréra, in *Annal. Clin. de Montp.* t. iv. 1819.

‡ Obs. et réflex. sur quelques lésions de la moelle épinière, etc., Dissert. inaug. Montpelier, 1826.

§ Della struttura, delle funzioni, e delle Malattie della Midolla Spinale, p. 374, 1816.

|| Phil. Trans. p. 282, 1822.

The points, then, that appear to me to admit of discussion, in addition to those that will doubtless suggest themselves to your own minds, are—

1. Was this disease of recent date, arising from, and the result of, cold and inflammation? or,

2. Was it a disease that had been coming on gradually, as instanced by the shooting pains in the neck and shoulders occurring occasionally at night for five or six weeks before the final attack, connected in some way with the disturbance that takes place in the system on the approach of all the exanthemata, and as one of the consequences, in the case before us, of *variola*? or,

3. Was it a case of *ramollissement* of the cord that had been going on slowly for years, dating its origin from the time when the injury was received from the horse, and the transverse processes of the vertebræ were fractured, and probably other violence done at that time to the spinal column?

Whichever of these points may be decided on as having been the probable cause of the mischief,—and I would beg to say that we have a better opportunity of deciding on them, now that the case has terminated, and given us an opportunity of examining the body, than when it was under treatment,—will somewhat influence the opinion we may form on the propriety of the remedial measures that were or ought to have been employed. If, for instance, we view the case as an attack of meningitis of the cord, or of myelitis, or both, arising from cold, depletory and other remedies for the subdual of inflammation will be considered to have been proper: if, on the other hand, we view it as the result of small-pox, occurring twelve months after this disease; or, going back still further, as dating its origin from the time when the injury was received from the horse in early life, then, I fear, the resources of our art must be deemed to have been altogether unavailable for affording relief.

COMPOUND FRACTURE OF THE THIGH.

FOUR cases of recovery, without amputation, are recorded as having occurred in four years; one gunshot, the others from falls or other violence. All were in young men of good constitution. The duration of the case was, in no instance, less than a year.—Dr. Hartshorne, in *American Journal of Med. Sciences*, Jan. 1850.

MEDICAL GAZETTE.

FRIDAY, JULY 19, 1850.

THE latest Memorial* addressed by the Society of Apothecaries to the Home Minister, is a document worthy of the attentive consideration of the profession. It directs itself to two points—on the one hand, to a general defence of the conduct of the Society and the designation of its licentiates; and, on the other, to a proposed alteration of its powers, so as to meet the views and wishes of at least one section of medical reformers. It is chiefly to the latter point that we wish especially to direct the attention of our readers.

The first proposition is to alter the mode of electing *examiners*. Instead of restricting this office to members or shareholders in the City Company, it is proposed to admit licentiates of ten years' standing; and it is advised that not less than one-half of the Court should consist of licentiates who are *not* members of the Society.

This is undoubtedly a liberal proposition: the only question which is likely to arise respecting it, is whether the best men to be selected as examiners in the different departments of medical science will always be found among the licentiates of the Society? Will the men best qualified to examine in Medicine, Chemistry, Midwifery, and Materia Medica, possess the Apothecaries' licence? The fact of their practising as licentiates must deprive them of those opportunities of special study which are necessary to qualify a man for properly conducting an examination in some of these subjects. We do not exactly see why an examiner should necessarily be a licentiate; or why a man otherwise well qualified, but not a

licentiate, should not be permitted to offer himself for election. Still we must admit that the Society are here only following the example of other institutions, if we except the College of Surgeons in its regulations recently issued. The examiners in Classics, Mathematics, Logic, and Languages, are fortunately not required to be members of the College!—Probably the Society believe that they will always be able to find among their licentiates, men possessed of quite as much knowledge of the medical sciences as may be sufficient to conduct an examination for the license. If this be the view upon which the restriction is based, we have nothing more to say to it. Time alone will show whether such a limitation is judicious.

In respect to *examinations*, the student will, we doubt not, be greatly benefited by the proposed change. An examination on so many branches of study conducted at *one* sitting, is not a fair or a just way of testing the knowledge of a candidate. His mind will necessarily become confused by the multiplicity of subjects to which it must be almost simultaneously directed; and a well-qualified person may be thus rejected, while a man of sufficient assurance, and with the aid of cramming, may readily get through the examination.

On the proposed alteration of the *fee* for the Certificate of Qualification, the Memorial is not very clear. The Society propose that it should be *equalized* for the whole kingdom; but they do not say whether it is to be equalized by lowering the fee to six, or raising it to ten guineas.

The alteration of the *place* where the examiners meet is a matter which concerns their own convenience. It probably implies the building of a new College for examinations, and the formation of a Museum, Library, &c.

* See p. 125.

The abolition of the *apprenticeship*, and the legal *registration* of all persons licensed to practise by the Society, are important improvements, which have been long urgently demanded.

The next clause, regarding the admission *without* further *examination*, and upon the payment of a small registration fee, of all persons holding a degree in *Medicine* granted by any British University, and of all members of Scotch or Irish Colleges, legally authorised to grant licenses for the practice of *Medicine* after examination, is highly liberal: it shows a *bonâ fide* desire to secure the public; and not that greediness for fees which has been sometimes charged against the Society. The same remark applies to Army and Navy Surgeons after five years' actual service. Those who will benefit by this clause will be the possessors of British diplomas to practise *Medicine*. A surgical diploma alone will not give this right to an *ad eundem* license at Apothecaries' Hall.

Some consideration is required in order that this liberality should not give rise to abuse. A student must not be led to seek for a University or College in Scotland or Ireland, where the examination may be comparatively easy, and then claim the English license of the Apothecaries' Society. This would defeat the purpose of the provision. The Apothecaries' Society should insist upon an *equality of examination*. This, however, is a matter of detail. The propositions of the Society would render a new College of General Practitioners unnecessary; and it remains to be seen whether they will be accepted by the Government.

ROYAL COLLEGE OF SURGEONS.

GENTLEMEN admitted members on the 19th inst. :—P. P. Lattey—J. Bischoff—J. Carroll—L. Paine—A. Peckett—F. T. Van-Hemert—J. H. Morgan—C. Young—W. Wightman—W. Eddowes—J. T. Small—and W. B. Butt.

LECTURES ON THE TREATMENT OF DELIRIUM AND COMA;

(IN SEQUEL TO THE LUMLEIAN LECTURES
FOR 1850.)

Delivered at King's College Hospital,

By R. B. TODD, M.D., F.R.S.

(Reported by Mr. LIONEL S. BEALE, Med. Associate K.C.L.)

LECTURE V.

Treatment of the delirium of scarlet fever and other exanthemata—The delirium of typhus or typhoid fever—two forms—Nature of typhus fever—outline of its treatment—use of stimulants—local treatment in the delirium—blisters—Dr. Corrigan's plan—use of opium—Dr. Graves' plan of tartar emetic and opium—Treatment of hysterical delirium—Case—use of opium—restraint—Treatment of puerperal delirium—prognosis—Case.

GENTLEMEN,—I shall not occupy your time with any lengthened discussion of the mode of treatment best adapted to control the delirium which sometimes accompanies scarlet fever, measles, and other exanthemata; because I think I may state with confidence that this delirium is in all essential points the same as that of erysipelas, and that in your treatment of the former you must be guided by the same principles as would direct you in managing the latter.

I now proceed to consider the treatment of the delirium of typhus fever. In all low forms of fever delirium frequently manifests itself; and this is particularly the case in the course of typhus fever; and of typhoid fever also, if you choose to adopt the opinion of those who admit the existence of two distinct forms of disease: the one a fever of low type, characterized by a tendency to ulceration of Peyer's glands and diarrhoea—the *typhoid fever*; the other, the *true typhus*, a contagious disease, with brown tongue, great prostration, and without the tendency to diarrhoea. This is not the time to discuss the question whether two such distinct forms of fever really do exist. I must content myself with using the term typhoid delirium generically; for in both states of fever we meet with delirium of the same character, which requires the same kind of treatment, but we notice it under the two following forms:—

1st. Delirium of the low and muttering

kind, in which the patient lies on his back, constantly muttering to himself, apparently unconscious of what is going on around him, but capable of being roused by loud speaking.

2ndly. Delirium of the active kind, in which the patient is very restless, wakeful, talkative, wanting to get up, and requiring careful watching, and perhaps restraint.

When the delirium is of the active and wakeful kind, it generally comes on quite suddenly; but the low form of delirium, which is the most common, comes on very gradually, being first noticed perhaps at night as a slight wandering, then the next night becoming more marked in its characters, and afterwards continuing through both day and night.

Now delirium in fever, of whatever kind it be, is a very serious symptom, and demands the closest attention from the practitioner. Its occurrence generally denotes great depression of the vital powers, and it is of itself calculated to increase exhaustion, especially if the delirium be of the active kind.

In considering the treatment of the delirium of typhus, we shall do well to look a little into the nature of typhus fever itself, as in a former lecture I referred to the chief points in the nature of erysipelas while discussing the treatment of the delirium which accompanies that disease. Typhus fever, then, may be said to arise from the introduction into the system of a special morbid poison, capable of being generated in the human body, and of being communicated from one person to another, although possibly this may not be the only way in which the poison may be propagated. The severity of the symptoms produced by the imbibition of the poison varies much in different cases, depending upon the dose of the poison which has been imbibed, and perhaps, also, upon the condition of the patient at the time of the imbibition, and likewise upon the nature of the poison itself; for doubtless this may vary at different times, and so give rise to that variety in the nature of different epidemics to which most practical men bear testimony.

The morbid poison is eliminated from the system through certain channels, as the skin and mucous membranes; but, like the poison of erysipelas, scarlatina, and many others, it exerts an influence on the condition of the system, which is generally of a very depressing kind, and this continues until the poison is eliminated, or its influence exhausted by some change which it may undergo in the blood.

This seems to be the view of the nature of typhus fever which is most consonant with reason and experience. If it be true,

we can scarcely expect that any means we can devise will cut short the fever, or rapidly eliminate the poison from the system. We must deal with the disease as we would with erysipelas, scarlet fever, small-pox, and support our patient while the fever runs its course, guarding him as much as we can against the destructive influence of the poison. And as this poison is apt to cause great depression, the chief business of the practitioner will be to devise the best means of opposing this depressing influence. This may be best done by giving food, of a nutritious kind, in such a way as may be most easily digested, and will occasion the least irritation of the digestive organs. It will generally be found necessary to administer stimulants, and I think that it is unwise to postpone this part of the treatment long; on the contrary, I prefer giving stimulants *early*, as, by so doing, the necessity for giving them largely is generally avoided. Throughout the whole course of the fever, the greatest pains should be taken to husband the strength of the patient by good nursing, and by instant attention to every want. He should never be suffered to do anything for himself, nor allowed to get out of bed; he must be raised or turned in bed when necessary, and he must be fed as you would an infant. Too much importance cannot be attached to these apparently trivial matters in the management of so serious a malady as typhus fever. Many a patient has lost his life for want of these necessary attentions,—either from the culpable neglect of them on the part of attendants and nurses, or from his inability to obtain them.

If the bowels are confined we should give a purgative which will act very mildly, or they should be opened by enema; and, in giving medicine for the bowels, we should never lose sight of the danger there is in all these cases of the supervention of a troublesome and debilitating diarrhoea. It never can be necessary in typhus fever to purge, or to do more than keep the bowels in such an open state as will assist digestion.

In consequence of the liability of Peyer's glands to be affected with ulceration, strong purgatives may do great mischief, by increasing the inflammation, and perhaps exciting ulceration, which may end in perforation of the intestine and fatal peritonitis. On the other hand, it is very necessary to provide for the due evacuation of the intestinal canal, not only to promote the digestive powers and the due absorption of the food, but also because, if the secretions are allowed to remain in the bowels, they will themselves excite irritation, and interfere with the due elimination of the poison.

All men of experience agree that it is

right and necessary to uphold the strength in typhus fever, but differ as to the best mode of effecting this object. Some only give nutritious food, others think it necessary to combine stimulants with it. My own experience has led me to the conclusion that stimulants are necessary in the vast majority of cases. I have never seen any ill effects arise from the early administration of stimulants; and, on the other hand, I have often had occasion to regret that the use of them had been too long delayed. Moreover, this fact weighs much with me in inducing me to give stimulants early,—namely, that if a patient seem overstimulated, nothing is easier than to pull him down, and that pretty quickly; but if he be insufficiently supported and stimulated, it is often of extreme difficulty to build him up: *hic labor, hoc opus est*. And with reference to the administration of stimulants, I have only to repeat the rules which I gave you on former occasions. Do not give large quantities at a time; do not embarrass the patient, but begin with small quantities frequently repeated. Give wine first, and then, if necessary, change to brandy, or some other spirit. Chloric ether is one of the best medicinal stimulants: it may be given in half-drachm doses every three or four hours, or more frequently if necessary, alone, or in combination with five or six grains of carbonate of ammonia.

Such is a brief outline of the general plan of treatment to be pursued in typhus fever. If, now, we find that delirium sets in in the course of the fever, what is to be done? Ought this to lead us to alter in any way our mode of treatment, and to adopt new and different measures? Whatever be the nature of the delirium, we must, I think, deal with it as part and parcel of the fever, and view it as an indication of a more depressed state of the system calling for an increased amount of stimulants, or a change in the nature of them, and for greater diligence in the administration of nutritious food.

Is there any necessity for local treatment directed to the head when delirium comes on in the course of typhus fever? Nothing is more certain than that there is no inflammation of the brain in typhus fever, not even when delirium occurs: there is, therefore, no need for active local measures on this account. Some years ago a theory was put forward by a distinguished physician of this town which ascribed typhus fever to inflammation of the brain; and this gained some support from the fact that inflammation of the brain frequently takes on the characters of typhus fever, as was the case in a patient whose history I related to you in a former lecture. But the experience of nearly all practical men

in all countries tends to show that there is no connection between typhus fever and inflammation of the brain.

You will say, however, that, though there is no inflammation, there may be congestion. Undoubtedly this is the case; but the congestion is not peculiar to the brain: it is only part of a general congestion which affects the capillary system of the whole body, and is probably due to the presence of the typhus poison in the blood, which weakens the forces by which the blood is moved in that system of vessels. To relieve this congestion, therefore, ought we to resort to local depletion? Upon this point you must be guided by the circumstances of each particular case, bearing in mind that in so enfeebled a state of the capillary circulation the mere taking away of blood is seldom of much use. What is most needed under these circumstances is something to stimulate the capillary circulation, so as to promote the flow of the blood, which tends to stagnate in the fine bloodvessels. Now for this purpose the application of blisters to the shaven scalp is most useful; and you will often find it a better plan to apply several small blisters to different parts of the scalp in succession, than to apply one large one. Sometimes, however, you will not have time to wait for this process; the patient has a strong tendency to coma, or is actively delirious, and at the same time his strength is rapidly on the wane. When this is the case you must apply one large blister all over the scalp; and you will find Dr. Corrigan's plan a very good one—namely, to cut the blister plaster into strips, and to lay them over the scalp as you would strap a man's leg, so as to bring the blistering material into contact with the scalp at every point.

If you should see any clear reasons for taking away blood, you may best gain your object by having it done by expert cupping from the temples. The rapid application of the well-exhausted cupping-glass may act as a stimulus to the capillary circulation, and promote the flow of blood through it.

When the delirium is of the restless and wakeful form, are we to give opium? As a general rule, we must be extremely cautious in the use of opium in typhus fever. We know that opium tends to produce the same effects as the typhoid poison—namely, to favour capillary congestion; therefore, by giving opium, we should only aggravate one of the great evils of the typhoid state. There is another reason why opium should not be administered in fever—namely, that it tends to clog up the secretions, and so to prevent the elimination of the morbid poison, which is an object we are desirous of promoting.

Nevertheless, when the state of congestion is not obvious, and the powers of the patient are not very low, I have seen the very best effects derived from the administration of one or two doses of opium well timed.

You will often find it necessary and most useful to give opium by enema in those cases of diarrhoea which often occur in typhus, or, if you will, in typhoid fever. This is a practice which I always follow, and with the best results, never to allow the diarrhoea to get ahead, but to keep it down by the frequent administration of small enemata of starch, sometimes, if the diarrhoea be urgent, repeated twice or thrice a day, with five, ten, or twenty minims of the tincture of opium. Dr. Corrigan, who has had great experience in the treatment of fever in Dublin, states that the application of not more than two or three leeches to the temple is often of great benefit in promoting sleep; and he was first led to adopt this practice by observing that spontaneous hemorrhage from the nose was followed by sleep in some cases. Dr. Graves lays great stress upon the advantages of combining tartar emetic with opium: he says that the antimony prevents the too narcotic effects of the opium, whilst the junction of opium with antimony promotes the sedative influence of the latter, and guards the system against its depressing power.

Are we to be deterred by the occurrence of delirium from the continued use of stimulants? The mere occurrence of delirium need not deter you from the continued use of stimulants: on the contrary, in some cases it should incite you to give them more freely, in larger and more frequent doses. But in all cases their influence upon the pulse will serve you as a useful guide; and if you find that under stimulants the pulse does not quicken, but improves in quality, and more especially if it diminishes in frequency, you may continue the use of stimulants.

Hysterical delirium.—I shall now call your attention to the treatment of that form of delirium which is apt to occur in cases of hysteria—namely, hysterical delirium. A hysterical girl begins to form strange fancies: she becomes obstinate and perverse, and it is with great difficulty that she can be managed. Perhaps she may refuse to take food, and conceive a great dislike to some of her relatives, and refuses to see them. Sometimes we can detect no obvious immediate exciting cause for these symptoms beyond the existence of a chronic hysterical state; but in the great majority of cases, however, while the hysterical diathesis is present, the patients have been

exposed to some causes of depression. Such is the case with a girl of the name of Walker, now in the hospital, who is suffering from this form of delirium. She belongs to the hard-worked and ill-paid class of needle-women who have excited so much interest of late. For some time past she has been sitting at her needle for many hours a day, getting no air nor exercise, and feeding badly. She is a Roman Catholic, and an enthusiast in religion, and has been led by her religious views to practise a considerable amount of asceticism, which, in addition to her privations from poverty, have contributed much to increase her depression. In this girl there was at first no violence, but she was continually sighing, and seemed always in a melancholy mood; sometimes reciting hymns to the Virgin, at others praying fervently, imagining herself in chapel. She frequently spoke of her father and mother, without having any definite notions as to where they were, or seeming to know that one of them was dead. She could seldom be engaged in conversation, and could give no satisfactory account of herself, and she exhibited great reluctance to take food. In addition to the other causes of depression I have mentioned, she has been irregular in her catamenia; and on the last occasion, a few days before she became delirious, she had a very profuse discharge of blood, which weakened her extremely.

Sometimes patients suffering under hysterical delirium are extremely violent and mischievous, and even furious, using the most coarse and even obscene expressions. Often there is great wakefulness, which threatens to exhaust the patient.

In cases of hysterical delirium we generally meet with disturbed uterine functions; the catamenia are affected in some way, being too copious, or perhaps deficient in quantity or altered in quality; the intervals of their recurrence may be very irregular, or many months may pass without the patient having menstruated: sometimes, on the other hand, they recur too frequently. Leucorrhoea is also often present in these cases to an extent which produces extreme weakness. Under such circumstances, when a patient is exposed to privations, or from loss of appetite is unable to take food in sufficient quantity, or receives some severe mental shock, delirium is very apt to show itself. Moreover, young women, and especially those of the hysterical diathesis, are extremely apt to neglect all attention to the action of the bowels, which alone must seriously injure the general nutrition. It is not uncommon for such young women to suffer from a confined state of the bowels for many days, without exciting attention from them.

Thus, with deranged catamenia, confined bowels, irregular habits, capricious appetite, mental anxiety, often ill-regulated thoughts, and in the lower classes inadequate food and over-work, it is no wonder that a depraved nutrition should be produced sufficient to disturb the balance of the nervous system, and the due action of the mind.

In treating a case of hysterical delirium we must set ourselves diligently to discover the causes which have produced the deranged general nutrition upon which the delirium mainly depends. We must relieve constipation, without using violent means calculated to depress. If the catamenial or leucorrhœal discharge be excessive, it must be immediately checked by suitable remedies, among which, I think that the frequent use of cold water by hip baths is among the best. Careful attention must be paid to improve appetite and to help primary digestion. Hence we shall derive most benefit, generally speaking, from the combination of tonics, mineral acids, and purgatives, and sometimes from the use of chalybeates.

It is of great importance to devise some good means of administering food according to the particular circumstances of each case. As to the use of stimulants, it is difficult to lay down rules of even frequent, much less of general, application: in this part of the treatment our best guide will be derived from the peculiarities of each case: sometimes it is better to give them, and sometimes to withhold them. The state of the digestive organs, the previous habits of the patient, the effects which stimulants produce upon her, will in general afford us great help. To these I may add the rate of the pulse; a quick pulse, of 110 or upwards, indicating the need of stimulants; a slow pulse, below 90, denoting that they are not necessary. But this you must always bear in remembrance, that starvation will not answer in these cases, nor will antiphlogistic measures: such means always tend to aggravate the delirium.

In the treatment of these cases a great deal is to be done by moral means. Very frequently it is desirable that the patient should be isolated from her friends: by this I do not mean that she should be sent to a lunatic asylum; on the contrary, in a large number of instances such a course would be injurious to the patient, both at the time and subsequently. We have no more right to send a patient labouring under hysterical delirium into a lunatic establishment, than to place a patient affected with the delirium of erysipelas or of typhus there. We have no good grounds for classing these cases, more especially the

more acute ones, with ordinary cases of insanity; and we should be careful to avoid stamping a young woman with the reputation of having been once insane. The delirium in these cases is as much dependent on the disturbed state of nutrition which constitutes the hysterical state, as the peculiar delirium of erysipelas or of typhus depends upon the peculiar fever excited by the respective poisons of those maladies. The patient then, as I said, should be isolated from her friends, and be placed under the care of a kind and attentive nurse, who will keep a constant watch over her, attending carefully to her wants, and restraining her by firm but kind influence from doing what is injurious or mischievous.

Mechanical restraint is highly objectionable in the hysterical delirium, and should never be practised unless when there is an absolute necessity for it. It excites opposition, and irritates the temper of the patient often to a very serious extent. You have lately had a good opportunity of observing its injurious effects in the case of the girl up stairs. She had been treated much on the plan I have already described, with tonics, good diet, and shower bath; but she had got into the habit of wandering about from ward to ward, and the nurse was unable to control her. I directed her to be kept to her bed, and isolated from the other patients by surrounding the bed by screens. Still she could not be kept quiet; and I then allowed the nurse to tie her feet to the bed. This irritated her very much, and the next day we found her much worse, and her delirium much increased. All restraint was then removed, and since then she has been gradually improving.

The same rules which I laid down as to the employment of restraint in cases of delirium tremens, are applicable to the question of using these means in other similar cases. We must be guided by the symptoms which arise in the course of the disease, being always slow to adopt restraint.

Puerperal delirium.—I shall conclude this lecture by a brief allusion to the treatment of another form of delirium, nearly allied to the hysterical—namely, the puerperal delirium. A puerperal woman is in much the same condition as a woman affected with excessive leucorrhœa, or with any other exhausting disease tending to deteriorate the general health. She is exhausted by the efforts of parturition, or her vital powers are depressed by some condition consequent on the puerperal state.

When speaking of hysterical delirium, I omitted to mention that there are two classes, differing from each other in their results.

1. A class of cases in which the pulse is very quick, varying from 120 to 140.

2. Cases in which the pulse is usually below a hundred.

In these two classes the prognosis is very different. If the pulse be quick, and continue quick despite of support, stimulants, and tonics, the prognosis is in general unfavourable; on the other hand, if it be below 100, and show no tendency to become rapid, the case may be looked at more favourably. By far the greater number of the cases of hysterical delirium belong to this latter class, and by far the greater number get well. Now it is just the same with puerperal delirium. We find two classes of cases distinguished by the difference in the rapidity of the pulse, and which affords the same prognostic indications as in hysterical delirium. The existence of these two states of pulse in puerperal delirium was long ago pointed out by Wm. Hunter and by Gooch.

The general treatment of puerperal delirium must be conducted on much the same plan as that which I have laid down for the hysterical. Almost always some exhausting influence has been in operation, or there has been some severe mental shock. You must combine a soothing and strengthening treatment with moral management, and remove as far as possible all causes of mental excitement, as well as of bodily depression.

When you have to deal with a case in which the pulse is extremely rapid, you will generally find it necessary to be very diligent in the use of stimulants and of nutritious food; and indeed your ultimate success in the treatment of the case will much depend on your patient's ability to digest and assimilate these aliments.

We had a good example of the good effects of this treatment in a case of this kind of delirium, which occurred in the hospital last November. The patient's name was Mary Tilley, and she was only twenty years of age. Although in this case the pulse reached the high number of 160, and continued at that rate every day, accompanied by extreme debility, the patient did well. The delirium had been brought on by excessive hæmorrhage after parturition. The patient stated that on the third day after the birth of the child she passed something as large as a child's head, having the appearance of black currant jelly—a tolerably good description of a large clot of blood: this was followed by suppression of the lochia, great prostration, and delirium. When she was admitted, the tongue was covered with a brown fur; she was delirious, talked very much in her sleep, and frequently jumped out of bed, and exhibited a certain strangeness and wildness of

manner; her pulse was 160, and the respiration 40. She was placed upon a freely stimulating treatment, with ammonia, and wine, and beef-tea; for several days she took half an ounce of wine every hour. On her admission the pulse was 160, and respiration 40; the next day it had not diminished in frequency: during these days she was taking half an ounce of wine every hour. On the 14th (the third day after admission) the pulse fell to 130, but the delirium continued. She was sleepless; talked a great deal, and fancied some one was coming to take her away and kill her. The quantity was now increased to an ounce every hour. On the 17th (the sixth day after admission) she was reported to have slept well, and the delirium had considerably subsided. The pulse, however, was still 130. On the 19th she had improved still further, and the pulse had fallen to 120. I now gave her quinine (the tongue having become clean) in doses of two grains every fourth hour. On the 20th the pulse was 108. The quantity of stimulants was now gradually diminished, and on the 8th of December the pulse had fallen to 88, and from this time she rapidly convalesced.

You must deal with these cases of puerperal delirium in much the same way as you deal with cases of erysipelas or typhoid delirium, because in all these conditions the patient suffers from a state of exhaustion detrimental to general nutrition. In puerperal delirium, however, we may give opium with great safety: in violent cases, in which sleep could not otherwise be procured, I should not object to administer chloroform, bearing in mind the cautions to be observed during and after its inhalation, which I mentioned in a former lecture.

It remains for me to discuss the treatment of the epileptic delirium: this, with some remarks on the treatment of the various forms of coma, I propose shall form the subject of my next lecture.

ELECTION OF PRESIDENT AT THE ROYAL COLLEGE OF SURGEONS.

At a meeting of the council of this institution on the 11th inst., Mr. James Moncrieff Arnott, F.R.S., was elected president, in the vacancy occasioned by the retirement of Professor Joseph Henry Green, F.R.S., who, it is stated, has during the past year filled the high position with a dignity, and at the same time an amiability, never surpassed. Messrs. John Flint South, of St. Thomas's Hospital, and Caesar Henry Hawkins, of St. George's Hospital, were elected vice-presidents of the College for the ensuing year. At the same meeting of the council Messrs. Pilcher, Macnirdo, and Kiernan, the recently elected members, took their seats at the board.

Reviews.

Anormal Nutrition in the Human Articular Cartilages, with Experimental Researches on the Lower Animals. By P. REDFERN, M.D., London, Lecturer on Anatomy and Physiology, and on Histology; and Examiner at the University and King's College, Aberdeen, &c. &c. Pamphlet, 8vo. pp. 86. (From the Edinburgh Monthly Journal of the Medical Sciences.) Edinburgh: Sutherland and Knox. London: Simpkin and Co. 1850.

"PERHAPS there is no tissue," the author remarks, "which affords a better opportunity for the examination of many of the changes which nucleated cells undergo, in the different periods of their existence, than that of cartilage; for in it, changes which take place at certain periods only in other textures, are continually going on during the whole life. Moreover, these changes in the articular cartilages in man are effected without any more direct communication with blood-vessels than that which exists upon their surfaces; for it is only when such cartilages exceed a certain thickness—i. e. about 1-8th of an inch, that they are permeated by canals which contain blood-vessels, as occurs in the large mammals."

Dr. Redfern refers to the researches of Drs. Alison and Bennett, showing that inflammation consists in a peculiar perversion of nutrition or of secretion, and "that all the changes that take place in the matters exuded in inflammation, are alone referable to the general plan by which the nutrition of the healthy textures is effected." (p. 6.)

The author then proceeds to observe—

"That changes, which may be equally referred to an abnormal nutrition of the textures, take place in the diseases to which articular cartilages are so liable, appears plain from the nature of the changes that occur in them in such cases, and which I shall endeavour to describe and illustrate as they appeared to me during lengthened examinations of a number of cases, the particulars of which will be afterwards detailed.

"It is said that inflammation, ulceration, softening, hypertrophy, atrophy, and ossification, are the diseases to which cartilages are liable. Of the existence of inflammation in them there is perhaps no other evi-

dence than that furnished by ulceration, which is unquestionably the most important of all the affections of this texture, and is generally believed to be the climax of the most severe affections of the joints." (p. 6.)

Dr. Redfern briefly reviews the most important of the opinions that have been advanced on the nature of ulceration of the cartilages.

In the next place, the author details a series of cases, and illustrates his examination of the diseased cartilages, in almost every instance, by several well-executed wood-engravings of the microscopical characters of these morbid structures. The fulness of the observations, and their accompanying remarks, with the abundance of illustrative drawings, constitute the essential value of Dr. Redfern's work, which, we think it no slight praise to say, may be compared, in these respects, so far as it extends, with Dr. Bennett's work on Cancerous and Canceroid Disease.

Dr. Redfern devotes also a portion of his work to the consideration of the healthy structure and mode of nutrition, before treating of the affections to which the articular cartilages are liable: these are discussed under the heads of Morbid Anatomy, Chemistry, Pathology, Diagnosis, and Treatment. We quote the author's conclusions, deduced from his previous observations and examinations.

"1st. That the whole of the known forms of disease in articular cartilages are connected with changes in the texture, which are essentially similar to each other.

2d. That during the progress of these changes the cells of the cartilage become enlarged, rounded, and filled with corpuscles, in lieu of healthy cells, bursting subsequently, and discharging their contents into the texture on the surface; whilst the hyaline substance splits into bands and fibres, the changed hyaline substance and the discharged corpuscles of the cells afterwards forming, in many cases, a fibro-nucleated membrane on the surface of the diseased cartilage.

3rd. That these changes are alone referable to an abnormal nutrition as their immediate cause, and in no case to mechanical or chemical action,—such as attrition or digestion in a diseased secretion.

4th. That most extensive disease may be going on in many joints at the same time, and may proceed to destroy the whole thickness of the cartilage in particular parts, without the patient's knowledge, and

while he is engaged in an active occupation.

6th. That the disease commences most frequently upon the free surface; but it may proceed from the bone to affect the attached surface, or it may take place in the middle of the thickness of the cartilage.

6th. That it is at least very doubtful if the symptoms, which are believed to indicate the existence of ulceration of articular cartilages, are not really dependent on a morbid change in the bone.

7th. That disease of the whole thickness of an articular cartilage, at particular parts, admits of a natural cure, by the formation of a fibro-nucleated membrane from the substance of the cartilage, without the occurrence of any new exudation." (p. 65).

The importance and value of Dr. Redfern's investigations will be apparent from the nature of the above conclusions, more especially when taken in connection with a series of comparative experiments on lower animals, instituted by Dr. Redfern, whereby these conclusions are confirmed. The results in the last cases are similar to those observed in analogous diseases of cartilage in man. On a subject on which such diversities of opinion have been held as on ulceration and other diseases of the cartilages, it is something to have a series of trustworthy facts carefully recorded by a competent observer. These will be found in Dr. Redfern's work; and we therefore press its study upon every pathologist.

Proceedings of Societies.

NEWCASTLE AND GATESHEAD PATHOLOGICAL SOCIETY.

February, 1850.

Case of Diseased Bladder, with Formation of a large Sac containing numerous Calculi, and communicating through a comparatively small aperture with the general cavity of the viscus.

In this interesting case the bladder consisted of two sacs communicating by a small aperture, the posterior sac being filled with calculi. The man was 36 years old, and broken down by dissipated habits. He was first admitted into the Newcastle Infirmary, under Sir John Fife, in May 1849, with some secondary ulcerations of the legs; he never complained of any urinary disorder until about a month after his admission, when re-

tention of urine suddenly took place; a catheter was with great difficulty introduced by one of the pupils, much blood having been lost, with some suspicions that a false passage had been formed; there was no stricture, nor any other cause to account for the retention. The ulcers on his legs were at this time nearly healed, when severe inflammation of the bladder, extending to the pelvis of the kidney, set in, and he remained in the hospital during three months: he at first required the catheter twice daily, but gradually acquired the power of dispensing with it after the severity of the attack passed away. Along with the acute symptoms, the urine presented a highly ammoniacal and purulent character, which changed to a slightly albuminous condition, depositing a thin layer of pus, and frequently very copious and of a light green colour. When he left the hospital he had recovered his health to a great extent; but still passed urine slightly purulent and albuminous. He was re-admitted under Mr. Heath four months afterwards, and was now found to be weak and wasted, with hectic fever, the result of great accompanying urinary irritation, together with a copious excretion of purulent urine, occasionally mixed with blood. He frequently required the use of the catheter; and, except the difficulty experienced in avoiding the aperture of a false passage at the membranous part of the urethra, nothing could ever be found in the bladder. He died exhausted on the 13th of February, 1850. On opening the abdomen the bladder was seen to be firmly contracted, empty, and in its natural position; projecting from it behind was a large sac of the rounded form of a moderate-sized orange, pressing back upon the rectum and filling the pelvis. The urethra and bladder were laid open. The floor of the membranous part of the urethra presented the open mouths of several enlarged lacunae, and immediately in front of the prostate, a smooth slit-like opening, allowing a No. 10 sound to pass into a small false passage, running behind the prostate, and terminating about an inch above it in the coats of the bladder. The muscular coat of the bladder was greatly hypertrophied; the mucous membrane, thickened, congested, and covered with a purulent layer of mucus, was not ulcerated; at its posterior aspect there was a round aperture of the size of a florin piece, leading into the cavity of the sacculated dilatation of the viscus: the margin of the aperture was rounded, nearly half an inch thick, slightly extensible, and consisted of the hypertrophied muscular fibres of the bladder, covered by the thickened mucous membrane. The sac was filled with purulent urine, and about forty calculi, composed of triple phosphate; the largest set being about the

size of hazel-nuts; the others gradually diminishing until they formed a coarse powder, which was mixed with coagulated blood. The wall of the sac was about 3 lines thick, and consisted of the mucous membrane bound to the peritoneum by lymph and a fibrous kind of cellular tissue: no muscular tissue was found in it.

The ureters and pelvis of the kidneys were considerably dilated; the mucous membrane thickened, congested, and secreting purulent fluid. The secreting structure of the kidney was coarse and flabby; but not otherwise diseased. The other viscera comparatively healthy.

This case is highly interesting and instructive, whether considered with reference to the diagnosis of stone in the bladder, or the practical difficulties occasionally met with in the performance of the operation of lithotomy. None of the symptoms ordinarily relied on as pathognomonic of the existence of a calculus, were here presented, nor did even the passage of a catheter into the bladder lead to the detection of the numerous calculi existing in the posterior sac of that viscus. They would, however, probably have been perceptible by a finger introduced into the rectum; and the history of the present case may perhaps be received as an additional illustration of the importance of instituting such an examination in doubtful cases.

But assuming the calculi to have been detected by the catheter or sound of the surgeon, and the operation of lithotomy to have been undertaken by the latter, how vast and inextricable the difficulties and confusion opposed to his success! On completing the requisite incisions, and passing his finger into the bladder, the surgeon would have felt in vain for the stone, whilst an examination per rectum, performed immediately afterwards, would have satisfied him of the existence of several calculi in the posterior part of the same bladder.

Under these perplexing circumstances he could only have chosen between the unpleasantness of allowing his patient to return to bed with the calculi unremoved, and the certain danger consequent on any attempts to divide the septum which intervened between the pouch containing the mass of calculi and the general cavity of the bladder.

Conversion of a Diseased Kidney into an immense sac, without any perceptible symptoms during life.

Dr. BULMAN communicated the subjoined particulars of this case, which occurred in a seaman aged 26, who awoke one morning with partial hemiplegia of the left side, and after becoming gradually worse, entered the hospital, under Dr. B., in November, 1849, five weeks after the attack. His symptoms

indicated softening of the brain; and after a course of mercury and counter-irritation for one month without any improvement, he died of a most rapid and uncontrollable pleuritis. He had never once complained of any symptom indicating any affection of the kidney.

On examination of the brain, the right corpus striatum was found almost entirely converted into a soft, greasy, pultaceous mass, containing numberless exudation cells of varied size and form. The left pleural sac was filled with semi-purulent fluid, and covered with false membrane. The left kidney, occupying the whole of the left loin, and extending across the spine, was converted into an immense membranous sac, similar in structure to the fibrous pelvis of the kidney, and filled with nearly a quart of slightly turbid, but otherwise healthy urine. Slight remains of very pale secreting structure were observable in the outer and posterior aspect of the walls of the sac. The ureter was found perfectly healthy; but its opening into the pelvis of the kidney was so small, that the head of a probe was with difficulty pushed through it into the great sac occupying the place of the kidney and its pelvis. There was no induration or hypertrophy of tissue around this narrow opening, which appeared to have been gradually contracting during a very long period, but on the contrary the usual healthy tissue of the ureter. The other kidney was very large, lobulated, and of healthy appearance.

Osteo-sarcoma of Tibia and Fibula.

SIR JOHN FIFE exhibited a beautiful specimen of osteo-sarcoma of the lower ends of the right tibia and fibula, removed by amputation of the leg from a countryman, aged 17, whose family were highly scrofulous, and who was admitted into the infirmary. He had first observed a little general swelling above the ankle six months previously, which rapidly increased, accompanied with little pain, and now formed a large and characteristic tumor immediately above the ankle-joint. There was no glandular disease. He quickly recovered from the operation, and was sent home in a greatly improved state of health. The tumor on being cut open presented the brain-like appearance of medullary cancer, with vascular tufts of fungus hæmatodes and numerous spicules of bone radiating from the enlarged tibia and fibula, which were implicated in the diseased growth. Microscopically, the tumor was composed entirely of cells of rounded or ovate form, of varying sizes, and highly organised; the cell walls being covered with numerous small nuclei refracting the light very powerfully.

ACADEMY OF MEDICINE, PARIS.

July 2nd, 1850.

Epidemic Dysentery of Mortagne in 1849, and its Relations to Epidemic Cholera.

M. GIBARDIN read a report on a paper by M. Hullin, of Mortagne. It was stated that in 1832, when cholera prevailed in the district adjoining Mortagne, that place was spared, an epidemic dysentery prevailing there at the time. The same circumstances had occurred in 1849.

The Alkalinity of the Human Blood.

M. CAHEN read a memoir, in which he sought to establish that albumen and fibrine are held in solution by virtue of the alkalinity of the serum: a decrease in the alkalinity of the serum augmenting the proportion of spontaneously coagulable albumen, and *vice versa*. The author laid down these conclusions.

On the healthy state.—1. The serum holds in solution a substance, which ultimately separates the albumen and fibrine. 2. The albumen and fibrine are susceptible of mutual transformation. 3. Fibrine is converted into a substance not distinguishable from albumen, by a solution of soda at the temperature of 106° F.

In inflammatory diseases.—1. Fibrine is increased, albumen decreased. 2. The salts of soda are deficient. 3. The serum is deficient in alkalinity. 4. The increase of fibrine is owing to the diminution of the alkalinity of the serum.

In diseases called putrid, the alkalinity of the serum causes diminution of fibrine, and indicates the use of acids in treatment.

SURGICAL SOCIETY OF PARIS.

July 3rd, 1850.

Reduction of long-standing Dislocation.

M. MAISONNEUVE related the particulars of a case of dislocation of the humerus under the *pectoralis*, in which many fruitless efforts at reduction had been made by M. Velpeau, and in which he, M. Maisonneuve, had succeeded by longitudinal traction continued for ten minutes, chloroform having been administered. M. Maisonneuve added, that in his opinion the adhesions, &c. of old dislocations could always be overcome.

M. CHASSAIGNAC observed that the proceeding adopted by M. Maisonneuve, after the failure of so skilful a surgeon as M. Velpeau, was somewhat hazardous. The employment of anæsthetics in such cases,

the patient being in the upright posture, M. Chassaignac considered to be attended with danger. In long-standing cases he also thought the use of anæsthetics to be contraindicated, as the resistance did not proceed from the muscles alone. M. Chassaignac expressed his surprise that the heel of the operator is not used in France, so much as its success in England shows it to merit, in reducing dislocation of the shoulder.

M. MOREL pointed out the dangers which attend the attempt to reduce long-standing dislocations; and observed that the insensibility produced by anæsthetics deprived the operator of the indications furnished by the feelings of the patient as to how far the extension may be carried.

M. FORGET concurred in the objections to the violence necessary in reducing old dislocations; and, more particularly with regard to the use of anæsthetics, observed that in proportion to the length of time required for the reduction were the dangers of anæsthetics increased by their lengthened employment.

M. MAISONNEUVE did not participate in the fears of other members, and thought that all dangers might be avoided by great care.

ACADEMY OF SCIENCES, PARIS.

July 1, 1850.

Staphyloraphy.

M. ROUX read an essay of considerable length on this subject, of which the following are the chief points:—

M. Roux divided the lesions for which this operation is required into four kinds—the recent accidental; the congenital; the surgical; and the old accidental, or ulcerated and cicatrised solutions of continuity. The operation is modified according to the character of the lesion. 1. A complete cleft in the velum, not implicating the palatine arch; 2. A simple perforation of the velum; 3. A perforation of the arch not extending to the velum. The term staphyloraphy is strictly applicable to the two first only: the last should be designated *palatoplastie*.

M. Roux related cases of these various kinds in which he had operated successfully, and pointed out at the same time their causes—syphilis, tubercle, &c. &c. M. Roux had performed staphyloraphy on one hundred and six subjects; in nine of these a second time, making a hundred and fifteen cases: of these individuals 57 had simple fissure of the velum and uvula; 49 had also fissure of the palatine arch. The

particular features of the result of the operation in many of these cases were also stated by M. Roux. For these details, however, we must refer our readers to the *Comptes Rendus* of the Academy, or to the French journals, our space preventing a more extended report.

Hospital and Infirmary Reports.

LONDON HOSPITAL.

Case of Dislocation of the Thigh into the Foramen Ovale, reduced after four weeks. With Clinical Remarks by JOHN ADAMS, Esq., Surgeon to the Hospital.

JAMES COE, *etat.* 32, a strong muscular man, employed as a navigator on a railway, was admitted on Tuesday, May 28th, with a dislocation of the thigh into the thyroid foramen; the accident having occurred a month previously, in the country, in consequence of the fall of a large quantity of gravel into the left slip, while he was in a stooping posture, with his legs widely separated. He was extricated by his fellow-workmen, being unable to move himself: and on examination, was found to have sustained severe laceration of the perineum and scrotum. Considerable swelling and extravasation ensued, and he was confined to his bed 20 days, the ordinary surgical appliances being resorted to by his medical attendant, and upon the subsidence of the swelling and tumefaction, and the healing of the lacerations, it was then discovered that the left thigh-bone was dislocated from the acetabulum into the foramen ovale: attempts at reduction were then made, and repeated on the following day, under the influence of chloroform; but, as no success resulted, he was sent up to the hospital, and placed under the care of Mr. Adams, who made a careful examination of the limb, with the following results:—The patient was lying in a supine position, with the left limb widely separated from the right, at a distance of nearly seven inches, the corresponding knee being considerably elevated in advance of that on the sound side: complete adduction was quite impossible, although the limb could be drawn considerably towards the axis of the body, but immediately on remitting the pressure it resumed its former position with considerable elasticity: the foot and toes were slightly everted: there was a remarkable sinking of the hip, so that the trochanter major was barely perceptible, and a considerable hardened swelling was evident

close to the pubis, beneath the adductor muscles. From the position of the limb, and other appearances, it was at once inferred, that the head of the bone was in the foramen ovale; some doubts, however, were entertained on the subject, from this circumstance, that on measuring the limb, from the spine of the ileum to the inner malleolus, and comparing it with the sound side, no actual lengthening was apparent; indeed, by some surgeons present, there was considered to be evidence of a slight amount of shortening. On placing the patient in the erect posture, at the side of his bed, the ordinary signs of displacement into the foramen ovale were rendered quite evident. Dissatisfied with the usual mode of measurement, as presenting an anomaly in this case, Mr. Adams found that when one end of the tape was applied to the dorsum of the penis close to the pubis, and was carried down to the base of the patella, there was an absolute elongation of nearly two inches. In consequence of the excoriations, resulting from the previous attempts at reduction not being quite healed, and at the urgent request of the patient for a slight respite, Mr. Adams postponed any further attempts for a couple of days: at the expiration of that period, the patient was brought into the operating theatre, and placed on his back upon a sofa, the pelvis being brought as near as possible to the edge, with a padded post directed close against the left side of the perineum: a jack towel was then applied above the knee of the dislocated limb, to which the hook of the pulleys was attached, and counter-extension was effected by a broad towel, passed under both ilia, and secured by assistants on the opposite side, with a view alone of affording complete relaxation of all opposing muscular force: chloroform was administered, and when the patient had become completely under its influence, the extension was commenced in a direction obliquely across the lower third of the sound thigh, the post forming a fulcrum, around which the upper part of the thigh was drawn, and by which movement it was hoped the head of the bone would be tilted into the acetabulum: to prevent the possibility of its slipping beneath that cavity into the sciatic notch, great care was taken during the continuance of the extension that the shaft of the bone was not raised above the level of the sound limb. Much difficulty was experienced in keeping the pelvis perfectly in the horizontal position and uninfluenced by the extending force, but this difficulty was with care effectually counteracted by the assistants. During the first attempt at reduction the man was sitting up and directed to embrace

the post firmly, but in consequence of the difficulty experienced in maintaining that position, he was, as just stated, placed in the horizontal position, whereby also the full effects of the chloroform could be more efficiently maintained: he was subsequently raised again to the sitting posture, (the extension still being continued) when on examination it was believed that the head of the bone had resumed its natural position: all extension was then immediately remitted, and as there was still some little doubt upon the subject, the lower end of the thigh-bone was carried suddenly as far as possible across the sound limb, and all apparatus being removed, it was found, on measurement, that the limb was reduced; the legs were then tied together, and the patient put to bed. The force employed during the extension did not cause any fresh accession of inflammation about the parts, neither did he suffer from any constitutional irritation, but he was kept as much as possible motionless, until eight days after the reduction, when the bandage, which had been carefully retained, was removed, and the limb was found to have a perfectly normal aspect; he was then ordered to rub it night and morning with soap liniment, and to take an occasional dose of aperient medicine. Three days after the bandage was removed from his knees, he was allowed to sit up daily in the semi-recumbent posture, which he continued to do for a week, and at the end of that period, no untoward symptoms having arisen, he was ordered a pair of crutches and directed to get up every day, and gradually bring the leg into use. To-day, (June 21st) which is just three weeks after the reduction, and seven weeks from the original accident, the man is able to walk the length of the ward, and to bear considerable weight upon the dislocated limb with no inconvenience, and his health has in no way suffered from the necessary confinement which he has sustained: he will evidently be discharged from the hospital in a few days, cured. (For the particulars of the above case we are indebted to the notes of Mr. Thomas Bell, the patient's dresser.)

Upon the circumstances attending the above case, the following clinical observations were offered by Mr. Adams:—

Gentlemen,—I am anxious to make a few remarks on this very important case, as there are some circumstances connected with it which require explanation, and especially as such cases are not of very common occurrence. Some years ago, I remember well that four dislocations of the hip into the foramen ovale were admitted into this hospital within the space of

twelve months, but since that period not more than two or three have been admitted, and the four cases to which I allude were all reduced in the same manner, and I may add with comparative facility. In the first case, which was admitted at the time I now speak of, extension was made unsuccessfully, and the surgeon who had the management of the case surrendered it as in his opinion irremediable, when, by the adoption of a plan similar to that you have just seen put into execution, the dislocation was reduced with comparative ease. Let me, however, briefly allude to the circumstances connected with the case before us, as there are some apparently anomalous symptoms which I am anxious to explain to you, and which I think I can do satisfactorily. The man is brought to the hospital a month after the occurrence of the accident, with most of the ordinary signs of dislocation into the thyroid foramen, the accident also having been caused by a fall of earth upon the left hip, while his legs were widely separated (I mention this particularly, because it is the usual position to favour the occurrence of this variety of displacement of the head of the thigh-bone). It appears that in consequence of an extensive laceration of the perineum and scrotum, together with the great tumefaction of the parts about the pelvis and hip, the nature of the accident could not be accurately made out, but after the lapse of four weeks, on the discovery of the dislocation, forcible extension was made on two separate occasions (the last while the patient was under the influence of chloroform) but without success. The man was therefore sent up to the hospital by the medical men in the country, and the symptoms, as already described, were sufficiently evident as indicative of a dislocation of the hip into the thyroid foramen, the only discrepancy arising from the fact that so far from their being any considerable lengthening, (as ascertained by measurement from the usual points) the dislocated limb was found certainly to be not longer than the sound one; and it is to that point I now wish to direct your special attention. If you will refer to the work of our great authority on Dislocations, Sir Astley Cooper, you will find, in that portion devoted to the account of this variety of the luxation of the hip, that after having described the usual cause of this accident (which, by the way, fully corresponds with the account given by the patient in the present case) and having related the peculiar appearances as indicative of this dislocation, he concludes with the following important review of the previously described symptoms. "The foot, though widely separated from the

other, is generally neither turned inwards nor outwards, although I have seen it varying a little in this respect, in different instances; *but the position of the foot does not, in this case, mark the accident. The bent position of the body, the separated knees, and the increased length of the limb, are the diagnostic symptoms.*" Now all these appearances were, in this case, found to obtain, and were very evident, except the increased length of the extremity, and I think a sufficient explanation of this fact can be afforded in the following manner: in the first place, you will observe that if a line be drawn from the anterior superior spinous process of the ileum to the base of the patella, it represents one side or base of a triangle, of which the other two sides are represented, the first, by a line drawn from the anterior superior spinous process of the ileum to the trochanter major of the femur, and the other from the trochanter to the patella. Now it is of course a well-known mathematical axiom, that any two sides of a triangle are greater than a third; therefore, the line which would, as previously stated, represent the base, will be less than the other two lines which converge at the trochanter; but as the foramen ovale is in a plane below and internal to that of the acetabulum, it follows that, when the head of the bone is thrown on to that aperture, the whole axis of the shaft is changed, the lower extremity having necessarily an inclination outwards and forwards, varying in degree, (as regards the latter particular) in different cases; the apparent lengthening being in an inverse proportion to the degree of advancement: now, from the position which the axis of the bone follows when dislocated, it is quite manifest that the bone falling from the spine of the ileum to the patella would be actually diminished, and the converging angle at the patella would be proportionately more acute. As all authors who have written on the subject of this dislocation have described the spine of the ileum as the point from which the line of measurement should be taken, and as the adoption of that plan in this particular case would not indicate the most diagnostic symptom of the accident, I conceived that this apparent anomaly might be satisfactorily explained, by assuming that the spine of the ileum was here more depressed than usual, on account of the very great tension and contraction of the abductors of the thigh, and that if some intermediate point was substituted which did not vary its relative position with the two sides of the pelvis, any actual alteration of the limb in length would be clearly indicated in the

result. Now such a point is to be found in the symphysis pubis, and would be represented in the living subject by a line passing along the centre of the dorsum of the penis: I therefore applied one end of the tape to this point, and then ascertained, as I expected, that the distance intervening between it and the base of the patella was increased by two inches when compared with the sound side.

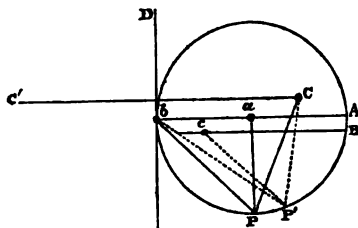
Permit me now to refer to the treatment adopted in this case, and which I consider requisite to be pursued under similar circumstances. If we look to the position of the parts about the pelvis, we shall find that the head of the bone is depressed into the foramen ovale, and that the glutæi muscles, the powerful extensors of the limb, are principally put on the stretch: but perhaps I shall better explain the condition of the parts by referring you to the account of the dissection of a similar case, given by Sir Astley Cooper, in his work on Dislocations. At p. 66, he writes:—"The head of the bone was found resting on the foramen ovale; but the obdurator externus muscle was completely absorbed, as well as the ligament naturally occupying the foramen ovale now entirely filled by bone. Around the foramen ovale bony matter was deposited, so as to form a deep cup, in which the head of the thigh-bone was enclosed, but in such a manner as to allow of considerable motion; and the cup thus formed surrounded the neck of the thigh-bone without touching it, and so inclosed its head that it could not be removed from its socket without breaking its edges. The inner side of this new cup was extremely smooth, not having the least osseous projection at any part to impede the motion of the head of the bone, which was only restrained by the muscles from extensive movements. The original acetabulum was half filled by bone, so that it could not have received the head of the thigh-bone if it had been put back into its natural situation. The head of the thigh-bone was very little altered; its articular cartilage still remained; *the ligamentum teres was entirely broken*, and the capsular ligament partially torn through. The pectoralis and adductor brevis had been lacerated, but united by tendon; the psoas and iliacus internus, glutæi, and pyriformis, were all upon the stretch."

Now what I wish especially to observe is, that in the case just related, the ligamentum teres had been completely ruptured, as this kind of dislocation is said by some to be the only one in which that ligament remains intact; and to prove to you that modern authors of considerable experience do not consider this as an inevitable conse-

quence of this accident. I need only quote an observation of Chelius, from his *System of Surgery*, where, at p. 795, vol. i., he says:—"The capsule, as well as the round ligament, is torn in every one of these dislocations, though in that inwards and downwards it may remain entire." As regards the treatment necessary to be adopted in dislocations of this description, with whatever force you apply extension, unless that extension be used in a right direction, your efforts at reduction will be totally unavailing. And you must remember, in these cases, especially when the bone has been out of its socket for four or five weeks, that muscular action will assist you but little in drawing the head of the bone into its natural position; and that you must not expect, in the return of the bone into its original socket, any evidence of such an occurrence by the jerk or click, which is so characteristic of reduction in a recent case. The great object in the reduction of a dislocation from the foramen ovale is to apply a fulcrum to the upper part of the thigh-bone; then, using the limb as the long arm of a lever, and drawing it inwards, forcibly to thrust out the superior third of the bone, by which means the head is restored to its natural position in the acetabulum without any extraordinary extension of the limb. Sir Astley Cooper advises that you should apply a girth or band (after fixing the pelvis) to the upper third of the thigh, and, drawing it outwards, it should be fixed to a staple in the wall, and that the surgeon, then grasping the ankle with his hand, should draw the limb inwards behind the opposite leg. But the most simple and effectual method is to make a fulcrum with a pillar or bed-post, or any other similar contrivance (which, I believe, was first recommended by Mr. Hey, of Leeds), and draw the limb around it, by which the head of the bone is tilted up, and the reduction easily accomplished. Owing to the lapse of time since the occurrence of the accident in the present case, we naturally anticipated unusual difficulties: but I always had an impression in my mind that this dislocation might be reduced, as I was convinced that muscular action was not the sole antagonistic power, and that if extension were made in the right direction reduction might be accomplished. And I think I am justified in saying that no extraordinary exertions were used in this case, and that the restoration of the limb was due rather to the correct application of power than to any violent extension. (Mr. Adams here exhibited the cast of a preparation, displaying an unreduced dislocation of the head of the thigh-bone

into the foramen ovale, where a new socket had been formed by a deposition of bone round the edge of the foramen ovale, forming a complete acetabulum.)

The above remarks concerning the peculiarities attending this case will be easily understood on reference to the following diagram, which has since been suggested to us by Mr. Wyatt, the late house-surgeon, for the purpose of demonstrating the alteration in the axis of the bone in this variety of dislocation, and the effects thereby produced on the usual points of measurement, as seen below:—



He assumes that the lower extremity of the shaft of the femur, during its normal movements, describes part of the circumference of a circle, the centre of which is represented by the head of the bone in the acetabulum; and that a horizontal line, passing through the entire pelvis on a level with the crest of the pubis, would pass through the centre of the acetabulum, and also divide the imaginary circle into two equal portions. That line is represented in the diagram by the symbol A: another horizontal line, on a lower plane B, passes through the centre of the foramen ovale (c), while the upper line CC' indicates the level of the anterior superior spinous of the ilia. (a) represents the acetabulum, with a line passing in the normal inclination of the thigh-bone; and D another, which, passing through the centre of the symphysis pubis (b), indicates the central axis of the body. P represents the normal position of the base of the patella; therefore aP would indicate a line passing through the central axis of the femur, and bPC the normal angle produced by the convergence of the lines drawn respectively from the superior spinous process of the ileum and crest of the pubis. Now, when the head of the bone is shifted from (a) to (c), or the foramen ovale, cP' will represent the axis of the femur in the abnormal position; and, although the lines cP' and aP must necessarily be equal, yet it is quite evident that the dotted line CP' must be less than the line CP, and that the dotted line bP

will be greater than the line δP . In consequence of the point (e) being a plane below that of the point (a), it follows that the lower extremity of the line cP' being thrown away from the perpendicular axis of the body, the point P' will approach nearer the point O , and therefore will be further removed from the central point δ . And these conclusions are found accurately to obtain on the skeleton on the following proportions :—

$CP = 21$ inches, and $\delta P = 17$ inches ;
 $CP' = 21$ " and $\delta P' = 19$ "

Upon consideration of these facts, it would naturally be inferred, that in every case of this species of displacement no actual lengthening of the limb could be ascertained to exist between the spinous process of the ileum and patella, because lines drawn from fixed points can never vary, and that could not obtain, except when distortion of the pelvis existed. He believes, therefore, that the discrepancy arises from the oblique position of that side of the pelvis, and also from the peculiar inclination of the limb (when dialocated) ; which certainly does then *assume* an aspect of apparent elongation between the points CP' , but not between the points δP , where an actual lengthening must invariably exist.

Correspondence.

THE CASE OF MR. DYCE SOMBRE.

SIR,—It will be in your remembrance that the case of Mr. Dyce Sombre received a long hearing from the Lord Chancellor, in the spring of 1849.

In the course of his elaborate and careful judgment, it fell in with his Lordship's views respecting this important case to animadvert on doctrines laid down in a report, addressed to the Lord Chancellor by the President of the College of Physicians, Sir Alexander Morison, Dr. Copland, Dr. Ferguson, Dr. Costello, and myself. Whatever our opinion might be of the estimate of these doctrines formed by his Lordship, of the extent to which they might require modification, or the clearness with which they had been propounded by us, we considered our duties at an end, when we had sent in our report to Mr. Dyce Sombre's solicitor ; and we abstained from comment. A publication has, however, since taken place, in one large volume, of all the documents adduced under the charge of lunacy, brought against Mr. Dyce Sombre, from the commencement of these proceedings to the present time.

As this book may be widely circulated, it seems desirable that as much light should be thrown, as the subject will afford, on the apparently conflicting principles contained in our report to the Lord Chancellor, and his Lordship's judgment on the case.

The subject in regard to which the public has to be defended against misconceptions is mainly involved in the following extracts—one from our report, the other from the judgment of Lord Cottenham: and however appreciated that principle may have been by his Lordship, we must, I think, admit that it has not been sufficiently developed by ourselves or by the learned counsel who argued the case. The passage in our report, after some discussion of the circumstances under which one special delusion in Mr. Dyce Sombre's mind appeared to have been formed, the only one which still appeared to us to hold possession of his mind, proceeds as follows: "It entirely falls in with our experience, that a delusion thus formed and thus strengthened should survive the insane state; and we are of opinion, that we should act with very great injustice if we should consign a patient to a mad-house on the ground of his asserting one morbid impression, when the whole context of his character indicates sanity. On this passage, Lord Cottenham animadverts as follows—"When physicians tell me, that delusion is not inconsistent with a sound state of mind, I cannot help believing that they regard delusion as a separate disease; whereas it has always been held to be the result of unsoundness of mind; the symptom merely of the disease, and not the disease itself. The delusion may exhibit itself more or less; but, so long as it exists at all, there must be some unsoundness of mind. When they tell me, that notwithstanding there is delusion the mind is sound, they appear to involve themselves in a contradiction."

It must certainly be admitted, that the proof of mental unsoundness does not rest exclusively on the proof of delusions. Incoherency of thought, and incompetency to manage property and person, will afford ample grounds for a commission. Where, however, as in the case of Mr. Dyce Sombre, presumed delusions constitute the gravamen of the charge that unsoundness of mind is still persisting, it becomes of immense importance to the public that the sense in which these last quoted remarks of Lord Cottenham are true in regard to delusions, should be distinctly appreciated. The following is, I believe, the doctrine of medical philosophy on this subject. A delusion may be considered as involving or implying mental unsoundness, when it is either a hallucination, *i. e.* a false perception of the senses, or a perversion of the understanding analogous to this; which latter state it is

easier to exemplify than to describe. Of the first head we have repeated instances in those who hear unreal voices and sounds, and have waking dreams of visual objects: in each case acting or reasoning off their false impressions. The wayward misconceptions of the nature and value of property, of the possession of rank, of power, and even in regard to sex, which are met with in the occupants of lunatic asylums, are instances of the second kind of delusion, supposing them similarly operative on conduct. Now with these meanings of the word delusion, in which alone the Lord Chancellor's supposition is tenable, namely, that "the existence of delusion is inconsistent with soundness of mind," we were unable to bring the delusive opinions of Mr. Dyce Sombre into any conformity or agreement. They appeared to us such, indeed, as he might have conceived, under the circumstances in which he was placed, in a healthy state of mind; and we saw no more reason for expecting that any of them should disappear with his insanity, than we might have to expect any other association, with which his mind might have been familiarised during his morbid state, to disappear at its close. For myself I should almost be inclined to doubt whether the fearful visitation of insanity had really taken place, in one on whose mind no traces of the storm were discoverable in after life in the form and texture of his associations; and this, sir, is the sense in which, I imagine, we wished to be understood, in the passage above quoted from our report, in which we speak of "those morbid impressions which do not imply unsoundness where the whole context of the character indicates sanity." Endeavouring to apply to the case before us the distinction above laid down in reference to delusions, we found no evidence that the visual mistakes of Mr. Dyce Sombre were false perceptions; and with respect to the most important of his assumed intellectual delusions, we found it impossible to place in the same category the Englishman who may fancy himself an angel, a griffin, or an emperor; and the Rajah who, happening to have formed a very low opinion of the pecuniary honesty of the East India Company, is pleased to impute to them an act of stercidity, of another kind indeed, but equally conformable with his notions of vice.

It is not my purpose to defend the opinion thus formed by us as to the imputed delusions of Mr. Dyce Sombre, but to point out that the principle on which it is formed is not, as Lord Cottenham appears to think, irreconcilable with that other principle which his Lordship affirms, that, "as long as delusion exists, there must be unsoundness, the origin of its existence,"—

provided that the term delusion is used by him in a specific sense as applicable to questions of mental unsoundness. Meanwhile, that very liability to survive the disease, which attaches to the *other* delusions of the insane because the removal of the unsoundness of mind does not imply their removal, gives additional importance to the distinction which I am endeavouring to enforce. If it be ignored, the consequences may be indefinite detention or privation of rights to any one, who having been of unsound mind, has not subsequently learnt the necessity of dissembling or concealing all such notions and opinions as may replace him in the more disagreeable entanglements of constructive insanity. Nor is this misconception confined in its detrimental consequences to the civil position of the suitor in Chancery for a superedeas. It involves in a criminal phase of the subject, much detriment and danger to the public. Thus, if the opinion entertained by Mr. Dyce Sombre in regard to the East India Company, as interfering in his family concerns, constitutes him of unsound mind and incompetent to manage his person and property, it must equally afford him the irresponsibility of mental unsoundness, if he should choose to inflict an act of violence on any one of his presumed enemies in the Honourable Board, under that singular modification of freedom which the law seems to allow him, viz. the power to do mischief. His immunities derived from his unsoundness must be commensurate with the privations which it entails on him. I may generally observe, (for I am anxious not to enter into the details of this unhappy and strangely-managed case), that the constructive imputation of insanity applicable to all persons who have been once insane, under a misconception of the nature of insane delusions, is liable to furnish an indefinite number of cases of ex-lunatics retained under some form of coercion or restriction long after their disease has subsided; and again of ex-lunatics allowed to escape the penalty of crimes committed after their enlargement or presumed recovery, on the ground that they have once been of unsound mind, and still are in possession of some of the opinions which they then entertained.

I am not aware that the leading idea which pervades this letter is at variance with the sentiments of my colleagues in the report on Mr. Dyce Sombre's case in 1849; but I ought to admit, that I have not communicated it to any one of them, and am therefore exclusively responsible.

Yours faithfully,

THOMAS MATO.

55, Wimpole-Street,
July 5th, 1850.

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Medical Intelligence

PROPOSED CHANGES IN THE APOTHECARIES' ACT—MEMORIAL TO SIR GEORGE GREY.

To the Right Honourable Sir George Grey, Bart., Her Majesty's Principal Secretary of State for the Home Department.

Apothecaries' Hall,
June 26, 1890.

SIR,—Referring to the memorial which I had the honour of laying before you on behalf of the Society of Apothecaries on the 4th ultimo, I am now desired by my colleagues to submit for your consideration the following suggestions for an amendment of the Apothecaries' Act in several particulars, in which a change in the existing law would, in the opinion of the Society, be attended with beneficial results, not only as regards the class of practitioners in this country whose education and examination the Act more immediately regulates, but also other classes of practitioners, who, not having pursued the course of study, or submitted to the examination prescribed by the Act, may still be desirous of practising as apothecaries in England or Wales.

Before proceeding, however, to state the alterations which the Society's experience leads them to suggest in the Act of 1815, I have been requested to notice one or two objections which have been urged to the Society of Apothecaries being entrusted with the superintendence of the education and examination of that class of medical practitioners.

The Society of Apothecaries, it is well known, is one of the municipal corporations of the city of London; and it is objected that it is unbecoming that the important functions which devolve upon the Society should be administered by a city guild. It is further objected that the Society of Apothecaries is a trading body, and is on that ground unfit to be entrusted with the performance of duties confessedly of a scientific character. And a further ground of objection to the Society is the name under which the holder of its certificate of qualification acquires the right of engaging in medical practice,—namely, that of the "apothecary."

As regards the first of these objections, it is to be observed that the Society of Apothecaries, which was then, as now, one of the municipal corporations of the city of London, was in the year 1815 selected by the Legislature, with the acquiescence of the Royal Colleges of Physicians and Surgeons, as a fit and proper body to be entrusted with the administration of the Act

in question, the Government having refused to constitute a fourth body for the purpose. That the selection of an existing municipal corporation for the discharge of such duties as the Act contemplated (whether wise or unwise) was certainly in accordance with established precedent, both in this country and elsewhere. The Surgeons' Company of London, which had been entrusted with duties in connexion with the science of surgery analogous to those confided to the Society of Apothecaries in connexion with the science of medicine, although separated from the Barbers in 1745 (more than a century and a quarter after the separation of the Apothecaries from the Grocers), was constituted a distinct City Company, and so continued till the beginning of the present century, when an accidental dissolution of the Company, by the sudden death of the Master on the day of election, led to the re-incorporation of the members of the old Company under the name of "The Royal College of Surgeons in London," and its connection with the Corporation of London then ceased. The Royal College of Edinburgh, also, "as the Surgeons' or Chirurgens' Craft," until within the last few days, formed one of the fourteen incorporated trades of the city of Edinburgh; and a Bill, introduced into Parliament in the present session, has just received the royal assent, one of the objects of which is to dis sever the connexion existing between the College and the incorporated trades municipal corporation of the city of Edinburgh.

With respect to the objection that the Society of Apothecaries is a trading body, much misapprehension exists. The Society in its corporate capacity is not a trading body, nor are the individual members of which the Society is composed united in the prosecution of a joint trading. No portion of the funds or property of the corporate body is embarked in trade; and no individual member of the corporation, in his character of a member of the corporation, is, or can be, benefited, directly or indirectly, by the operations which are carried on at Apothecaries' Hall. The facts are these:—Within a few years after the incorporation of the Society, certain of its members, finding great difficulty in purchasing unadulterated drugs in the market, formed a plan for supporting a Dispensary of their own at Apothecaries' Hall. This was followed in 1676 by the formation, by subscription, of a Chemical Laboratory, for the purpose of supplying the subscribers with such chemical preparations as they might require in their medical practice as apothecaries; and it appears that the supply was accordingly for some years confined to the subscribers and to the other mem-

bers of the Society. This restriction, however, was afterwards removed, and the public were permitted to purchase such preparations as they might require. In progress of time the undertaking extended itself; and at the present day the Navy, the East India Company, and numerous public institutions, derive their supply of drugs and chemical preparations from the laboratories at Apothecaries' Hall. The capital, however, with which this undertaking is carried on, is provided by a *limited* number of individual members of the Society, who are, in fact, associated together in partnership (undoubtedly under the partial control of the corporate body): but the number of such partners is considerably less than the number of the members of the Society at large; and the profits arising from the undertaking are, of course, divided among the individuals composing the partnership, and not among the members of the Society generally.

The real answer, therefore, to the objection is, that the Society is *not*, as a public body, engaged in trading operations, nor are the individual members of the Society whose capital is embarked in the undertaking I have described, traders, in the sense of men who devote their *personal* time and energies to the prosecution of a trade; but they are individuals who, being themselves engaged in the practice of the medical profession, are at the same time promoting, through the agency of competent officers, an undertaking which has for its object the production and supply of the various remedial agents of which the medical practitioner avails himself in his treatment of disease.

An objection is occasionally urged to the name of "apothecary," as designating rather a compounder of medicines than a medical practitioner qualified to treat disease. The Society have no particular partiality for the name, if a better can be found; but they feel that there are advantages in adhering to an old name, which ought not to be lightly abandoned. The functions of the English apothecary as a medical practitioner are now clearly and distinctly ascertained. Successive acts of the Legislature, followed by a course of judicial decisions, have determined his status in the medical profession, and established his legal claim to fitting remuneration for his professional services. The law forbidding the practice of an apothecary by unqualified persons, though long difficult of execution, from the cumbrous machinery by which it was put in motion, can now be expeditiously and economically enforced through the comparatively simple process of the county court; and, more than all, the apothecary of the present day (what-

ever may have been the claims to medical skill of the apothecary in earlier times) is recognised as a *well-educated medical practitioner*, thoroughly competent to the discharge of the duties which devolve upon him, and who has given satisfactory evidence of such competency before entering upon practice. There seems little temptation, therefore, to change an ancient name, which has, happily, become identified with an honourable and scientific calling.

I now proceed to state the alterations in the Act of 1815 which the Society of Apothecaries respectfully suggest for your consideration.

The Act requires that the members of the Court of Examiners should be elected from Members of the Society of not less than ten years' standing. The Society propose that in future all licentiates of the Society of ten years' standing (whether members of the Society or not) shall be eligible for seats in the Court of Examiners, and that not less than one-half of the members of the Court shall be licentiates who are *not* members of the Society.

The Society propose that power should be given to the Court of Examiners to divide the examination for their certificate into one or more examinations at proper intervals, with a view to relieve the student from the inconvenience of being subjected to an examination in all the branches of study on which he has been engaged on one and the same occasion.

At present the fee paid for a certificate of qualification for practice in London is ten guineas; and for practice in the country, six guineas. The Society propose that the fee should be equalised for the whole kingdom.

The Court of Examiners are at present required to hold their meetings at Apothecaries' Hall. The Society suggest that this restriction should be removed, and that the Court should be authorised to sit in any building which may be provided for them, not being beyond a limited distance from the General Post-office.

No person is admissible for examination, under the present act, who has not served an apprenticeship of five years to an apothecary. The Society propose that the service of an apprenticeship shall be no longer compulsory on the student.

The Society propose that there should be a registration of all persons admitted to practise as apothecaries, after examination by the Court of Examiners.

That all persons holding a degree in medicine, granted by any British University, and all members of Scotch or Irish Colleges which are authorised by law to grant a license or certificate of qualification to practise in medicine after examination,

such license or certificate conferring the right to practise medicine in those countries, should be admitted to registration, without further examination, on payment of a small registration fee.

That all surgeons, and assistant-surgeons, in the Army, Navy, and East India Company's Service, should be admitted to registration after not less than five years' actual service.

The Court of Assistants of the Society of Apothecaries, in whom the control of the Society is vested, is a self-elected body. The Society would not be indisposed to adopt any change in the mode of election of that body which might, upon due consideration, be thought advisable; but the subject has been found one of considerable difficulty. An election by the Members of the Society at large appears fraught with numerous objections, and the creation of a limited class for the purposes of the franchise has been found in practice to be productive of great dissatisfaction. The society, in truth, are not prepared at the present moment to suggest any specific change in the mode of election of their governing body; but they would be willing to give the most favourable consideration to any alteration in this respect which you might recommend for adoption.

I have the honour to be, sir,

Your most obedient humble servant,

J. B. EYLES,

Master of the Society.

MEDICAL REFORM. REPORT OF THE SOUTH-EASTERN BRANCH OF THE PROVINCIAL ASSOCIATION.

[REPORT presented by the Committee of the South-Eastern Branch of the Provincial Medical and Surgical Association, at the annual meeting at Guildford, and referred for consideration to a special general meeting of the members of the branch, held at the Town Hall, Reigate, on Wednesday, the 10th instant; at which meeting it was approved and adopted, with one dissentient only.]

The Committee of the South-Eastern Branch of the Provincial Medical and Surgical Association, in presenting their Report to their brother members assembled at this annual meeting, consider that they would be deficient in their duty did they not offer a candid and impartial statement, rendered as concise as possible, of the present state of the public affairs of the profession, more especially as affecting gentlemen engaged in general practice.

Their brother members may be assured, that at all times, and more especially at the present important crisis, the laws by which medical practitioners are governed are of superior importance to every other

consideration, as having a paramount influence, not merely on their political, but as affecting their moral and social as well as their professional welfare.

With respect to the subject of the reform of our medical polity, we might feelingly wish to say—"Would that it were settled!" but from its vital and incalculable importance, so far as the general practitioner is concerned, we cannot shrink from the consideration of it.

However much we might wish to avoid or evade it, we must all feel that in the present state of the question it affects most nearly and deeply all concerned in general practice, as well as the public at large. The obligations and duties which are attached to the subject are therefore imperative upon us.

As members of the commonwealth, as well as members of the medical profession, any system of public measures affecting the profession is as much a question of *personal duty*—as much concerns the consciences of individuals, as the determination of any doubt relating to the conduct of private life.

It has been said, and with much truth as regards individuals, "that every man makes his own place, and finds his own level in the profession and in society"—and certainly, gifted men endowed with the force of genius, talent, and industry, have, as a necessary consequence, risen to eminence under difficulties; but, in legislating for the whole profession, we are bound to conclude that the practical usefulness and elevation of character of the great majority, depend on the laws which regulate the education, preliminary and professional, of every individual candidate for medical practice.

The universal principles, therefore, of medical polity are not idle or fanciful speculations, but truths from which must be educed the educational processes which constitute the characteristics of the accomplished, able, and skilful practitioner—not merely of the favoured few, but of all who practise the divine art of healing, whether exercising it in our great metropolis or large provincial towns, or in remote and thinly-peopled districts.

After many and often-repeated applications, and solicitations almost humiliating, from the various representatives of the general practitioners, the Council of the College of Surgeons have rejected all propositions, and have repudiated the deliberate and solemn engagements of their delegates, to the great disappointment of the other parties to the conference committee at the College of Physicians.

After these anxious and hopeless endeavours, therefore, the general practitioners must come to the conclusion of the utter impossibility of rendering the College of

Surgeons what has been appropriately and emphatically termed their "head and home."

The Society of Apothecaries have deserved well of the profession and of their country, from the able and efficient manner in which they have carried out the provisions of the Act of 1815. Nothing could surpass the caution, judgment, and ability with which their Court of Examiners have gradually raised their scale of required attainment in candidates for their certificate of qualification, and for which we are infinitely indebted to them.

Your committee therefore trust that they will firmly adhere to the determination which they have expressed, namely, never to resign their powers under that Act until an incorporation of the general practitioners can be effected, which, as able and enlightened men, they judge to be equal to their wishes and expectations, and all-sufficient to render the future general practitioners fully competent to meet and treat successfully all the extraordinary, as well as ordinary, emergencies of medical and surgical practice.

Under all the circumstances of the present state of the case, your committee do not hesitate to recommend, as of pre-eminent importance and desirableness, the incorporation of the general practitioners in an independent College, which, to the credit of the Council of "The National Institute of Practitioners in Medicine, Surgery, and Midwifery," has been advocated by them with the greatest truth and clearness of argument, a steady perseverance, a calm dignity, and an incomparable temper, for a long course of time, and under many circumstances of difficulty and discouragement, so as richly to deserve the warm approbation of the whole body of general practitioners.

With the same steady and unflinching courage these gentlemen will persevere until the accomplishment of a separate college is complete; but at the same time they call upon the general practitioners at large to afford them their cordial support in their meritorious exertions, and throughout the country to co-operate, and, with united action, to interest their influential and parliamentary friends in favour of a new and independent college, assured as they must be that it is the only practicable organization by which their interests, and the interest of the whole population of the country, as respects their health, can be secured.

Your committee, therefore, have no hesitation in submitting for adoption at this meeting the following propositions, as unquestionable, as well as important truths.

1. That medicine, surgery, and midwifery, are departments of the same profession, which are inseparable in an educational and

scientific point of view; and in the exercise of the profession, they are equally inseparable for nine-tenths of the practitioners of the healing art.

2. That the practitioners of the three branches of the profession are the medical advisers, in ordinary, of almost the whole population of the country, whose health, comfort, and well-being depend on the competency and skill of those to whom they look up for counsel and assistance under the various forms of disease which occur.

3. That every defect in the medical institutions of the country, or any line of policy calculated to retard the progressive improvement of the general practitioners, or to depress the standard of their qualification, or to diminish their scientific and practical attainments, or in any way to lower their status in society, will have the effect of debasing the character of the profession in this country, and is fraught with incalculable direct and consecutive evils to society at large.

4. That in consequence of the failure of negotiations which have been going on for a great length of time, and the occurrence of recent events, your committee entirely despair of being able to obtain any alteration or modification of either of the existing Colleges, so as to acquire the means of securing a complete and scientific medical and surgical education for the general practitioner, and entertain the strongest conviction that the objects of medical reform, so far as respects him, can now be certainly attained only by the establishment of a new College on the representative principle, distinct and different from either of the existing institutions, which shall provide for the education, qualification, and examination of all its future members.

5. That the accomplishment of such a measure would develop the energies of the medical profession to an extent hitherto unprecedented, and would sustain the usefulness and respectability of its practitioners, in not only promoting the science and art of medicine and surgery, but also, as bearing on public health and sanitary improvement, would be one of the most direct and efficient modes of ameliorating and preventing many physical and social evils which are found to prevail both in town and country.

The establishment, therefore, of a new College of general practitioners on the representative principle, by royal charter, confirmed by Act of Parliament, providing an efficient control over the education of members of that class, so that their progressive improvement in preliminary knowledge, in the collateral as well as medical sciences, tested by sufficient examination

would secure the true respectability and usefulness of the great body of practitioners, to whose skill and judgment the limbs and lives of the great mass of the population are confided.

It is with much reluctance your committee desire to recall the attention of the meeting to several recent leading articles in your journal. The gentlemen who have in other respects so ably conducted the general management of that periodical have, it appears to your committee, not written their leaders, referring to the question of medical reform, with that candour and fairness which the numerous members of the Association who differ from them had a right to expect. The result of the opinion of the special branch meetings may warrant the conclusions they come to; yet those opinions, which have been gathered under peculiar circumstances, might have been promulgated in a way less likely to disturb that harmony which it is the object of the association to maintain.

There are three prevalent fallacies which, *a priori*, are so obviously improbable, that your committee would not have thought of advertizing to them, but that they appear to exist in the minds of some general practitioners.

One is, that in the event of the establishment of a new College, they would lose, or be obliged to lose or relinquish, the title of "surgeon," which, in the first instance, is manifestly unlikely, and is actually untrue.

The second is, that we entertain a desire to supersede the College of Surgeons, which would not be possible, did we wish it ever so much.

We all acknowledge the advantage of consulting surgeons, as well as consulting physicians, and with perfect friendliness and cordiality of good feeling do we view the existence and the prosperity of that as a special College.

May it long enjoy a prosperous career, directed by liberal and generous principles—a high degree of public virtue—an enlightened, and not a low and gross selfishness!

And may its diploma be the reward of merit, and be justly appreciated as being rather difficult, instead of continuing to be too easy of attainment!

The third is, the fear of a supposed inferiority of the new College. Whereas, a College comprehending the practitioners of all the three branches of the profession, all the sections of the healing art, with high qualifications in every department, must, surely, be as dignified and as elevated in its attributes as any College taking into its study and qualification of membership one section only, and a supposed practical

exercise of one division only, of the profession.

With respect to the benevolent fund of the Association, its able and indefatigable advocate will not fail to be present on this occasion.

Your committee, therefore, will be content with recommending it to your kind consideration, and of again submitting the often-repeated suggestion, that the small annual contribution of five shillings from each member would form, altogether, a handsome income.

With regard to the subject of poor-law medical relief, your committee regret that they cannot announce any progress; and in this matter, perhaps, more than in any other, is the want of a real and effective representation of the wants of the medical profession most keenly felt.

Grievances of a pressing nature, acknowledged by every one who has investigated the subject, weigh upon a large portion, numerically, of medical practitioners; and, although the constituted authorities of the profession express sympathy with their suffering members, no authoritative or energetic remonstrance is offered by them to those branches of government with whom the redress of these grievances should be a matter of duty.

Could such a state of things endure if the interests of the profession were in the hands of an energetic body, sincerely anxious for the good of the great mass of their constituents?

Your Committee have no hesitation in stating that, in their opinion, active and repeated efforts should have been made by the Colleges of Physicians and Surgeons to obtain for the three thousand poor-law medical officers that justice which is so notoriously denied to them.

And your Committee as fully believe that such efforts would have been crowned with success.

In the absence of an authoritative adoption of their cause by the recognised heads of the profession, the poor-law medical officers have remained disunited—their efforts feeble—their success null; and this great opprobrium to a rich, and considering itself an enlightened country, as well as to a profession daily claiming more and more of a scientific character, remains undiminished, with some of its evils even aggravated.

For the convenience of gentlemen residing in different parts of the district, it has been usual to hold the annual meetings in a sort of order of succession, which will give the preference next year to East Kent.

The town of Folkestone, by the convenience of its railway communication, and as being the birth-place of Harvey, your

Committee consider to be an appropriate place of meeting for next year, and beg to recommend it to the adoption of the members present for the assemblage of 1851.

JAMES STEDMAN,
Chairman of the Committee.
THOMAS MARTIN,
Secretary.

COPY OF A RESOLUTION adopted, with one dissentient only, at a special general meeting of the South-Eastern Branch of the Provincial Medical and Surgical Association, assembled at Reigate on the 10th instant:—

“Moved by Mr. Smith, of Crawley, seconded by Mr. Dulvey, of Brompton, Chatham, and

“Resolved—That the report which has been adopted be inscribed in the minutes, and printed for circulation, as has been usual on former annual occasions.

“And that the editors of the medical journals be respectfully requested to favour this meeting with the insertion of the Report in their next number.”

Reigate, 11th July, 1850.

PROPERTIES OF THE WATER OBTAINED FROM WENHAM LAKE AND NORWEGIAN ICE—A SUBSTITUTE FOR DISTILLED WATER.

IN confirmation of the observations of Mr. Faraday on the purity of water obtained from Wenham Lake and Norwegian ice,* a specimen was lately submitted to examination, with the following results:—The water was colourless, tasteless, and rapid; it was free from acid and alkaline reaction; was not affected or rendered turbid by nitrate of baryta, oxalate of ammonia, nitrate of silver, ammonia with phosphate of ammonia, or lime water. It gave only a slight opacity after some minutes with acetate of lead, but this was no greater than the effect produced by the same reagent on distilled water.

Tried with the soap test, it marked 0° of hardness; and, when a sheet of polished lead was plunged into it, a white deposit of hydrated oxide and carbonate of lead was formed in an hour. These facts show that the water derived from pure ice is absolutely pure, and may be used in remote places as a substitute for distilled water when this cannot be readily procured. This suggestion may be of use to those who have hitherto found a difficulty in procuring an abundant supply of distilled water for the purposes of chemical research.

The rapid action on lead is in conformity to general experience, and is a strong indication of the great purity of ice-water. The

fact that white lead is so abundantly produced by the contact of this metal with ice-water may be of some importance in relation to health, since it would render it unsafe to employ refrigerators lined with lead for the purpose of preserving the Wenham Lake or Norwegian ice.

It should be observed that the water here experimented on was derived from ice absolutely transparent and pure, without air-bubbles. Some kinds, when melted, produce a kind of water which has a slight opacity, obviously from the presence of impurity.

CONFESSION OF PROFESSOR WEBSTER.

LETTERS from the United States announce that Professor Webster has confessed the murder of Dr. Parkman. He states that the deceased went to his (the professor's) room on the fatal day to receive some money owing to him by the prisoner; that he provoked him without measure by taunting reproaches and threats, thrust his fist in his face, and called him liar, scoundrel, &c., and that in the heat of passion thus roused he (prisoner) dealt a sudden blow at the deceased, which almost immediately destroyed his life. His confession, which is very long, adds, that the thought of proclaiming the deed, and explaining the circumstances, never entered his head, but that he at once determined on concealment, and took measures for that purpose with singular composure and skill. He confesses to having afterwards cut up the body, and, having burnt several parts, thrust the trunk into a sink. The confession seems to be pretty generally believed, but it is thought that it would have been of more avail to him if it had been made immediately after the event. He solemnly declares that he never for a moment premeditated the murder, and asserts that his temper has been always uncontrollable.

* * We have hitherto delayed giving a report of this remarkable trial, owing to the crowded state of our columns. We shall, however, take an early opportunity of publishing the evidence in an abbreviated form.

APOTHECARIES' HALL.

NAMES of gentlemen who passed their examination in the science and practice of medicine, and received certificates to practise, on Thursday, 11th July, 1850:—Edward Simpson, York—John Todd, Rothbury, Northumberland—William Charles Hille, London—Henry Noble Watson, Clitheroe, Lancashire.

* See our last volume, page 1066.

Selections from Journals.

EPILEPSY FROM PRESSURE UPON THE BRAIN.
(CLINIC OF JEFFERSON MEDICAL COL-
LEGE.) REPORTED BY MR. JAMES A.
MEIGS, STUDENT OF MEDICINE.

A CASE of considerable importance in surgery was presented by Professor Pancoast to the class of Jefferson Medical College, on Saturday, January 13th, 1849.

The patient, a lad aged 14 years, had, about nine years previous, received a severe blow upon the sinciput, just over the left orbital ridge, by being precipitated from a cart upon a pile of stones. He was taken up insensible; but, under judicious treatment, recovered, and was to all appearance perfectly well. Some time after, when the circumstance was almost forgotten, the patient was suddenly seized with epileptic fits, a disease with which, prior to the accident, he had never been troubled. These untoward symptoms gradually increased in frequency and violence, until it was not uncommon for them to recur ten and even twenty times per diem.

Coincidentally with this epileptiform condition, a slow but progressive decay of his mental faculties became evident, till it was finally feared a total alienation of his mind might supervene. As indicative of this, his features were impressed with the peculiar fatuous expression of confirmed epileptics, while his whole conduct evinced a moody and abstracted state of mind.

These abnormal symptoms had thus far been steadily increasing in magnitude and violence, despite the various and well-directed remedies employed, when the patient was placed under the charge of Professor Pancoast, who, after a careful investigation, both of the history of the case and the condition of the lad, became convinced that the evil resulted from the pressure of a portion of the vitreous table of the os frontis upon the anterior lobe of the left cerebral hemisphere. This projection of the bone he thought had been undoubtedly established at the time the accident occurred, but had not manifested itself by its alarming results,—the child being then very young,—until the brain became considerably developed.

Here, then, was an extremely delicate point for the formation of a diagnosis, and the establishment of the consequent treatment. The question forcibly presented itself, whether to operate or not. If the meninges of the brain were inflamed, or the orbital plate broken, it was obvious that no benefit would accrue, and the patient be needlessly subjected to a painful operation.

Again, if the frontal sinuses existed to any extent, the danger was manifest of forming cranial fistula, which would be extremely difficult, if not impossible, to cure.

Notwithstanding these manifold obstacles, the operation was resolved upon, inasmuch as it seemed to give the lad the only chance for his recovery.

His father assenting, the lad was brought before the class on the 17th of January. He was placed upon a table in the clinical room, and as a return of his paroxysms during the operation was feared, he was held firmly down by several assistants. A sort of triangular opening was made, the flaps of which being turned back, the pericranium was exposed. This was divided, and the branches of the supra-orbital and frontal arteries, the hemorrhage from which was considerable, were taken up. The trephine was now applied immediately above the superciliary ridge, and as near the depression as possible. Extreme caution was necessary at this point of the operation, this being a difficult and dangerous place for the application of the trephine. In this case the danger was increased by the incessant struggling and resistance on the part of the patient. A circular piece of the skull was removed, having upon its inner face a spiculum of bone pressing upon the dura mater; thus triumphantly verifying the diagnosis. The dura mater was perfectly healthy, presenting its usual opaque pearly hue.

The edges of the periosteum being brought together, and the flaps laid down and supported by a compress of wetted lint, lightly held in its place by adhesive strips, the patient was transferred to one of the clinical wards of the institution. Here he remained during the ensuing month, under the attendance of Drs. Rand and Horner.

For some time after the operation, it was frequently noticed as a fact worthy of consideration, that any attempts to approximate the lips of the aperture closely, and thereby dispose them to heal at once, were speedily followed by a return of the epileptic paroxysms, which were as readily dissipated by the immediate removal of the approximating force. The same disagreeable results were also found to be induced by the slightest indulgence in any highly nutritious or stimulating aliment.

In addition, therefore, to cold applications to the head, absence of light, and the scrupulous avoidance of all anodyne preparations, which were resorted to immediately after the operation, the lad was kept upon a spare diet, and the aperture allowed to remain open for nearly a month. The judicious nature of this treatment was soon made manifest by the happy restoration of the lad to mental and physical health.

The scar necessarily left by the operation is scarcely perceptible, while the aperture is filled up with a cartilaginous deposit, as is evident from the resistance offered upon pressure.

The lad is now (August 1849) employed by his mother to run errands, and attend occasionally to a little store which she keeps in this city. In his daily conduct he evinces an intelligence and physical strength usual to lads of his age and condition in life.—*Phil. Med. Examiner.*

PATHOLOGICAL ANATOMY OF CHOLERA.

MICROSCOPIC examination has shown, as being constantly present in this disease, an abnormal state of the intestinal follicles, with or without enlargement, and a more or less vivid injection of Peyer's glands. At the hospital of Val de Grâce, besides these morbid changes, there were found, in the young soldiers who have died of algide cholera, ecchymoses in the parenchyma of the liver, spleen, kidneys, and lungs. These ecchymoses appeared not only on the surface, but also in the interior, and seemed to result from a combination of the blood with the tissue of the organs. In the large intestines, these ecchymoses have been found so extensive and deeply coloured, as to resemble gangrenous patches.—*L'Union Médicale.*

WOUND OF THE BRAIN—RECOVERY. BY WILLIAM KENNEY, M.D.

THE subject of this case was a lad 17 years of age, who received a stab with a common pocket-knife, blade two inches and a half in length, and three-quarters of an inch in width, tapering abruptly on back and edge to a point. The wound was in the left temporal region. The knife was driven with such force as to penetrate the brain the full length of the blade, at a point midway and about three-quarters of an inch above a line drawn from the external angle of the eye to the *meatus auditorius internus*. The handle of the knife, as it stood, looked slightly forwards and upwards, and was so firmly fixed between the divided bone that it was with great difficulty that the knife was removed. During its presence in the brain, and after its removal, the patient complained of great pain in the left eye, and over the frontal region of that side. Its removal was followed by hemorrhage, to the amount of ten or twelve ounces; vomiting and stupor. The patient was taken from the street, his wound washed, and without farther dressings being applied, he was placed in bed, with the head raised, and ordered the free use of cold applications to the scalp; sinapisms to the extremities; twenty grs. calomel; rest and quiet. Under this treatment, in less than three weeks he entirely recovered.—*American Journal of Med. Sciences*, Jan. 1850.

BOOKS & PERIODICALS RECEIVED FOR REVIEW.

(The List will be given in our next No.)

BIRTHS & DEATHS IN THE METROPOLIS During the Week ending Saturday, July 13.

BIRTHS.		DEATHS.	
Males....	608	Males....	305
Females..	637	Females..	386
	1245		781

CAUSES OF DEATH.

ALL CAUSES	781
SPECIFIED CAUSES	760
1. <i>Zymotic</i> (or Epidemic, Endemic, Contagious) Diseases....	166
<i>Sporadic Diseases</i> , viz.—	
1. Dropsy, Cancer, &c.	36
2. Brain, Spinal Marrow, Nerves, and Senses	104
4. Heart and Bloodvessels	21
5. Lungs and organs of Respiration	57
6. Stomach, Liver, &c.	50
7. Diseases of the Kidneys, &c.	11
8. Childbirth, Diseases of Uterus, &c.	11
9. Rheumatism, Diseases of Bones, Joints, &c.	8
10. Skin	1
11. Old Age	35
12. Sudden Deaths	7
13. Violence, Privation, Cold, &c.	12

The following is a selection of the numbers of Deaths from the most important special causes:

Small-pox	9	Convulsions	27
Measles	16	Bronchitis	20
Scarlatina	23	Pneumonia	42
Whooping-cough	23	Phthisis	116
Diarrhoea	37	Lungs	4
Cholera	7	Teething	11
Typhus	30	Stomach	6
Dropsy	7	Liver	16
Hydrocephalus	26	Childbirth	5
Apoplexy	29	Uterus	5
Paralysis	18		

REMARKS.—The total number of deaths was 105 below the average mortality of the 26th week of ten previous years. The only point worthy of remark is, that there were seven deaths from Cholera. The disease, however, was the common English epidemic, and not the malignant form.

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer 29.77
 " " Thermometer " 59.5
 Self-registering do. " Max. 110° Min. 34°
 " From 13 observations daily. " Sun.

RAIN, in inches, 0.52.—Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was 1°.5 below the mean of the month.

NOTICES TO CORRESPONDENTS.

Description of a new Method of converting Gas, &c. into Fuel for Fire: with some Observations on its bearing upon the Sanitary Condition of Towns, by D. O. Edwards, Esq. M.R.C.S. has been received.

The letter of Mr. B. W. Richardson shall appear next week.

The long document on Self-supporting Dispensaries can only be inserted as an advertisement. Communications have been received from Dr. Hassall, Mr. Girdlestone, and Mr. Galloway (No. 2).

Lectures.

LECTURES
ON THE
TREATMENT OF DELIRIUM AND
COMA;

(IN SEQUEL TO THE LUMLEIAN LECTURES
FOR 1850.)

Delivered at King's College Hospital,

By R. B. TODD, M.D., F.R.S.

(Reported by Mr. LIONEL S. BEALE, Med. As-
sociate K.C.L.)

LECTURE VI.

Hysterical delirium in men.—Treatment of epileptic delirium—Diagnosis of epileptic delirium—Epileptic delirium a prolonged epileptic paroxysm—can it be cut short? Bleeding—Moral treatment—Treatment of coma—Diagnosis of apoplectic coma.—Treatment of traumatic coma, of epileptic coma, and of the other varieties of coma.

GENTLEMEN,—In my last lecture I directed your attention to a case of hysterical delirium, now in King's College Ward; I refer to this case now, in order that I may tell you that this patient is going on very favourably under the plan of treatment which we have adopted; namely, paying due attention to supporting her strength with nutritious diet, and isolating her from other patients in the ward by placing screens round her bed, at the same time exercising a firm but mild moral influence upon her, without mechanical restraint.

I must here allude briefly to the treatment of that form of delirium which we now and then meet with as apt to occur in men who have been very hard-worked: over-worked, either as students, or in their professions or other callings, or who have drawn too largely on their powers by habits of dissipation, especially when sexual excesses have been conjoined with them. In such persons, I say, we now and then meet with a form of delirium which often passes under the name of brain-fever; this delirium is closely akin to that of hysteria in women; indeed, the state into which men bring themselves by overtaxing their powers is very similar to, if not identical with, the hysterical state in women, and the delirium which is developed in the course of it is therefore most probably of precisely the same nature.

The occurrence of a delirium of this kind in men forms an interesting feature in reference to the pathology of hysterical affections, inasmuch as it clearly shows that the hysterical is not a condition especially connected with the peculiar characters of the female sex, but that, under circumstances

analogous to those which are apt to produce it in women, a similar state may be brought on in men.

Men who addict themselves to masturbation, or to excessive sexual indulgence, become affected with nervous symptoms precisely of the same nature as those which are so common with hysterical women. They become effeminate in appearance and habits; and often, if I may so speak, more effeminate even than women. In such men you may witness the hysterical paroxysm as complete and as intense as any that may be seen with women. Men of strong sexual passions, even although they may be kept under proper restraint, when exhausted by other means, as by undue mental exertions or anxiety, likewise become hysterical, although to a less extent than when the exhausting cause has been sexual.

The hysterical states, then, in both men and women, may be regarded as pathologically the same. The very same rules of treatment which I have laid down for the management of the hysterical delirium of women are applicable to that of the hysterical delirium of men; namely, careful attention to the support of the patient's general nutrition and to the state of his digestive organs, moral management, isolation as far as may appear advisable according to the particular circumstances of the case, which also, rather than any general rules, must guide you as to the use of sedatives, tonics, and stimulants.

Treatment of epileptic delirium.—I have not yet had an opportunity of speaking of the treatment of that most interesting form of delirium which I have ventured to designate *epileptic delirium*. It is met with in persons subject to epileptic fits, or it may occur in persons who have never had a fit, but who are disposed to epilepsy, and may have the complete epileptic paroxysm at some future time. Epileptic delirium may come on before or after a fit; it usually comes without any forewarning, and having lasted a certain time, passes off,—why, we cannot tell. Many of the cases which are designated *acute mania* are most probably of this kind. In many such cases epilepsy may be found to have affected some members of the patient's family. So also the cases of the so-called *phrenitis* are examples of epileptic delirium. Now it is obviously of great importance to be able to diagnose a case of epileptic delirium, and to draw a clear distinction between it and the several forms of delirium which I have described.

For the purpose of this diagnosis you must avail yourselves of both negative and positive evidence. By the former you will be able to exclude the various other forms

of delirium which I have enumerated. The history, and the absence of certain obvious phenomena, will denote that it is neither erysipalatos nor typhoid, nor pneumonic nor rheumatic; and the absence of certain other symptoms, as pain in the head, sickness, sluggish pulse, and the non-existence of the tendency to coma, will point out that the symptoms are not due to inflammation of the brain or its membranes. Then you must satisfy yourselves that it is not delirium tremens; in which you will again derive much aid from the history of the patient, and from ascertaining whether he has been intemperate in his habits or not; also from the absence or presence of the peculiar tremor in the voluntary actions, and from the character of the delirium, which in delirium tremens is generally of the busy kind. You will further inquire whether the patient's delirium may not be of the hysterical kind, to which I referred in a former lecture; whether he had not subjected himself to exhausting influences, sexual or otherwise, and so given rise to the delirious state.

Having thus determined that the delirium under which your patient labours is not to be referred to any one of these varieties, it is highly probable that it must be of the epileptic kind; and you must now look for some positive signs to prove that it is so. The aspect of the patient will afford some help: there is, in these cases, a peculiar haggard, wearied aspect of the countenance, with dilated pupils, which should always excite your suspicions as to the epileptic nature of the disease. The character of the delirium is also to be taken into account; it is almost always of the noisy and violent kind; the patient is uproarious, to use a common expression, wakeful and talkative. Then, if previously the patient have suffered from regular epileptic paroxysms; if the delirium have been ushered in with a fit; if epilepsy be distinctly a feature of his family history; if the convulsive fit have occurred in the course of the paroxysm of delirium,—any or all of these points will assist you greatly in determining the epileptic character of the delirium.

What, then, is the appropriate treatment for a case of epileptic delirium? Have we any royal road to cut short the paroxysm, and bring the patient quickly to his senses? I fear that we can no more cut short this maniacal paroxysm, than we can cut short the convulsive fit of epilepsy.

Indeed, the paroxysm of delirium may be looked upon as a prolonged epileptic fit. It is a fit in which the disturbance of cerebral nutrition is mainly limited to the convulsions of the brain. In the ordinary convulsive fit, the parts of the brain which are affected are probably the tubercula quadrigemina and the hemispheres: in the de-

lirious fit, *without convulsions*, the latter parts alone are affected. In some of the milder cases of epileptic disease we see this isolation of the mental affection and of the convulsive very conspicuously. Thus, we observe in some cases that the paroxysm consists only in a momentary loss of consciousness, from which the patient instantaneously recovers; while in others it consists of sudden convulsive starts affecting the upper or lower extremities or both, and which, when the latter are affected, are sometimes so severe as to throw the patient down. Yet in many of these the patient retains his consciousness perfectly undisturbed. You have examples of both of these states now in the hospital; one in Sutherland ward, in the man whose skin was darkened by a course of nitrate of silver, which he took before his admission. This man has the smaller fits of loss of consciousness, momentary faintings, of which he will sometimes have several in the course of the day. The other case is a lad of Jewish parents, who has the convulsive startings to a very great extent, sometimes fifteen or twenty times a day, and frequently with great violence; but he assures us that never, in even the most severe of them, by which he is thrown down with violence, does he lose his consciousness. This lad, however, has also the regular and fully developed fits, but not more frequently than once in three or four weeks.

It is not, then, unreasonable to suppose that if you may have a short and very temporary affection of the intellect and consciousness, you may also have a prolonged affection of them, constituting the epileptic delirium, which may be ushered in by a regular fit, or which may be determined by a regular fit.

Now I apprehend that no one, now-a-days, will pretend that we have as yet discovered any mode of cutting short the ordinary epileptic convulsive paroxysm. Many means for this purpose have been proposed; such as putting salt in the mouth, pressure on the carotids, bleeding, splashing with cold water, but none have been followed with any degree of success which justifies us in adopting them. I do not say that you may not, if you fancy, try the more harmless of them, such as the salt, and the cold water; but anything which interferes with the circulation is dangerous, and must not be tried on light grounds.

I do not believe, then, (to answer the question which I just now proposed), that we have any sovereign means of cutting short the paroxysm of epileptic delirium; and, therefore, I regret to say, that very much of the advice I have to give you on the management of these cases must consist of cautions to you as to what you ought not to do, rather than of positive instructions as to what you must do.

We have, in fact, in the treatment of a case of this kind, to guide our patient through a prolonged epileptic fit; to support his powers until the excitement of the paroxysm passes off, and to guard him against injury. Time is the great element in his cure, and the clinical history of similar cases gives us the best assurance that the paroxysm will in time, if we do not allow the patient's strength to be exhausted, exhaust itself. Our treatment, then, must be mainly *supporting* and *expectant*, with due attention to the ordinary functions of the digestive organs.

And now I must tell you what you ought not to do.

In the first place, as we have the most satisfactory evidence that there is no inflammation of the brain in these cases, you need not harass your patient by the employment of antiphlogistic remedies. You must not bleed him: there is no necessity nor demand for this practice: it often increases the violence and the duration of the ordinary epileptic paroxysm, and it also tends to increase and prolong the delirium, as it does in other forms of delirium. Neither is it advisable to bleed locally, either by leeches or cupping,—or, if you think it prudent to yield to the solicitations of friends, take care to be very sparing in the quantity of blood you take away. When the pulse is strong and slow stimulants are not required; your patient will be better without them: but when the pulse is weak, vacillating, irregular, and more especially when it is quick and running, then stimulants carefully given will prove advantageous. You may shave the head; and, if it be hot, apply cold applications, but in such a way as not to depress the heart's action. This will give the patient and his attendants something to do, and enable you to proceed the more comfortably with your expectant plan.

You may likewise apply small blisters in succession to the scalp, but do so without irritating the patient much, and desist immediately if it seem to have that effect. The moral treatment is particularly to be noticed here, as in many cases it proves of great benefit: all causes of excitement should be carefully avoided. As to restraint, you must exercise the rules I mentioned in speaking of delirium tremens. All means must be taken to prevent the patient from injuring himself; but the gentler the means employed (provided they be effectual) the better. Upon this point I should be glad, if I had time, to read to you some extracts from Dr. Comolly's valuable lectures on the treatment of acute mania, published in the *Lancet*; but I advise you to peruse them for yourselves—to read, mark, learn, and inwardly

digest the wise and humane cautions there given as to the management of these cases.

Sometimes there is extreme wakefulness, and you cannot get the patient to sleep. What are you to do? Avoid opium, generally speaking: if you give a sedative at all, let it be hyoscyamus or hop. Sometimes the cold douche or the shower-bath prove very effectual in inducing sleep. The head may be placed out of bed, and cold water poured upon it from a height, or it may be very freely sluiced with a large sponge. I have seen chloroform of great use where opium had utterly failed: but you must bear in mind the same precautions with regard to its use as to the use of opium; do not give it if you can do without it. Any other drugs which you administer should be of the tonic kind, of which the most useful are the metallic tonics, as zinc and iron, or you may give bark or quinine.

Of the treatment of coma.—The advanced period of the session warns me that I must bring these lectures to a close. But I have yet to speak of the treatment of the different kinds of coma. Fortunately, it will not be necessary for me to occupy your time at much length with this subject. I have in the course of these lectures adduced the strongest evidence that, in the great majority of instances, the state of coma is but a more advanced stage of the same essential condition as the state of delirium. Therefore, much of what I have said of the treatment of the various forms of delirium will apply to that of the corresponding forms of coma.

Now, in speaking of the general treatment of coma, I must exclude the apoplectic coma; by which term I mean coma produced by extravasation of blood, or by pressure of some other kind upon the brain,—as from fracture of the skull with depression of a portion of bone. I must likewise exclude that comatose state which arises from inflammation of the brain.

In dealing, then, with any particular case of coma, you must assure yourselves, in the first instance, that it is not coma from pressure, and that it is not coma from inflammation of the brain. I need not dwell here upon the symptoms of inflammation of the brain, as I have already sufficiently discussed that subject in a former lecture; but I shall make a few remarks as to the diagnosis of the coma which is brought on by effusion of blood within the cranium.

This diagnosis is by no means always easy: with the greatest precautions, you will now and then find yourselves mistaken,—so closely do the symptoms of one

kind of coma often resemble those of another.

The points to which you must look are—

1st. The history of the case.

2d. The mode of accession of the coma.

3d. The state of the pupils.

4th. The existence of a paralytic state.

In most cases of apoplectic coma there have generally been some threatnings beforehand; such as pain or uneasiness in the head, giddiness, *muscæ volitantes*, tinnitus aurium, or other subjective phenomena of the senses. You will be particular to inquire about these points. The coma of apoplectic effusions always comes on more or less suddenly. The sudden supervention of coma in a man previously healthy affords a strong presumption in favour of the apoplectic nature of the coma; and this is especially the case when there have been no convulsions, no mental excitement or emotion, previous to the attack, and when epilepsy does not appear in his history. A dilated state of the pupils generally accompanies the apoplectic coma; but as this is very common, even to a greater extent, in epileptic coma, it gives us no efficient aid in the diagnosis. When, however, one pupil is dilated, and the other natural or contracted, we may have strong suspicions of injury of the brain. This is especially the case if the coma be accompanied or preceded by paralysis of one side of the body.

When the apoplectic effusion takes place in the vicinity of the third pair of nerves, tearing up the brain more or less, a highly contracted state of the pupils is apt to take place.

The sudden occurrence of a hemiplegic paralysis simultaneously with the state of coma affords a strong indication that the cause is an apoplectic effusion. Even with this symptom, however, you will occasionally be deceived. In the Lumleian Lectures I referred to a case of this kind, in which there were sudden hemiplegia and profound coma, and after death I was unable to find a clot in the brain.

In investigating cases of coma you must be most careful to inquire into the state of the renal and hepatic secretions. When either the liver or the kidneys fail, the patient becomes comatose. The liver may fail either from actual non-elimination,—the elements of the bile remaining in the blood,—the liver having lost its power of attracting them out of it,—or there may be some mechanical impediment to the flow of the bile, either in disease of the ducts themselves within the liver, or in some stoppage of the hepatic or common ducts outside the liver. The hepatic derangement shows itself plainly enough in the jaundiced state of the patient. When the kidney is

at fault I need not tell you that you will find the evidence of it in a careful chemical microscopical investigation of the urine.

The former history of the patient affords the most valuable and important guidance in the diagnosis of the various forms of comatose affections. You must inquire into the state of the patient previous to the occurrence of the coma, and must consider whether he has been the subject of epilepsy, gout, rheumatism, hysteria, or renal disease, as the coma may arise from any of these conditions. You should also inquire into the previous habits of your patient, as to intemperance, taking opium, &c., as coma may arise from the presence of opium or of alcohol in the system.

You must, then, before you fix upon your line of treatment, be satisfied that the coma is not apoplectic,—that is, from pressure; and also that it is not due to the presence of opium or of alcohol in the system.

Excluding these, the coma may be traumatic, from shock, producing simple concussion of the brain, or it may be epileptic, or renal epileptic, or hysterical, or rheumatic, or gouty. I shall not dwell upon the diagnosis of these forms of coma from each other, but proceed to refer briefly to the treatment of each.

First, then, as to the *traumatic coma*. This is that state which surgeons describe under the name of concussion of the brain. We have unquestionable evidence that it is not a state of inflammation, or of active disease of any kind, but simply one of suspension of the powers of the brain due to the shock occasioned by the injury. We do not know exactly what the precise condition of brain is in this traumatic coma. It is, however, a state analogous to that of sleep, in which the natural actions of the brain are depressed rather than exalted: to use an expression borrowed from the Stock Exchange, they are *below par*. In the milder cases, as when a patient is simply *stunned*, recovery takes place quickly and perfectly without any medical interference. Why should not this be the case with the more prolonged cases, in which the stunning effects of the injury last considerably longer? Indeed, I do not know why it should not; and I believe that in the great majority of cases this state of coma passes off spontaneously, just as it does in the slighter cases.

Are we, then, in cases of concussion of the brain, to content ourselves with looking on, and to do nothing? I believe that the opinion is rapidly gaining ground that this expectant method,—this system of non-interference,—is the best. Upon this point, however, I speak with diffidence, and must refer you to the great surgical authorities.

I shall only add, that most of those with whom I have conversed on this subject have expressed themselves most favourable to this plan. Among them I may especially refer to my friend and neighbour Mr. Bransby Cooper, whose large experience at Guy's Hospital entitles his opinion to great weight. In conversation upon this subject, he likened the state of coma after concussion to a state of sleep which has a distinctly reparative object and effect.

Most of you will remember the case of a woman who was thrown from a window by her husband, not long ago admitted into one of the surgical wards. This woman had very decided traumatic coma. At first it was thought that some depression of bone had taken place; but it was soon found that the injury was limited to the external table of the skull. This patient recovered completely and most satisfactorily under a treatment which was mainly of the expectant kind.

Then do I advise you absolutely to do nothing in these cases? My advice is, to attend to the functions,—relieve the bowels by mild means, support the system without stimulating the patient, unless great debility calls for more active support; and, that you may not appear to the friends of the patient to be absolutely inactive, shave the head, apply cold to it, or, if there be no contra-indication, apply a small blister now and then.

Formerly all these patients used to be bled, almost as a matter of course, and with the view of anticipating the inflammation which it was expected would follow upon reaction after the shock. But this idea of inflammation following reaction rests upon no good ground: it was suggested by the occasional occurrence of delirium after this form of coma, the delirium being supposed to indicate a state of inflammation. We now, know, however, that delirium is by no means a certain indication of inflammation within the cranium, and more especially delirium arising out of coma. Delirium *passing into* coma would be a more likely indication of an inflammatory affection.

You will ask, are there no circumstances which justify bleeding in cases of traumatic delirium? I cannot take upon myself to answer this question in the negative. I do, however, say that it appears to me a very unmeaning practice to bleed in anticipation of inflammation; and that you should wait for some decided symptom, some good evidence of inflammation or of congestion, before you subject your patient to the risks which arise out of the loss of blood.

Treatment of epileptic coma.—The epileptic coma is the most common form of coma we meet with; and here, like-

wise, the expectant mode of treatment, with moderate purging, answers better than any other. This condition presents many analogies to the traumatic coma. The brain experiences a shock from the epileptic discharge. The shock is generally followed by a longer or shorter sleep, from which the patient awakes up relieved, and often with no other symptom than a feeling of exhaustion. We do not find that anything cuts short the attack. Bleeding depresses the heart's action, and is favourable to the development of the epileptic state, and therefore it cannot tend to cut short the coma.

Epileptic coma, like traumatic coma, may go on for a very considerable time, and yet the patient will perfectly recover. As an instance of this kind I will refer to the case of Eliza Williams, a girl of 13 or 14 years of age, who was admitted into Augusta ward on the 26th of March last, in a state of profound insensibility, which continued till the 30th, a period of four days, and for a week afterwards in a less profound degree.

In this case the treatment was of the expectant kind. We kept this patient's bowels open, and attended to her general nutrition, taking care to avoid any causes which might operate injuriously on her. She recovered perfectly; had several epileptic fits afterwards, with coma of very short duration, and left the hospital much improved in her general condition.

There are, however, cases of coma in which more active treatment than this is required; as, for instance, in cases of coma arising from rheumatic fever, gout, or scarlet fever, or in coma arising from poisoning by urea, in diseased states of the kidney. In such cases it is very necessary to do something more than watch; your treatment must be of the eliminating kind, such as blistering and purging; at the same time, it is necessary to uphold the powers as far as the digestive organs will admit. When you have reason to believe that the blood is poisoned by urea, as in renal disease, you will frequently find the hot air-bath of service; and you must give drastic purgatives, such as elaterium, or the compound powder of jalap, or the Iberis amara. In fine, as a general rule in the treatment of the comatose state which does not arise from pressure, you must bear in mind what I think I have made out in these lectures,—namely, that delirium is the slighter degree, and coma the more aggravated condition of the same state; hence, in a great measure, the treatment of the two conditions must be similar; for coma and delirium differ from each other in degree, the former being only an advanced stage of the latter affection.

Original Communications.

PARTIAL DISLOCATION OF THE
RADIUS AND ULNA
BACKWARDS,

WITH THE FORMATION OF A NEW JOINT.

*Read before the Abernethian Society,
March 14th, 1850,*

BY T. M. GIRDLESTONE, F.R.C.S.

THE preparation here described was taken from a subject in the dissecting-rooms, and is consequently without a history; but, from the appearance of the joint, we may conclude that the displacement occurred some years before death.

The forearm could be flexed to a right angle, but was here stopped abruptly by an unyielding barrier in the joint: extension was but little impaired; rotation somewhat limited: none of the muscles were torn, but those in front of the arm and forearm were wasted.

The following is the condition of the joint:—The anterior ligament had been ruptured, but a part of it remains, and has formed fresh attachments; the lateral ligaments have suffered but little injury; the orbicular ligament has been torn across, and the posterior portion of it passes obliquely upwards over the head of the radius, and terminates in the external lateral ligament, which is thus lengthened; while the anterior portion has become adherent to the neck of the radius, and limits its rotation outwards. The posterior ligament has acquired an attachment to the old articular surface of the olecranon, and is considerably strengthened by some strong transverse fibres which are attached to the condyles, and appear to prevent the coronoid process of the ulna from slipping into the olecranon cavity of the humerus.

The coronoid process of the ulna, rounded at its extremity, and flattened on its articular surface, rests on the margin of the olecranon fossa, at the back of the articulation of the humerus, and is here supported in a new joint, which is formed by some irregular-shaped pieces of bone developed in the posterior ligament, as well as by a con-

siderable prominence of bone growing from the back of the inner condyle. The head of the radius rests on a similar process behind the outer condyle.

The ends of the bones entering into this new joint are smooth and hard, and covered by a substance resembling a thin layer of fibro-cartilage, excepting the head of the radius, which is but little altered, its cartilage remaining on it, and giving rise to some fibrous tissue which is attached to the cup-shaped cavity on its summit. A thin layer of cartilage, with some tough fibrous tissue adherent to its surface, covers the old articulation of the humerus. The radius and ulna are not separated from each other.

When the bones of the forearm are driven backward from their articulation with the humerus, they do not always occupy the same spot; for the coronoid process of the ulna may be drawn upwards and slip into the cavity, at the end of the humerus, destined for the reception of the olecranon, or it may remain between this cavity and the centre of the trochlea over which it has passed: thus the dislocation may be complete or partial. On examining the bones of the elbow-joint we might be led to suppose that a partial dislocation backwards was impossible; and, indeed, it is not recognised by several eminent writers, among whom are Sir Astley Cooper and Boyer. The latter says—"Dislocations of the forearm backwards can never be incomplete." And several modern authors, in describing the numerous luxations to which the elbow is subject, invariably state that, in dislocation backwards, the coronoid process of the ulna occupies the cavity formed for the olecranon. Now this is by no means so constant; and I believe, if we examine carefully the seat of the bones in these dislocations, we shall hardly find two cases precisely alike, although there are many in which the coronoid process does not occupy this fossa.

In the Dublin Quarterly Journal for May 1848, Dr. M. H. Stapleton relates three cases of partial dislocation; one of the ulna alone, and two of both bones backwards, which came under his notice, and were easily reduced.

In determining that these were cases of incomplete luxation, Dr. Stapleton was guided by the projection of the

olecranon backwards, which was less than it would have been had the displacement been complete: but what he chiefly relied on was the relation this process held to the inner condyle,—a feature by which we may determine whether the luxation is complete or incomplete. And he here follows the rules laid down by Malgaigne, who states that, in partial dislocation, “the coronoid process of the ulna lies upon the inferior part, and a little posteriorly to the pulley or trochlea of the humerus; the fore-arm is scarcely at a third of its flexure, the olecranon process is an inch and a half behind the inner condyle, but upon an horizontal plane perceptibly inferior to it; whilst in the complete dislocation, the coronoid process of the ulna being lodged in the cavity usually occupied by the olecranon, the olecranon is found almost equally distant from the inner condyle, but evidently superior to it.”

We may easily satisfy ourselves of the truth of this assertion by placing the coronoid process of the ulna (of the dry bones) first on the back of the trochlea, then in the fossa just above it, at the same time keeping the arm partly flexed. This appears to me to be a diagnostic sign, which, in many cases, may be practically applied. In partial dislocation, when the arm is semi-flexed, the olecranon is in the same *horizontal plane* as it would be if the ulna were in its natural position,—that is, “level with the external condyle of the humerus, and inferior to the internal” (Boyer); but the olecranon, though in the same plane in both cases, is much nearer to the inner condyle in the natural position than in dislocation.

There is a preparation in the museum of the hospital (3. 33.), of an unreduced luxation which very much resembles the one here described. In both instances the coronoid process of the ulna is not in the olecranon cavity, but on its margin, and supported by a growth of bone springing from the back of the humerus; and in both the olecranon is *inferior* to the inner condyle: thus agreeing with Dr. Stapleton's cases, and the rules laid down by Malgaigne. I may add, that the latter author gives it as his opinion “that the incomplete dislocation is by far a more frequent occurrence than the complete.”

We ought not to be surprised to see the coronoid process remaining on the

back of the trochlea of the humerus, when we find some of the lateral ligaments and the brachialis anticus muscle remaining entire; for the former would be put on the strain, and tend to hold the bones in this position, while the latter must be ruptured if the ulna pass further upwards and backwards.

The fore-arm in these injuries may be either supine or prone. In those cases which came under the notice of Sir A. Cooper it was supine, while other observers have found it in a state of pronation.

The heaping up of new bone in the neighbourhood of an unreduced dislocation is stated by Cruveilhier to be common in the hip, but rare in the elbow-joint. It has taken place, however, in this case, as in the one in the museum: and there is one preparation in the Museum of the College of Surgeons (Patholog. Cat. vol. ii. p. 208), where “the radius and ulna are dislocated outwards, and *partially backwards*: the ulna is fixed to the humerus by bone.” Cruveilhier, also, in his *Plates of Anatomy and Pathology* (Liv. ix. Ch. iv. p. 8), gives the representation of an unreduced complete dislocation of the radius and ulna backwards, with a shell of new bone adhering to the radius and ulna near the joint, and projecting for some distance up the arm in *front* of the humerus, where it must have assisted in fixing the dislocated bones in their abnormal position.

Since writing the above I have seen a little girl in whom the same accident, attended by precisely the same symptoms, has been observed. The patient is now in St. Bartholomew's Hospital, under Mr. Stanley. The accident happened four months previous to her coming to the hospital, and, not being reduced, the bones became fixed in their new position.

CONGELATION OF PROTOXIDE OF NITROGEN AND ALCOHOL. BY M. DESPRETZ.

A PORTION of protoxide of nitrogen in the fluid state being poured into a platina capsule placed on a brick, under the receiver of an air-pump, became, by the first few strokes of the piston, covered with a white stratum, and was quickly converted into a snow-like mass of white substance. In a similar manner alcohol, mixed with protoxide of nitrogen, solid carbonic acid, and ether, was solidified, although imperfectly. —*L'Union Médicale.* x

CASE OF
UNUNITED FRACTURE OF THE
TIBIA,

OF TWENTY-FOUR YEARS' STANDING,
SUCCESSFULLY TREATED.

By R. W. TAMPLIN, Esq.
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Miss —, æt. 25, at the age of fourteen months slipped between the bars of a garden-seat. The only circumstance which attracted attention was a broad discoloration at the lower third of the leg, and a slight curvature of the bone. After a few weeks the child was noticed to walk less strongly, when a surgeon was called in, who consoled the parents with the assertion "That there was no necessity for uneasiness, as many children had a difference in the straightness of their legs at that age." And upon the mother's remarking that such had not been the case until the bruise was noticed, replied, "That all would be well in a few months." Friction and bathing were resorted to for some time, and the child is stated to have walked without limping until she was three years old. Her manner of walking then became irregular, and the bone was noticed to project, which was supposed to arise from her increased weight. Another surgeon was now consulted, who discovered the fracture, and pronounced the bone to have been "falsely united." An endeavour was made to rupture the false union, but without success. Frictions and ointments were again used, with a view of obtaining absorption of the extraneous matter, and the part supported by plaster. The case appeared to be progressing satisfactorily, when the child met with another accident, and the smaller bone of the leg is stated to have been broken. Splints were then applied. From nine years of age the leg was supported by an instrument, and crutches were occasionally used. The spine is stated to have been also curved at this time. Her general health became impaired, and constant suffering was occasioned by any attempt at walking.

The patient was now placed under a physician, since deceased, who especially treated spinal cases. The uniting medium is stated to have been broken

by that gentleman, with the view of reducing the fracture, and forcible extension had recourse to, which brought the leg down for the time being, but it always returned to the malposition. This treatment was continued for two years, and then the case pronounced "incurable." The patient was at this period eleven years old. The leg was placed in splints, which were continued until she was fifteen; but she was unable to put the foot to the ground. Many surgeons of eminence were consulted, and amputation was the general advice. An instrument was then obtained to hold the knee and ankle stiff, with a high-heeled boot, and the leg supported by a case of sole leather from the knee to the ankle: with this she managed to get about, but not without great pain; the weakness increasing in spite of the artificial support.

On the 17th of July, 1849, I first saw the patient, and, upon examination, found that the leg had been fractured at the lower third obliquely upwards, the upper portion of the tibia projecting pointedly forwards, and riding over the lower third. The leg was two inches and a half shorter than the other. It was freely moveable. The knee, from the constant irregular strain upon the ligaments, yielded outwardly, and the patient could not put the leg to the ground, even with the assistance of the support, without suffering severe pain both at the point of fracture and in the knee-joint. Her general health had suffered more or less, and her existence is stated to have been a burthen to her.

From the history of the case,—the fact that it had existed from childhood, that all attempts had failed, that amputation was the general advice given,—I gave a doubtful opinion, but determined to make an attempt to obtain a union without operation. Three indications occurred to me: first, to bring the bones in apposition; the second, to obtain absorption of the false uniting medium; the third, to endeavour to obtain union, either by the deposition of bony matter, which I thought might possibly be thrown out from the irritation which must necessarily be set up, or by a contraction of the portion of the false union which immediately surrounded the fracture. With these views I ordered an instrument to be so made that the thigh could be firmly grasped above the condyles of the femur, the

foot below having a screw by means of which the distance between the knee and foot could be gradually increased. This instrument was applied on the 31st of July, 1849, and the leg kept horizontal, in the extended position. Gradual extension was now commenced, and continued for four days. So much pain was occasioned in the gastrocnemius, that I found it necessary to divide the tendo-achillis, which was done in the usual manner by puncture from within outwards. The instrument was omitted for eight days, and then re-applied, and extension again used. From this time the leg became gradually and easily elongated; and during the extension a steady continued pressure was kept up on the tibia above the point of fracture, and counter-pressure at the back of the leg, just above the ankle-joint. This treatment was persevered in unremittingly until the 9th of January, 1850, without interruption to the health,—there was, however, at times severe pain, which was allayed by opiates,—when, upon examination, the leg was found to be of equal length with its fellow, and the bone retained its position unassisted. I then applied the common splints, and ordered an instrument, with a boot attached, to support the weakened knee-joint, which had resumed its proper relative position during the treatment, and also having a pad to support the tibia, in case the union was imperfect.

On the 26th of February, the patient could raise the leg in the horizontal position without the slightest pain, and without a sign of motion at the point of fracture. I then applied the new support, which was worn, without any attempt to put the foot to the ground, until the 8th of March. She was able to stand on the 3d April, and then, at the request of Mr. Travers, and afterwards in the presence of Mr. Lawrence, both of whom kindly visited the patient, she walked without pain, and without a sign of motion at the point of fracture. Since that time she has continued to use the leg freely, with slight intermissions from indisposition. Can walk up and down stairs. Her general health has improved, and I have every reason to believe that a perfect cure has been effected.

22, Old Burlington Street,
July 1850.

A CASE OF PROBABLY

INTRA-UTERINE ACUTE INFLAMMATION OF SEROUS MEMBRANES.

By W. B. KESTEVEN, ESQ. M.R.C.S.

Meningitis—pleuritis—peri- and endocarditis—pneumonia.—Death of the infant.—Spasmodic disease—and hemiplegia in the mother during pregnancy.

THE following case presents several points of practical and pathological interest. These I have endeavoured to point out in the observations which follow its recital, as they furnish the reasons for placing it on record in the pages of the MEDICAL GAZETTE.

A male child, born Feb. 17th, 1850; the labour, which was a first labour, was natural in every respect, and had occupied about fourteen hours. When born, the child, which was large and well-formed, did not breathe without the employment of the usual means for rousing apparently still-born children. Its cry, when these measures had established the act of respiration, was not as loud and strong as is common to children of an equal size, and as fully and well developed. The breathing was quick, and appeared as if performed by the shoulders rather than by the ordinary thoracic movements. There were very visible depressions above the clavicles during each inspiratory act. The cry was a sort of paroxysmal effort—he seemed to cry as if in bursts of passion. In the intervals he laid very quiet, with the exception of an occasional short dry cough. He took the breast, but always with some trouble. His intestinal and urinary excretions passed as usual.

On the fifth day he refused the breast: he had seemed to flag a little on the preceding day. The breathing now became still more frequent; the pulse was so rapid and feeble that it could scarcely be felt, and could not be counted; the action of the heart was so tumultuous, and its impulse so considerable, that the sounds were rendered indistinct. The respiratory murmur was loud all over the chest.

On the sixth day the symptoms continued the same. The nurse, an experienced, sensible woman, spoke of the

child's having been slightly convulsed; that it had stretched out its limbs now and then, and rolled its eyes about in an unusual manner. Dr. Robert Lee having been called in to see the child's mother on this day, saw the infant also, considered that he was only suffering from want of nourishment, and recommended a wet nurse. This advice was followed, but the child would only suck twice. He had been fed pretty much with milk and water, as the mother had not a very full supply of milk, and the infant had not taken the breast readily. No vomiting occurred.

In the course of the seventh night a severe and long convulsive fit occurred, accompanied with a peculiar and distressing cry. Convulsions now occurred several times a day for the next two days.

On the tenth day the child died in a fit, which had lasted several hours.

Assisted by my friend and neighbour, Mr. Thomas Graham, the post-mortem examination was made sixteen hours after death. The entire surface of the brain presented effusion of serum and yellow lymph in the meshes of the pia mater beneath the arachnoid membrane. The lateral ventricles contained each about a drachm of serum. The substance of the brain presented nothing morbid, except that it was perhaps a little firmer than usually met with in infants.

The surface of the medulla oblongata was thickly coated with fibrin, which was more abundant over the inferior than on the superior surface of the brain.

On opening the thorax, we found the pleural surfaces throughout adherent by recent soft yellow lymph; their cavities were completely obliterated. The two surfaces of the pericardium were also universally adherent by the same kind of lymph. The endocardium had not escaped; for, on closely examining the aortic valves, minute, transparent, colourless, and quite soft granulations were discovered on their edges.

On cutting into the substance of the lungs, the lower lobes were in a state of semi-hepatization; their structure dense and friable, but not without a slight degree of crepitation. In some parts they had a tough feeling, and when thrown into water remained suspended just beneath the surface, and in physical characters resembled what is called atelectasis.

The bronchial mucous membrane was of a deep red colour.

REMARKS.—The following points of interest are observable in this case:—

1. The probability that these diseases had their commencement during intra-uterine life.

2. The great extent to which acute inflammatory disease was developed in so young a child.

3. The degree in which the symptoms indicated such pathological conditions.

In tracing this extensive inflammatory disease to the period of intra-uterine life, we have to consider the history of a pregnancy and labour also possessing pathological interest. The following are the facts of the case:—

The mother of this infant is an apparently healthy young woman, about 23 years of age, of rather a full habit, but who had always enjoyed good health, with the exception of dysmenorrhœa at the catamenial periods. She was married in May 1849, and soon afterwards became pregnant. She suffered much more than usual from sickness, and an irritating cough, even up to within a few weeks of labour.

In the month of October, without any discoverable cause, she was attacked with severe cramps of the uterus, conjoined with spasm of the diaphragm. The pain of these cramps and spasms was most excruciating. The attack lasted fifteen hours without any amendment, notwithstanding the most active treatment. So agonising was the pain, and so distressing was the interruption to the respiratory movements, that I verily thought she would have died in some of the paroxysms. In a few days, however, she was restored to her former health.

On the 2d of January she suddenly lost her consciousness for a few moments: at the same time the sensation and motion of the right side and extremities disappeared for several hours. Numbness and tingling of the limbs, however, continued for several days. The pulse, it must be observed, was full, but not hard. The heart's impulse and sounds were perfectly natural. Active antiphlogistic treatment was adopted, and very low diet rigidly adhered to up to the end of gestation.

On the afternoon of the 17th February labour was suddenly ushered in by severe rigors, intense headache, and

pains in the limbs and back. The pulse was 140, and full. I attempted to bleed her from the arm, but could not by any means obtain sight of a vein; and, as the labour-pains began to manifest themselves most decidedly and quickly, and as she had been previously reduced by the means already stated, I did not prosecute the depletion.

The labour was in every respect natural, and terminated in about fourteen hours.

At midnight of the same date I was summoned to her in great haste, and found her in a paroxysm of puerperal mania: the countenance flushed; the surface of the body bathed in a most profuse perspiration: the pulse too rapid to be counted, it must have been nearly 200, and feeble: there was some little tenderness over the uterine region; the bowels had been opened; urine had passed freely; the lochia were abundant. A full dose of muriate of morphia and of tincture of digitalis completely dispersed this formidable group of symptoms: she fell into a quiet sleep, and awoke after several hours as well as if nothing particular had taken place. The next day was passed without the occurrence of anything remarkable; but about 8 o'clock in the evening she was seized with an attack of spasms resembling those from which she had suffered in the previous October,—with the difference, however, that this last attack yielded to remedies in four hours. Spasmodic pains in the right side of the chest continued for several days, and gradually merged into subacute pleurisy, producing effusion into the pleural cavity. Absorption of the effusion readily took place under the use of mild mercurials and counter-irritation, and the patient completely recovered.

The consideration of the history of this pregnancy and labour shows the existence of a disposition to excessive vascular action, and a morbid condition of the blood itself; and affords every reason to conclude that the maternal diathesis was transmitted to the foetus in utero in such intensity, that acute inflammation of the serous membranes was excited before birth.

This morbid condition of the blood in the pregnant female is a fact in physiology; but it is a source of some regret that in this particular case an analysis of the blood was not made. The occurrence of such an amount of disease,

however, in the infant, could never have been anticipated, and therefore the urgency of such an analysis was not apparent. It must be added that this lady had suffered considerable mental anxiety during her pregnancy.

2. The extent of disease here recorded is, so far as the author is aware, unparalleled, regarded as intra-uterine, and of extraordinary extra-uterine rarity in an infant of ten days old. The writer has searched through a great many of the first authorities, and can find no record of such an extent of such disease in so young an infant. The only case at all parallel, which he has met with, is one of an infant four days old which died of pneumonia, and is recorded by Dr. Casper, in his "Wochenschrift," Nov. 1849, in an article on the causes of death in new-born infants. Dr. Casper there observes that the appearances of pneumonia, as shown in new-born infants, so closely resemble those of the condition termed atelectasis by some pathologists, that he is disposed to think the latter has been mistaken for the former. The case now detailed presented the same feature, and favoured the same conclusion.

3. With regard to the disproportion between the amount of actual disease and the symptoms which were manifested, it may be observed that it has been pointed out by Billard, that in new-born infants the absence of febrile reaction under the most severe and acute inflammatory disease is a law which contrasts strikingly with the opposite condition in infants after the period of dentition.* Certainly such law alone can explain the existence of such extensive disease for the period of this infant's life with so few symptoms; since the state of the respiratory functions, of the pulse, &c., was not more abnormal than is often observed in feeble children.

Holloway, June 1850.

RUPTURE OF THE DUCT OF STENO.

A BOY was exhibited in whom this accident had taken place from a blow. No external fistula was formed, but a tumor, half as large as an egg, had accumulated from the effusion of saliva under the cheek. This was punctured by Dr. Pepper with a trochar. The tumor returned in a few days; but the patient was lost sight of.—*Dr. Hartshorne, in American Journal of Med. Sciences*, Jan. 1850.

* *Maladies des Enfants*, Preface, p. 2.

POSTSCRIPT TO SOME
OBSERVATIONS ON THE
CONDITION OF THE BODY
AFTER DEATH BY
CHOLERA,

BY MR. FREDERICK BARLOW, M.R.C.S.

In quoting Dr. Dowler on the subject of post-mortem temperature (see *MEDICAL GAZETTE*, July 5, 1850, p. 20), I have made an error which I wish to correct. I have stated that the temperature of the internal parts does not appear to have been observed; but I find, on reading the "Researches," the two following cases:—

"W. O., an Englishman, aged 27; dead five hours; neck moderately stiff; brain 93°; epigastrium 100°; chest 93°; thigh 99°; the arms strongly contractile, &c."

"J. H., an Irishman, aged 36; of gigantic frame, estimated at 200 lbs. In fifteen minutes after death the neck, jaws, and recti muscles were immovably fixed, the contractility of the arm being very powerful. In an hour and ten minutes the rigidity became universal, the thigh being at 107°. In one and a half hours the epigastrium was at 106½°, and the brain 101°."

The following is the account of the case whereto I have referred briefly, in which a rise of temperature appears to have taken place after death:—

"J. K., a Philadelphian, aged 25; in fifteen minutes after death, presented the contractile phenomena in their most intense form, but which declined wholly in one hour, the body being everywhere flexible. In half an hour after, rigidity set in. This body, which before death had been remarkably cold, had a temperature after death as high as 109°, and which did not refrigerate below 104° in three hours after."

Dr. Dowler makes the following observations in reference to post-mortem heat:—"The continuance of, or rather the degree in which post-mortem heat is evolved, bears no proportion, I repeat, to the intensity of post-mortem contrac-

tion. The great heat developed in the dead body I have endeavoured to illustrate in the medical journals of our country, and will not, therefore, dwell upon that subject at present. I find, however, on examination of the original papers not yet published in detail, that for the most part, when the heat had declined, the contractility was exhausted; but that the presence of great heat, ranging as high as 113°, did not by any means imply the presence of contractility nor the absence of rigidity. Authors seem not to have been aware of the augmentation of animal heat after death. Some have, it is true, noticed an increase of heat after death from cholera, compared with the extreme coldness of the surface during the last hours of life; but has any one hinted that this post-mortem heat ever rose as high as even the healthy standard,—to say nothing of 14° or 15° beyond that?"

It is quite obvious how extremely cautious we should be in using the words *rise* of temperature, as applied to the dead. The heat tested, *at the time of death*, by the thermometer in certain parts, should be compared with that manifested by the like means in the same parts at given periods afterwards. The colour of the surface, and whether it undergoes change or not, should be observed; the state of the body as to perspiration, or the absence of it, should be noticed particularly. Different parts of the surface should be compared as to heat; and the temperature of the skin and internal parts should be contrasted. The mucous membrane of the lower bowel might be examined without any incision being made.

The greater part of that which I have written on the subject, I have, in the desire to cause inquiry, written not positively, but *suggestively*. It is time that our knowledge of the alleged rise of heat in the dead should be more exact. The high temperature noticed by Dr. Dowler some time after death is of great interest; and, though it were shown to be the remains of heat generated in life-time, it would be so still. The highest temperature which I have observed in the dead is 104°. The observation was made on the axilla of a man who had died of apoplexy. There were well-marked muscular contractions seen also: I hope to detail them in a future contribution.

I find the parts tested by the thermometer by Dr. Dowler have been more

* See *Experimental Researches on the Post-mortem Contractility of the Muscles, &c.* By Bennet Dowler, M.D.

† I have concluded from the context that such phenomena are alluded to as may be excited by percussion.

frequently specified than I supposed. And I must conclude by the remark, that, though I differ from this writer in many of his conclusions, and dissent from the criticism in which he has indulged, the phenomena he has narrated are extremely well worthy the close attention of physiologists. Mr. Helps has already repeated Dr. Dowler's remark on the ease with which post-mortem contraction may be excited by *percussion*; and a careful inquiry into the condition of the dead as to the state of their temperature, might be found to confirm the accuracy of the observations, if not the correctness of the inferences, of this inquirer.

PROGNOSIS IN ANGINA PECTORIS.

THOSE authors who consider angina pectoris as depending on the organic lesions of the heart and its vessels so often found in it, of course regard it as a disease necessarily fatal. As we consider it a nervous disease, depending on a morbid condition of the par vagum, we should make a different prognosis, particularly if the affection be not of long standing, and the patient be young and healthy. We think it an error to suppose it peculiar to advanced age; it may occur, like other neuralgia and rheumatism, at all ages, and in such persons may be cured by similar remedies. There are, even among old people, several undoubted cases of cure recorded in the Dictionaries of Medicine. Allowing for inaccuracies of diagnosis, we may find Laennec not very far from the truth when he says—"Angina pectoris, in a light or moderate degree, is an affection extremely common, and exists very often in persons who have neither an organic disease of the heart nor of the great vessels. I have seen many who have had very violent attacks, but of short duration, who have recovered." (*Auscult.*, tom ii. p. 747.) If the disease be neuralgic, recovery may be complete without any organic change. If it be rheumatic, recovery might leave behind it a predisposition to cardiac disease, so often found in connection with it. Even supposing the disease to arise from a complete disorganization of one pneumogastric, life may be considerably prolonged, and good health enjoyed—the nervous communication being kept up by the anastomosis between the superior and inferior laryngeal, which has been found one-third the size of the cervical par vagum.—*Dr. Kneeland*; in *American Journal of Medical Sciences*, Jan. 1850.

FACTS AND ARGUMENTS IN SUPPORT OF THE CONVULSIVE THEORY OF MENTAL DISORDERS.

BY WILLIAM SMITH,

Member of the Association of Medical Officers of Hospitals for the Insane; formerly Resident Surgeon in the Lincoln Lunatic Asylum, and subsequently in the General Hospital at Lincoln.

THE more deeply I reflect upon the interesting phenomena of mental derangement, and the more attentively I peruse the recorded opinions of eminent authorities on the Diseases of the Brain and Nervous System, the more strongly am I convinced of the truth and correctness of my former proposition, that the whole class of mental disorders ought to be viewed as true convulsive or spasmodic affections, and treated as such; and, moreover, the present humane and enlightened mode of practice, now adopted and pursued in our best regulated lunatic establishments,—which places little faith upon mere medical applications, and relies almost exclusively on moral, regimenal, and dietetic remedies,—strongly corroborates this view of the subject. Nay, I will even venture one step further, and assert my belief, the result of considerable personal experience of such affections, that great benefit would accrue to society, if practitioners, in the treatment of what are usually termed nervous disorders, would take a hint from the experienced superintendents of our public lunatic institutions, and eschewing the present routine wholesale and indiscriminate administration of drugs and pharmaceutical appliances, allow their patients the benefit of fresh air, exercise, plain, nutritious, unstimulating diet, and mental as well as physical occupation. Notwithstanding the eager thirst for knowledge, so clearly characterising the present age, the public generally, and, it is to be feared, the medical profession themselves (in too many instances), are grossly ignorant of the organic laws which an all-wise and benevolent Creator has imposed upon living matter. Were that excellent and lucid work, "Principles of Physiology, applied to the Preservation of Health," by Dr. Andrew Combe, more attentively perused by the public and the profession, a large amount of dis-

ease, and consequent misery, might be prevented. Dr. Wm. Bush, in an excellent essay, contained in the 7th number of the *Psychological Journal*, remarks:—"In proportion as the improved sciences of physiology and pathology add to our knowledge of the essential elements of health and disease, they cannot fail to excite alarm in every reflective mind at the accidents so likely to accrue to the delicate and mutually dependent tissues of the body, and either to derange their functions in health, or to destroy their structure in disease. If this be found true in the general anatomy of the body, how much more applicable must it be where the minute tissues and intricate fibres of the brain are involved? an organ so sensitive to morbid influences, that its functions will be impaired, or altogether destroyed, by causes so subtle as not to be detected on post-mortem investigation. But the alarm which a review of these facts would naturally engender, is swallowed up in surprise on observing, that the functions of so delicate an organ are not more frequently and more seriously deranged by the turmoil of life, and the incessant demands which commercial and literary competitions make upon our energies. It is indeed true, that the consequences of these excessive mental demands are not always visited upon us in our own persons, but are reserved for more condign punishment in their infliction on our children, who become living and lasting judgments before our eyes, at the same time that they convey lessons of amendment to the present, and of caution to the future generation.

"If we more duly reflected how often with a name or fortune we transmit to our children the terrible nervous affections (and sometimes even the fatal seeds of family extinction) which they inherit from the exhaustion of our own energy in the gaining of these vanities, we might be induced to attribute more real happiness to health, virtue, and longevity, than to the pursuit of worldly gain, or to a conformity with the existing errors of our whole social system."

Do not these remarks apply to mental disorders with quite as much force as to the neuroses, or those of a convulsive or spasmodic character? The nervous system, in all, is at fault, and what matters it whether the cerebral, the spinal, or the ganglionic division be the

one mainly implicated? Do we not find, in actual practice, amongst these disorders, that the same grand fundamental principles of therapeutics direct us in the choice of our remedies in all of them? There is in all nervous diseases a want of tone or natural vigour in the system, a disturbance of the natural equilibrium between the nervous and vascular systems; a general intolerance of blood-letting, or powerful depletive or evacuating measures of any kind: strong purgatives even are invariably prejudicial, and though the momentum of the blood may appear to be, and probably is, greater than natural, and the pulse increased in frequency, as in the *clavus hystericus*, or during a maniacal paroxysm, still these alone, or even combined, are no proofs of actual power, any more than the raving delirium of fever, or the temporary excitement or exaltation of intoxication, denote increased tone of the natural powers.

The idea occurred to me the other day to consult Johnson's Dictionary as to the precise signification of the word convulsion. That erudite lexicographer thus defines it:—"A convulsion is an involuntary contraction of the fibres and muscles; an irregular and violent motion, commotion." Now, does not this description singularly apply to the maniacal paroxysm? The patient, however he may desire it, for we have ample evidence to prove that many strongly resist the insane inclination, and even warn their attendants of the impending storm, cannot control the overpowering and impetuous impulse of thought or action; volition or the power of the will is wholly in abeyance for the time: the reasoning faculty, which at other times reigns supreme, and presides over all the feelings, thoughts, sensations, actions, &c., restraining the lower, grosser, and more animal propensities of our nature, is, during the paroxysm, dethroned from its high estate, and "all the jarring passions are at strife." Thoughts, ideas, emotions, the various nerves of special sense (it may be one, several, or all of them), the secretions—under the immediate control of the ganglionic system, but yet subject (as governors of petty provinces under the absolute authority of a reigning monarch) to the one great controlling power, the sensorium—take on irregular or abnormal actions.

Numerous arguments might be adduced to prove the intimate connection and natural affinity existing between mental disorders and the whole class of convulsive and spasmodic diseases. One probably more striking than the rest I will mention here—viz., "the tendency of these morbid actions to distinct intermissions of longer or shorter duration, and more or less perfect in kind." Dr. Henry Holland, in an interesting chapter, "on morbid actions of intermittent kind," has some remarks which I consider peculiarly applicable to the question now under consideration. At page 329 he observes—"This tendency to intermission in the animal functions may justly be termed a law, inasmuch as it is natural, general, and manifestly designed. All the phenomena of mind, and those of body which have direct relation to mind, are more or less submitted to it. The alternation of sleep and waking, a phenomenon in which so many separate functions have part, and by which all are regulated and controlled, is the instance at once most familiar. Each organ of sense is more or less connected with this one important condition of existence, while they have, besides, various shorter and more irregular intermissions, depending on changes in the action upon them from without, and the proportion of this action to the excitability they possess." If we admit the truth and accuracy of these propositions, as applied to some forms of nervous and spasmodic disorders, is it philosophical, or in accordance with common sense, to deny their application to mental derangement, which is supposed to be an affection simply of another portion of the nervous centres? One cause of the endless disputes amongst physiologists, and ever-varying and discordant theories advanced by different authors, relative to the laws and action of the nervous system, is mainly to be attributed to the infinite divisions, subdivisions, and arbitrary distinctions which have been made with regard to that portion of the human frame. Had the illustrious Harvey puzzled his brains with such an infinitesimal arrangement of the blood-vessels and vascular system, that great boon to science and mankind, the discovery of the circulation of the blood, might still have remained buried in oblivion. Mr. Travers, the experienced surgeon to St. Thomas's Hospital, who

may be justly looked upon as the Nestor of modern surgery, has remarked, in his *Further Inquiry concerning Constitutional Irritation*—"In the rapid progress of all subjects of scientific inquiry during the last twenty years, that of physiology seems to have fully participated. The renewed intercourse between the nations of Europe stimulating, by the quick circulation of knowledge, a spirit of national emulation, has rendered the present age prurient of discovery; and it is not without some apprehension for their stability and value, that I observe the credulous reception and ready adoption, by the physiologists of this country, of the results of experimental inquiry on the continent. It is enough that a name of some celebrity stands connected with them, to obtain currency for many statements startling by their novelty, and reasonings irreconcilable with our previous observations and opinions.

"Some physicians, confessedly men of great learning and talent, little accustomed to the details of healthy anatomy, and unacquainted with the visible changes of living structure forced upon the surgeon's attention, have innocently contributed to the encouragement of crude theories and fanciful distinctions; add to this, a tendency to hypothesis is begotten by the obscurity in which the medical department of pathology is necessarily involved. The microscope, the means of discovery most commonly resorted to, is a most prolific source of delusion, judging from the strange and discordant appearances which it presents to the eyes of its different employers. I read that the globules of the blood are gaseous vesicles, whether of carbonic acid or atmospheric air, and imbued with the property of moulding their own vessels in a layer of coagulum; of the exudation of lymph of pus; of the separation of the colouring matter from the globule of blood actually taking place in the vessel, and thus constituting one of pus; the organization of a clot of blood; the formation of an entire organism—blood-vessels, absorbents, and nerves—in a floating layer of fibrine, wholly detached from all surrounding parts; the branching forth of new canals through the cellular texture, from the continuous sides of inflamed vessels; the transportation of pus by the veins from remote lesions, abscess or ulcer, to parts

free from any other sign of diseased action, as the parenchyma of the lungs, or the parietes of the heart, &c.;—these things, and a thousand others not less extraordinary, may be; yet if knowledge has no bounds for our senses, its advancement must keep some measure with our understandings, or it ceases to be useful. It is impossible to contemplate the present travelling rate of physiology, and the mass of unsatisfactory experiment and undigested speculation continually throwing up, without a feeling of sympathy with the hopeless perplexity of the student, in such an '*embarras de richesses*,' and, what is vastly more serious, some alarm at the possible consequences that may result to the usefulness and credit of the profession of medicine, and the health of the community."

In proof of the correctness of my views relative to the real nature or essence of mental derangement, I shall take leave to quote some extracts from the lately published Annual Report of the Lincoln Lunatic Asylum, for the year 1849. Everything which has emanated from that establishment, in the shape of Annual Reports, &c. bears the impress of extensive observation and acute discrimination, and has a decidedly practical bearing upon the welfare and happiness of the insane. The Governors, under the head of violence, remark:—"This difficulty is the point of view in which insanity has popularly been considered, and very little has been done by the profession to show the extreme exaggerations upon the subject. Violence is not the common characteristic of insanity; it is the exception; it seldom appears, except at intervals, and is very rarely of a sustained character. In common life there are great varieties of temper, and some are habitually violent; and a vulgar extension of the idea has encouraged a notion, that violence in the insane is to be subdued by terror, or chains, or in any way except by management. Two principles, however, ought to have suggested themselves;—that confinement will increase the irritability and violence of a man, as it would of a dog;—and also that confinement facilitates the ill-usage of patients, by making it perfectly safe to the attendant, and so additionally tends to increase that irritability. . . . "It is very obvious that exercise in the open air, a variety

of engagements, and considerable freedom of range, must tend to divert the mind from a tendency to violence, and diminish the irritability of the system. . . . Nine-tenths of the erroneous treatment of insanity has arisen from confounding insanity with phrenitis, or inflammation of the brain, a disease requiring remedies usually wholly contra-indicated in the treatment of the insane; and the same remark may be applied to cases of delirium tremens; the former introducing, among other proposed remedies, blood-letting, and shaving the head; and the latter the use of opiates. . . . "It is remarkable to observe how soon after admission the more exaggerated aspects of the disease seem to relax in this house. Every facility is offered for innocent outlets to the excitability of the patient, through the free use of his bodily organs. Exercise and other occupation in the grounds, a free range from apartment to apartment, games without doors in fine weather, dancing within doors in wet weather, are substitutes for violence with many; nor is any interference attempted with grotesque attitudes or motions, or grimaces, or declamation, in which the patient may indulge for the purpose. The irritability is not concentrated or stimulated by compression. The danger from violent patients is found to be greatly increased, not merely by the increased tendency to violence, but by the diminution of caution and watchfulness, which a reliance upon instruments afterwards produces. That the tendency to excitement is increased by instrumental restraint would appear from the fact that cases of death from maniacal exhaustion have disappeared from the books, contemporaneously with the disuse of the instruments. . . . "Experience will suggest to attendants resources for meeting the difficulties, where they cannot throw themselves upon the coarse and ready expedient of an instrument. In this house lately an epileptic female patient was subject to such an extraordinary tendency to violence in connection with the epileptic attacks, that an additional nurse was required on her sole account. The head nurse, however, in answer to an inquiry, observed, that they had comparatively little trouble with this patient now, as they merely sent her out of doors on these occasions, when

the weather allowed, and that after walking about sometimes for several hours, she would return, of her own accord, composed.

"Violence in an insane patient may sometimes be a mere blind animal impulse, quite involuntary on the part of the patient; and it is obvious that the proper course is to prevent the patient from doing mischief, until the fit is over, never, in such a case of long continuance. The mischief is prevented, as the mischief of an epileptic or hysterical fit would be, under which no rational person would think of applying an instrument. In the majority of cases the violence is semi-voluntary, and it is remedial, to rouse the patient's self-control. The condition is brought on and aggravated by every species of harsh usage, contempt, or injustice; and this feeling must be increased by the use of instruments, under which the motive to self-control ceases, and the patient indulges himself in letting his mind loose to its impulses of violence, leaning as it were upon the instrument; while the contempt and ill-usage which a cowardly bystander may then safely exercise aggravate the case. The irritated rebound of an insane mind cannot be crushed; it must be evaded and diverted."

Do not the remarks contained in the last paragraph most distinctly and substantially corroborate the views which I have lately advanced on the convulsive character of mental disorders? and, moreover, by their allusion to the hysterical and epileptic paroxysm, is it not evident that the Governors of the Lincoln Asylum view insanity as a kindred affection? There is, in truth, a strong family likeness pervading the whole class of nervous disorders; the difference is one of degree rather than of kind. If we dealt with the nervous system and its morbid affections more as a whole or entity, and not as composed of separate and disconnected portions, each having an action independent of, and beyond the control of the rest, much of the confusion and uncertainty that now hangs over the treatment of these disorders—suggesting an empirical and hit or miss selection of pharmaceutical remedies, or a blind routine plan of treatment, and rendering it a rich field for the unprincipled charlatan or the self-deluding homœopathist, would at once be obviated.

Whatever professed writers of treat

tises on insanity may think proper to advance as to the specific or exclusive location of mental disorders in the vesicular or cineritious portion of the brain, I shall take leave to view them as affections wherein all the nervous centres, cerebral, spinal, and ganglionic, are more or less implicated; and wherein the blood—a component and indispensable element in every vital function or action pervading the human frame—must also be taken into the account. Everything we know of the phenomena of insanity,—the derangement or disturbance of the special senses, one or more of them,—the depraved or perverted state of the secretions, sometimes in excess, sometimes defective;—the morbid sensibility or entire loss of sensation;—the irregular and irrepressible movements of the muscular system;—the noisy vociferations, or the sullen torpor, and refusal to answer the simplest question;—the extreme watchfulness (pervigilium), or the drowsy lethargic state of semi-consciousness;—the brilliant coruscations of wit and fancy, or the non-consecutiveness of thought or expression;—the ravenous appetite and intense thirst, or entire refusal of food and drink;—the irretention of urine; the involuntary and liquid dejections, or the obstinate costiveness and hardened scybala, frequently requiring copious enemata for their solution and evacuation;—all clearly demonstrate to the observant mind, that not only every portion of the nervous system—cerebral, spinal, and ganglionic, but likewise the vascular and assimilative systems, are alike more or less implicated in the one disorder usually designated mental derangement or unsound mind. Mr. Travers, whilst discussing the intricate phenomena of morbid action (page 253 of his *Further Inquiry*,) remarks:—

"The class of motor nerves, which in a state of health are subject to the will, are variously affected by disease; deprived of their tonic power, the muscles become tremulous and incapable of executing their ordinary actions steadily, both of resistance and of motion, passive and active. This, which amounts to a partial paralysis, may arise from causes operating through the brain or from the brain—i. e., from the defect or disorder of the stimulus conveyed by the sensitive nerves, or from the disorder or dis-

ease of the nerves of voluntary motion, singly or in association, or of the part of the brain in which they have their origin. They may cease to be influenced by volition, and the muscles be in consequence motionless from the defect of the stimulus, or an obstruction to its course; or other causes acting as a more powerful motive stimulus, they may be taken from under the control of the will and rendered altogether involuntary. Thus spasm and convulsion are accounted for by an action over which the will has no control. This may emanate directly from the brain, as we see in injuries of the head, or from some new, extraordinary, and powerful motive stimulus acting upon it. Such an one is injury or irritation of the nerves of the cerebral, spinal, or sympathetic division. It may be partial and transient, as in the cramp of the gastrocnemius; or with intermissions, or diffused, as in epilepsy, infantile convulsions, and tetanus; or it may be continued and partial, as in chorea; or continued and diffused, as in the paroxysm of frenzy.

"This is an extraordinary motive impulse, both as to origin, and course, and kind, operating upon the voluntary muscular system. But the involuntary, though less exposed and less frequently excited, is not beyond its reach: hence spasm of the heart, of the diaphragm, of the stomach, and the intestines. The nerves of these organs are those which belong to both systems—the cerebro-spinal and the sympathetic; for we are, perhaps, more accessible to pain from derangement of the vital functions than of any; and though we are not conscious sufficiently to identify the sensation which the blood produces on the lining membrane of the heart, or of food on the stomach, or of air on the cells of the lungs, we are quickly sensitive to the defect of these necessary stimuli to the motive power, by the distress which it occasions throughout the system: and this must be conveyed by the same nerves which render their morbid affections painful. Independently of the pneumogastric, the sympathetic ganglion upon the sensitive nerves of the posterior column establishes the communication of the cerebro-spinal and sympathetic sensitive systems; and as the muscles under the guidance and control of volition, which, in the ordinary state of health, are excited by the cerebro-spinal system of sensitive nerves, in disease obey the sensitive nerves with-

out its superintendence; so the sympathetic system, which in health maintains a partial independence of the cerebro-spinal, and is the centre of its own sensitive impressions and motive impulses, in disordered or diseased states of these organs, propagates its impression, and involves the cerebro-spinal system in an active participation and sympathy with its distress.

"Yet, as we have observed, the nerves which are proper to the special function of any organ are not those which convey the general sensitive impression which is the source of pain: as the optic nerve is not susceptible of conveying any sensation but that of light, or the olfactory but that of smell, so the morbid impressions are conveyed by the sensitive nerves in communication with the organ—whether the eye, or the heart, or the stomach—to the brain; and thence arise the morbid actions, the involuntary motors being excited to action by the extraordinary and powerful stimulus of morbid sensation. And it is this partial substitution of the involuntary for the voluntary, and the voluntary for the involuntary stimulus, or, more accurately speaking, of the sympathetic for the cerebro-spinal, and this for the sympathetic stimulus, which constitutes the derangements of consent and of action which we see in extreme cases of constitutional irritation. We see the muscles of voluntary and involuntary motion, as of the limbs and the heart, the muscles of respiration and of locomotion, obeying other than their natural and accustomed stimuli. It is true that the heart in spasm does not act at the command of the will, as the voluntary muscles in spasm defy the will; but its actions are so violent or so feeble, so unequal and inconsequent, as to excite universal agitation, and the utmost efforts of the whole muscular system in support of respiration and circulation, so as in some instances to render their continuance almost an act of volition; in short, to render the exertions of the cerebral system, while its powers remain, so far predominant as only establishes the partial independence of organic life, which we witness in the phenomena of sleep and death. The influence of the emotions and passions of the mind over the organic actions are sufficient to show the cerebral power of interference with them under extraordinary excitement."

I have made these copious extracts

from the instructive and carefully digested observations of Mr. Travers, for a two-fold purpose: 1st, to corroborate (which, to my thinking, they do most satisfactorily) the convulsive theory of mental disorders, and their natural affinity to the neuroses, epilepsy, hysteria, chorea, &c. &c.; 2nd, to prove, by the multiplied experience and logical arguments of so experienced a surgeon and indisputable an authority, the intimate and inseparable connections which combine together and incorporate into one focus the several portions of the nervous system.

It appears to me that we cannot take a philosophical and comprehensive view of the phenomena of disease, or apply our remedies with any degree of rational or scientific precision, unless we constantly bear in mind the reciprocal and consentaneous action of the several nervous centres upon each other and upon the blood. Even in so simple an affair as a common abscess or boil we have the clearest ocular demonstration of the principle that the whole system sympathizes with the local affection: and how, except through the medium of nervous influence, could morbid sensations be transmitted from the periphery to the centre, and the cerebral or spinal impressions (as the action may be motor or reflex) communicated from the centre to the periphery? That the constitution does respond to local irritation we have convincing proof in the rigor accompanying, or rather preceding, the formation of pus, even in the most remote portion of the human body. How, then, shall we presume to locate mental disorders exclusively in the vesicular portion of the brain, when common observation, or the most superficial attention to the morbid phenomena present, must convince us that the whole nervous system—cerebral, spinal, and ganglionic—and likewise the vascular and assimilative apparatus, are implicated in the disorder?

I feel thoroughly convinced, from an extensive research and laborious inquiry into the anatomy, physiology, laws, and actions of the nervous system,—as propounded by Carpenter, Watson, Travers, Henry Holland, Calvert Holland, &c., &c.—that we ought to view the noisy vociferations, restless mobility, and intensely accumulated excitability and exaltation of the insane, simply as an effort of nature to throw off the *mater*

ies morbi; and that the only philosophical method of dealing with nervous disorders, whether insanity, convulsive or spasmodic affections, is (after first removing all extraneous sources of excitement, mental or physical, and regulating the morbid or perverted secretions) to allow nature free vent for getting rid of the superfluous pent-up morbid irritability, by muscular exercise, the most obvious and natural outlet for the nervous energy, cheerful and agreeable occupation, a plentiful allowance of pure air, regular hours of rising and retiring to rest, a careful avoidance of all sources of mental excitement or irritation, plain, nutritious, unstimulating diet, cold shower-baths, sponging the surface of the body (in females, the spine more especially) with cold water containing salt, &c. &c. By adopting such a line of practice, even if we do not cure our patients, our remedies will not undermine the constitutional powers like copious depletion, salivation, caustic issues, long-continued courses of powerful medicines, drastic purgatives, rigid confinement to the couch, sick chamber, &c. &c. The sound common-sense observations of Sir Benjamin Brodie (contained at page 265 of his *Lectures on Pathology and Surgery*) cannot be too strongly or repeatedly impressed on the junior members of the profession:—"No one can be constantly dosed with medicine without the health being ultimately injured by it; and if you have not some reasonable grounds for giving medicine, you have no right to run the risk of doing harm by its continued exhibition. It is much more wise, as well as much more honest, when you do not know what to do, to do nothing, and advise your patient to wait and take the chance of the pain subsiding of itself, as, in fact, it does in a great many instances."

It is my intention (with the kind permission of the Editor) to adduce further arguments (in a future communication) in support of the unity of the nervous system, and to prove, by reference to the actual phenomena of insanity and other nervous disorders, the folly and mischief of acting on the supposition that any one of the nervous—cerebral, spinal, or ganglionic—centres has an action wholly independent of the rest, or is governed by laws inapplicable or inappropriate to the whole.

Belper, South Derbyshire, April 1850.

MEDICAL GAZETTE.

FRIDAY, JULY 19, 1850.

THE facility with which the Plea of Insanity has been received in many criminal cases of late years, has naturally led to a closer examination of the legal and medical grounds upon which a person charged with an offence should be held irresponsible for his act. The case of a person named PATE, recently tried at the Old Bailey for an assault on the Queen, was well calculated to bring this question to a close issue. It was distinctly proved that the prisoner was odd and eccentric in his habits, and that on various occasions since 1842 he had been guilty of strange conduct in reference to mere trifles. His character, however, had undergone no marked change within a recent period: he had not been treated as an insane person, and had always managed his own affairs with average ability, and without rendering the interference of others necessary. Dr. Conolly, who had been consulted by his father respecting him about nine months since, admitted that Mr. Pate was labouring under no delusion, and that he knew the distinction between a right and a wrong action, but was subject to sudden impulses of passion. In committing the assault, Dr. Conolly considered that he had acted under some *strange sudden impulse, which he was quite unable to control*. Dr. Monro stated that the prisoner might have known what he was doing, and that the act of assaulting the Queen was very wrong, but he was satisfied the prisoner was of unsound mind.*

* This led to a somewhat indecent and uncalled-for reproof on the part of the judge. If there was any impropriety at all, it was in the counsel putting the question, and not on the part of the medical witness in answering it. The witness

We have it on good authority that the prisoner himself admitted the wrongfulness of the act, and that he committed the assault under some momentary impulse at the sight of her Majesty, which he could not resist.

The defence of such a case on the plea of insanity was beset with difficulties. Supposing it to be successful, the result would be incarceration for life in a Criminal Lunatic Asylum,—in itself a dreadful punishment for a person labouring under no delusion, and manifesting no insanity, in the common acceptance of the term, but, on the contrary, quite reasonable enough to appreciate the horrors of an imprisonment for life with lunatics. We cannot suppose that those who would sincerely urge such a defence, would propose to allow liberty to a man whose impulses were so sudden and uncontrollable that no one could be regarded as safe from his violence; and it would be ridiculous to assume, in a case of this kind, that there could be any certainty of cure. In short, had the learned counsel succeeded in such a defence, the safety of the public, which is far above any private considerations, would have absolutely demanded nothing short of imprisonment for life. There could have been no proof of cure after any period of confinement greater than that which was deducible from the prisoner's condition even the day before the assault;

merely said, in answer to the question, "he was satisfied the prisoner was of unsound mind." Would the learned judge have wished him to remain silent, or to have returned an answer conflicting with the evidence which he had previously given? The witness, after having replied to the question, was rather uncivilly told by Mr. Baron Alderson not to take upon himself the functions of judge and jury. "While I am sitting upon the Bench, said the learned judge, I will not permit any medical witness to usurp the functions of both judge and the jury." Dr. Monro very properly replied—"he considered he had only answered the question that was put to him." The judge should have interrupted the counsel, who was, no doubt, well able to justify his conduct, and not have reproved a medical witness, who, from his position in Court, could not defend himself from an attack of this kind, happily very rare, issuing from the judicial Bench.

and it is clear that he was not then a proper object of confinement or restraint, or his friends would not have allowed him to be at large. Even Dr. Conolly, when consulted about him nine months previously, advised that nothing should be done at that time. It is clear, therefore, he then thought so lightly of his condition that he apprehended no evil, and it is only *after* the act that we find an opinion expressed that the prisoner was of unsound mind.

We come, therefore, to the conclusion that, but for the assault on Her Majesty, there would have been no imputation of insanity against Mr. Pate. The acts relied on as indicating this weak mental condition may be paralleled any day within the circle of any man's acquaintance. We could enumerate acts of a much more strange and apparently unreasonable kind, perpetrated by men with whom no one would either have dared or have had a legal right to interfere; and the simple question is, therefore,—Shall such persons escape punishment *because* they commit breaches of the law, or shall a *momentary impulse* be a sufficient plea to ward off punishment from an offender, when the only evidence of a want of power of control consists in a medical inference derived from the extraordinary nature of the act, and the statements of the prisoner who is to benefit by the admission of the plea? Supposing that Mr. Pate had assaulted a mechanic in the open street instead of Her Majesty,—if the defence set up were good it would have been equally valid in this case. It cannot be assumed that the impulse was irresistible *because* the assault was upon Her Majesty, and that it would have been resistible had it been perpetrated upon one of her subjects! Such a mode of reasoning would be absurd. Wanton and unprovoked assaults are daily committed in the streets by persons who *could*, perhaps, produce good evidence

of general eccentricity, if such a plea as an uncontrollable impulse were admissible; but, in spite of the motiveless character of these attacks, the offenders are properly punished, partly as a matter of retribution, and partly to prevent their example from being generally followed. The safety of society demands this; and the security of Her Majesty demands that she should be equally protected from the acts of individuals who, after having inflicted serious personal injury, may coolly plead that they could not avoid it: they had a sudden impulse to stab, to put out an eye, or to break a head, and they could not control themselves! The very object of law in a social state, is to teach a person not to give way to the impulses of savage life.

Mr. Cockburn, counsel for the prisoner, put it to the jury that "the act itself was that of an insane man." "it was motiveless, objectless." He asked—"What motive could there be for the commission of the offence?" Such a line of argument would either prove nothing, or it would prove too much. What motive that would satisfy Mr. Cockburn could any individual in the kingdom have for assaulting Her Majesty under any circumstances whatever? We say none. If, therefore, the view of the learned counsel be correct, and his reasoning in favour of the acquittal of Pate have the slightest force, an assault on the Queen, whatever might be the result, should be taken *ipso facto* as evidence of insanity. In Pate's case there was not even the pretence of a delusive motive existing in the prisoner's mind, or a want of knowledge of the wrongfulness of the act at the time of its perpetration; and yet because, as it must necessarily happen in an assault upon the Queen, there was no motive, it is contended there should be no conviction, and no punishment. The Sovereign of these realms is, therefore, to

have a smaller degree of protection thrown around her than the meanest of her subjects. The learned gentleman who used this argument is now Solicitor-General, and it would only be placing him in a fair position to have his argument turned against himself; and its validity tested, in the event of his being called upon to exercise his office on some future occasion for the protection of Her Majesty against a similar dastardly assault.

It is fortunate, however, that the law does not admit this fast and loose way of dealing with impulses,—i. e., of making them irresistible or not, according to the station of the person who is the victim of them.

Mr. BARON ALDERSON, in addressing the jury on the evidence, said—"In the first place, they must clearly understand that it was not because a man was insane that he was unpunishable; and he must say that upon this point there was generally a very *grievous delusion* in the minds of medical men. The only insanity which excused a man for his acts was that species of delusion which conduced to and drove a man to commit the act alleged against him. . . . They ought to have proof of a formed disease of the mind,—a disease existing before the act was committed, and which made the person accused, incapable of knowing at the time he did the act that it was a wrong act for him to do."

Such a statement of the law may be, medically speaking, open to some objection; but, while it is the law of the land, we are bound to obey it: and to us it appears to afford a better and stronger protection for life and liberty than the doctrine of "irresistible impulses," when nothing but eccentricity of character can be proved. It is to be regretted, however, that the summing-up was marred by the undignified and flippant statement that "A man might say that

he picked a pocket from some uncontrollable impulse, and in that case the law would have an uncontrollable impulse to punish him for it!" It is said that the Law is a silent judge, and the judge a speaking Law. We have here, however, a judicial joke more befitting the pages of PUNCH than the lips of an occupant of the judicial Bench.

We conclude our remarks by observing that we do not remember to have met with a case in which the plea of insanity was advanced upon weaker grounds than in this. Admitting Dr. Comolly and Dr. Monro to be correct in their opinion that the prisoner was of unsound mind, it is clear from the present state of the law that his unsoundness or weakness of mind had not reached that degree to render him irresponsible for offences committed against others. Mr. PARK was not so mentally unsound as to be unsusceptible of correction by punishment; and the verdict returned against him will, we think, have the good effect of preventing others in the same weak mental condition from committing an assault upon Her Majesty. If the moral powers of such persons are not sufficient to control their criminal impulses, the recollection that a similar act was punished by seven years' transportation, may suffice to restrain the arm thus wantonly raised against the Sovereign.

DIABETES RESULTING FROM AN OVERDOSE OF NITRATE OF POTASS. BY DR. CARDON.

A MAN swallowed about three ounces of nitre, by mistake for two ounces of sulphate of magnesia. The immediate effects were violent irritation of the intestinal mucous membrane, and profuse diuresis. The intestinal irritation subsided, and was followed by incessant desire to eat and drink, accompanied by urination to the extent of seven or eight pints during the night.—*Journal de Chimie Médicale*. X

COURSE OF LECTURES
ON
DISEASES OF THE HEART.

*Delivered at St. Vincent's Hospital during the
Session 1849-50.*

BY O'BRYEN BELLINGHAM, M.D.

Fellow of, and Member of the Court of Examiners
of, the Royal College of Surgeons in Ireland, and
one of the Medical Officers of the Hospital.

LECTURE VIII. (Concluded).

EXAMINATION OF THE HEART IN DISEASE,
CONTINUED.

*Fremissement cataire, or purring tremor—
Circumstances under which fremissement
cataire is felt—Cause of fremissement
cataire—Signs furnished by percussion—
Alterations in the extent and degree of the
heart's superficial dulness—Conclusions
respecting percussion of the præcordial
region.*

THE cause of jugular pulsation was first satisfactorily explained by MM. Bertin and Bouillaud—viz., that it depends upon regurgitation into the great veins during the contraction of the right ventricle; and this theory of its cause has been almost universally adopted. Dr. Hope, however, supposed that it might have its cause in the recoil of the tricuspid valve upon the blood, when the hypertrophied ventricle contracted with increased force, and that the column of blood descending into the ventricle would be repelled with sufficient force to propagate its impulse as far back as the jugular veins. From the explanation which has been given of the manner in which the curtains of the auriculo-ventricular valves close their respective orifices, it would appear that jugular pulsation could scarcely be produced in that way. Why regurgitation is permitted at the tricuspid orifice so frequently, as well as the exact manner in which it occurs, have been already alluded to in treating of the "safety-valve function of the tricuspid valve."

FRÉMISSERMENT CATAIRE, OR PURRING
TREMER.

Another sign which is recognised only by the application of the hand to the parietes of the chest is the "fremissement cataire" of Laennec, the "purring tremor" of English writers, the "frottement fremissement ou vibratile" of Gendrin; named so by Laennec, from its resemblance to the peculiar sensation experienced if the hand is laid upon the back of a cat when the animal is making the peculiar purring sound which it does when pleased; and compared

by M. Bouillaud to the thrill felt on placing the hand upon the larynx of a person singing. It is a peculiar thrill, or vibratory sensation, very easily recognised, sometimes perceptible on very slight pressure upon the part, sometimes requiring a stronger pressure in order to be felt, and communicated equally by the large arteries as by the heart.

This phenomenon had been recognised as a sign of cardiac disease previous to the discovery of auscultation. Corvisart, speaking of the symptoms of disease of the left auriculo-ventricular orifice, observes—"Among the symptoms of this affection there is a peculiar confused sensation, difficult to describe, felt by the hand applied to the præcordial region." He supposed it to indicate a contracted state of the mitral orifice, causing an impediment to the passage of the blood from the left auricle into the left ventricle. Burns evidently alludes to this phenomenon when he speaks of a "jarring motion accompanying the pulsation of the heart;" and, as he noticed it in several cases where, on a post-mortem examination, the opposed surfaces of the pericardium were found to be adherent, he attributed it to this lesion, causing an impediment to the free action of the heart.

Fremissement cataire is not limited to the heart, but is perceptible also in the large arteries, and in aneurism, particularly in varicose aneurism, in which it is often better marked than in the heart. It is felt over the large arteries when the lining membrane of these vessels is extensively diseased, as well as in cases where no morbid alteration of the parts exists; and its cause, or the mechanism by which it is produced, do not seem to have ever been fully explained. Laennec* observes—"It would seem that the immediate cause of a phenomenon so marked as fremissement cataire would be easily discovered. I confess, however, that, notwithstanding the pains which I have taken in this respect, I have not been able to find any satisfactory reason." "It appears to me (he adds) extremely probable that it is owing to some peculiar modification of innervation." M. Bouillaud,† in the second edition of his work on the heart, expresses himself in somewhat similar terms:—"What are the other causes capable of producing this phenomenon (he observes) I am ignorant. It appears to me that there is a certain condition of the blood, as well as of the parietes of the heart and arteries, which favours its production. But in spite of the assiduous researches to which I have devoted myself on the subject, I have not yet been able to determine properly all these conditions."

* Auscultation Médiate, tom. ii.

† Traité des Maladies du Cœur, tom. i.

Circumstances under which fremitus cataire is felt.—In every instance in which fremitus cataire is felt, a murmur of some kind is audible on auscultation, which is frequently blowing, and sometimes rough or harsh. It is obvious, therefore, that the cause which produces the latter is connected with the cause which gives rise to the former; but, as fremitus cataire is not felt in every case in which an abnormal sound is heard, it is obvious that there must be something superadded when the latter is felt to allow of this peculiar sensation being communicated to the hand.

The morbid condition of the heart in which this phenomenon is most generally perceptible is where the left auriculo-ventricular valve permits regurgitation, and the ventricle is at the same time hypertrophied. The morbid conditions in which it is felt in the arteries are where the aortic valves permit free regurgitation, or where the arch of the aorta is dilated, its lining membrane rough and irregular from adventitious deposit, and the large vessels which come off from its arch are likewise dilated; where an aneurismal sac springs from a large artery, where a free communication exists between a vein and artery, as in varicose aneurism; or, finally, where the blood is altered, its viscosity diminished, its watery parts increased, and the vessels are in an unfilled state.

Two or more of the foregoing states may be combined together, by which the thrill will be increased. Thus dilatation of the arch of the aorta may be combined with disease of its lining membrane, and both may be combined with a state of the aortic valves permitting regurgitation; or dilatation of the large arteries, or regurgitant disease of the valves of the heart, may be combined with a state of anæmia and alteration of the blood.

Cause of fremitus cataire.—In every case in which fremitus cataire is felt, whether in the heart, or in the arteries, or in aneurism, the cavity or the vessel is in an unfilled state. Thus in the heart it becomes perceptible when the mitral valve permits regurgitation; in the large arteries, when the aortic valves are patent, when the arteries themselves are dilated, when an aneurism exists, or when a communication exists between a vein and artery. In several of the foregoing cases, regurgitation of the blood occurs; this presupposes an unfilled state of the vessel or cavity: it could not occur if they were distended. Now I consider this unfilled state of the vessels combined with a certain amount of force and velocity of the current of blood to be the immediate cause of this phenomenon: the thrill or vibration communicated to the hand being more marked when

the lining membrane of the part is rough or irregular from disease.

For instance, we can produce a fremitus at any time in a large artery in a healthy subject by making pressure upon it; the thrill is not felt at the point upon which the pressure is made, nor between it and the heart, but at the distal side of the point of pressure, and where the vessel must necessarily be in an unfilled state: a murmur is heard at the point compressed, and both cease as soon as the pressure is removed. We know, too, that a thrill, identical with fremitus cataire, is felt in cases where the heart and arteries are perfectly healthy, but where the vessels are in an unfilled state, as in anæmia, depending upon hæmorrhage or other causes.

A certain amount of force and velocity of the current of blood appears to be likewise necessary to its full development; because we find that this phenomenon sometimes diminishes or even disappears in diseased states of the heart, when the action of the organ becomes tranquil, and that it reappears again when the heart's action is excited by exercise. It disappears likewise when the action of the heart becomes feeble, and ceases altogether in the last stage of valvular disease when the circulation through the heart is much impeded.

The jarring pulse in the radial artery, with which we are familiar in cases of patency of the aortic valves, appears to be nothing more than the fremitus cataire felt in an artery of small calibre. The thrill communicated to the finger is characteristic of an unfilled state of the arteries, and becomes perceptible whenever the semilunar valves of the aorta imperfectly close the orifice, or when regurgitation occurs into the aorta from the large arteries which come off from its arch. The same character of the pulse is observed in cases where profuse hæmorrhage has occurred; and in a less marked degree in cases of anæmia from other causes.

Fremissement cataire may or may not be a sign of organic disease; in the majority of cases it is: thus, whenever it is felt in the præcordial region, accompanies the ventricular systole, and is well marked, it indicates valvular disease; and the form of valvular disease which will be found under such circumstances is a state of the mitral valve or orifice permitting regurgitation. In the large arteries, on the other hand, a fremitus may be felt, although no organic disease of the vessel exists. In some instances this phenomenon is perceptible when the hand is laid gently upon the parietes: in others stronger pressure is required in order to develop it. Thus in varicose aneurism, the result of a wound of an artery and vein, a thrill is communi-

cated to the finger on the slightest touch; while in cases of anæmia some pressure upon the artery is often required in order to make it evident.

A peculiar thrill or vibration is occasionally perceptible in the region of the heart, that must not be confounded with the phenomenon just described, and which arises from the direct friction of two rough surfaces during the motions of the heart: here it depends upon the friction between the opposed layers of the pericardium, which are roughened in consequence of the deposition of lymph upon them; the thrill has a different character from that of fremitus cataire, and is felt at a different part of the præcordial region. A somewhat similar feel is occasionally experienced when the hand is laid upon the side in cases of pleuritis or pleuro-pneumonia with false membranes; in bronchitis, likewise, when the sonorous râle is loud, a sensible vibration is sometimes communicated to the hand: in the latter cases, however, it accompanies the respiratory movements, not the heart's action.

SIGNS FURNISHED BY PERCUSSION.

The præcordial region includes the whole of that portion of the chest beneath which the heart is situated; and, in a healthy subject with a well-formed chest, the extent of surface in this region which yields a dull sound on percussion is limited. We have seen that the lungs are in contact with the greater portion of the anterior surface of the thorax, and, of course, wherever they are, the sound elicited by percussion will be clear. Where the anterior margin of the opposite lungs separates from one another, the anterior surface of the heart comes in contact with the thoracic walls, being only separated by the pericardium and cellular tissue.

The portion of the heart uncovered by lung is very small, seldom exceeding two inches in any direction: it has a triangular shape—the base below, the apex above; it consists of a portion of the apex of the right ventricle, and of part of the left ventricle near its apex, and is seated on a plane below the nipple and the fourth left rib: its base is on a line with the cartilage of the sixth left rib; its apex is at the point where the margins of the opposite lungs begin to separate from one another; its right boundary (which is constituted by the thin edge of the right lung) is nearly a vertical line through the centre of the sternum; its left boundary (which is constituted by the thin margin of the left lung) is an oblique line through the cartilages of the fifth and sixth left ribs.

This, then, is the only portion of the heart which in the healthy subject is in contact with the parietes of the chest, and

it is the only portion of the præcordial region which yields what can be termed a dull sound on percussion. It is not, however, quite correct to term this a dull sound: it is less dull than that yielded by the hepatic region: the liver being a solid organ, and the heart hollow, the sound elicited by percussion over the former is more dull than over the latter. Indeed, as Dr. Latham* observes, "percussion here conveys to the ear a sense rather of less resonance than of positive dullness." "If the percussion used be but of moderate force, you must listen attentively to make sure that the resonance is really less here than elsewhere. It is only when the percussion used is of a force somewhat painful to the patient, that the ear begins to acknowledge a positive dullness." On strong percussion, likewise, a difference in the sound can be detected where the thin margin of the lungs covers the heart. The sound here is intermediate between the clear sound heard on percussion over the lungs, and that yielded by the part of the præcordial region where the heart is in contact with the parietes of the chest. We can thus, as M. Piorry remarks, trace the vertical and transverse diameters of the heart, and we can measure its size from above downwards, and from side to side. The extent of the surface of the heart which is naturally in contact with the parietes of the chest is diminished somewhat, it must be recollected, in the supine posture; still more by taking a full inspiration. It is increased by leaning forward, and still more by a forced expiration.

In employing percussion in cases of disease, we may commence the examination of the præcordial region either above, below, or laterally; we may trace the sounds yielded by the lungs above and upon each side to this region; or, commencing below, we may trace the sounds from the region of the stomach, and of the left lobe of the liver, upwards. The mode of manipulation recommended by Dr. Hope† is "to lay one finger over the decidedly dull part, and another over the slightly resonant edge of the lung, when, by striking the two fingers alternately, the arched line along which the organ lies in contact with the walls may be traced with surprising accuracy, unless the subject be remarkable for obesity, which obscures the resonance." "In females, the mammae may be pushed upwards, which generally leaves the dull portion sufficiently accessible."

Alterations in the extent and degree of the heart's superficial dullness.—The extent of surface in the præcordial region which yields a duller sound than natural on per-

* Lectures on Diseases of the Heart, vol. i.
† Treatise on Diseases of the Heart, 4th edit.

cussion is seldom diminished, owing to diseased states of the heart; but it is very often enlarged, while the degree of dullness is not unfrequently at the same time increased. The space will be diminished if the heart is congenitally smaller than natural, or if the lungs are largely developed; and it will disappear if the anterior margin of both lungs is emphysematous, and their edges meet in front of the organ. But, as the heart is generally somewhat enlarged under the latter circumstances, a portion of its anterior surface will still come in contact with the parietes, but the space which yields a dull sound will be on a plane somewhat lower than natural.

The region of the heart's superficial dullness will be increased whenever the heart is enlarged, or whenever fluid to any amount is effused into the sac of the pericardium; in the latter case the degree of dullness will be likewise much more marked. Thus, if the walls of the ventricles are hypertrophied, or if their cavities are dilated, a dull sound will be elicited by percussion over a wider surface; and the extent of this surface will be in some degree a measure of the increased size which the organ has attained: the enlarged heart pushing aside the lungs, and a larger portion of it coming in contact with the parietes in the præcordial region. When the two foregoing conditions are combined, the heart attains the largest size that it is capable of, and the præcordial region may yield a dull sound over a square surface of from four to six inches, in place of two. When the hypertrophy predominates over the dilatation, the space which yields a dull sound is greatest from above downwards; when dilatation predominates over the hypertrophy, the region in which a dull sound is yielded is greatest transversely. In pericarditic effusion, likewise, a large surface in the præcordial region yields a dull sound, while the degree of dullness is much more pronounced, and the amount of fluid effused may be pretty accurately measured by the extent of the dullness on percussion.

When a dull sound is elicited by percussion over an extensive surface in the præcordial region, the situation in which it is most marked, and the amount or degree of dullness, will be a guide as to its cause. When it depends upon enlargement of the heart, the site of the dullness is lower down, and more to the left side, than when it depends upon liquid effused into the sac of the pericardium. As the pericardial sac extends upwards to a level with the articulation of the second rib on the left side with the sternum, and sometimes, as I have found to be the case, as high as the first rib, if a large amount of fluid is contained in this sac a dull sound will be elicited by percussion as high as this point;

while the degree of dullness on percussion over a fluid is always much more marked than over the heart itself. It is said that hypertrophy may be distinguished from simple dilatation of the ventricles by the degree of resistance to the fingers employed in percussion being greater in the former than the latter; but there are other and better signs by which these conditions of the heart may be distinguished.

The results furnished by percussion are, however, sometimes doubtful; for instance, when the margins of the lungs are emphysematous, and approach each other in the præcordial region, a clear, instead of a dull sound will be yielded in the natural situation of the heart's superficial dullness. When the lungs are solidified, either as the result of pneumonia or of tubercular infiltration, or from any other cause, a dull sound will be yielded upon each side of the heart over a considerable surface. When the heart is displaced, owing to liquid effusion into either pleura, or to some morbid growth in the cavity of the thorax, the region of the heart's superficial dullness will be altered.

Conclusions respecting percussion of the præcordial region.

1. The region of the heart's superficial dullness in the healthy adult subject does not exceed two inches in any direction. This space is on a plane below the nipple and the fourth left rib; its centre about the middle of the cartilage of the fifth left rib.

2. The sound elicited by percussion here is not absolutely dull unless pretty strong percussion be used.

3. The region of the heart's superficial dullness may be diminished, or it may be increased, or the degree of dullness may become more marked, owing to disease. It is much more frequently increased than diminished.

4. It will be diminished if the heart is naturally small, or the lungs are large and overlap the organ; and it may disappear if the anterior margin of both lungs is emphysematous.

5. It will be increased whenever the ventricles are hypertrophied, or their cavities are dilated, or when fluid is contained in the pericardial sac.

6. The seat of the diminished resonance always extends higher up, and is more extensive, as well from above downwards as laterally, when much fluid is contained in the sac of the pericardium, than when hypertrophy or dilatation of the heart exists.

7. The degree of dullness is always much more pronounced when fluid is contained in the sac of the pericardium than when the heart is enlarged.

Rebiews.

The Three Kinds of Cod-liver Oil, comparatively considered with reference to their Chemical and Therapeutic Properties. By L. J. DE JONGH, M.D., of the Hague. Translated from the German, with an Appendix and Cases, by EDWARD OBBY, M.D. 8vo. pp. 176. London: Taylor and Walton.

Cod-liver Oil: its Uses, Mode of Administration, &c. &c. Compiled from the best and latest Authorities, British and Foreign, by JOHN RAYNER. Pamphlet, 8vo. pp. 16. London: Churchill.

THAT cod-liver oil has maintained its position as a therapeutic agent of some value, notwithstanding its injudicious and indiscriminate laudation, by many who have spared themselves the pains of investigating its properties, is proof that it has some real therapeutic action. If, however, some may still think that it needs extraneous support, it may be found in the "First Medical Report of the Hospital for Consumption," which we have already noticed in a leading article (Jan. 14th).

We, however, again extract the following paragraph from this Report, as affording a sufficient reason for giving our readers an abstract of the works before us. The medical officers of that institution, Drs. Hamilton Roe, Theophilus Thompson, George Cursham, Richard P. Cotton, Richard Quain, and John Bowie, thus express themselves in reference to the medicinal properties of cod-liver oil:—

"From these facts, and a more extended experience since the period at which this report terminates, no other conclusion can be drawn than that cod-liver oil possesses the property of controlling the symptoms of pulmonary consumption, if not of arresting the disease, to a greater extent than any other agent hitherto tried" (p. 41).

Regarding this statement, then, as the first authoritative document relative to the therapeutic qualities of cod-liver oil, we take this opportunity of bringing the subject under our readers' notice.

The first section of Dr. De Jongh's treatise contains the source, method of

obtaining, and physical properties of cod-liver oil.

"Oleum Jecoris Aselli sive Jecinoris Aselli, Olerum Morrhua, Levertraan, Huile de Morue, Cod Oil, Olio di Baccala," are its various synonyms.

This oil is obtained from the livers of several species of *Gadus*, a genus of fish of the order Thoracici, family Malacopterygii. They are found in the Northern Atlantic, Levant, and Mediterranean Seas. There are six different species of *Gadus* which are found in the northern seas, and are taken on the Irish, Scotch, Norwegian, French, and American coasts. But it is uncertain from which species the oil is procured: it is even said that the fishermen themselves are not able to discriminate between them. In fact, it is procured from the several species indiscriminately.

Opinions vary also as to the method of obtaining the oil. Dr. De Jongh had instituted inquiries on this point. We quote the following from the letter of one of his correspondents:—

"In commerce there are three kinds of liver oil,—namely, from the dorse, seij-fish, and the pollack. The liver oil from the dorse, which is most noted, is obtained in the following manner:—The livers of 40 to 80 cwt. of stockfish are thrown into tubs, and are allowed to remain there until they become putrescent, when the oil runs off of itself: this, when skimmed, is the pale oil. What remains in the tubs is boiled in iron kettles (from sixteen to twenty hours) until it becomes of a dark brown colour, and is quite clear: this is the brown oil, which is strained, and the residuum in the kettles is used for manure" (p. 8).

The same method is pursued with regard to the livers of the other kinds. The oil differs in quality. The "light brown oil" is obtained from the livers of the seij-fish and the pollack. Its colour is caused by its being left a long time on the livers before it is poured off.

As one of the chief objects of this work was to investigate the chemical properties of cod-liver oil, so a greater portion thereof is occupied with the very full details of the analyses of the various kinds of oil, and of their organic and inorganic constituents. Of these investigations, extending over more than fifty pages, it would be impossible for us, within our limits, to give a sufficiently clear abstract. We must therefore content ourselves with extracting the author's summary.

"One hundred parts of cod-liver oil contain :—

	Brown.	Light brown.	Pale.
Oleic acid with brown substance (gaduine and two peculiar bodies)	69.78500	71.75700	74.03300
Margaric acid	16.14500	15.42100	11.75700
Glycerine	9.71100	9.07300	10.17700
Butyric acid	0.15875	—	0.07436
Acetic acid	0.12508	—	0.04571
Fellic and cholic acids, with some oleine, margarine, and bilifulvin	0.29900	0.06200	0.04300
Bilifulvin and bilifellinic acid, and two peculiar substances	0.87600	0.44500	0.26800
A peculiar substance soluble in alcohol	0.03800	0.01300	0.00600
A peculiar substance insoluble in water, alcohol, and ether	0.00500	0.00200	0.00100
Iodine	0.02950	0.04060	0.03740
Chlorine, with some bromine	0.08480	0.15880	0.14880
Phosphoric acid	0.05365	0.07890	0.09135
Sulphuric acid	0.01000	0.08595	0.07100
Phosphorus	0.00754	0.01136	0.02125
Lime	0.08170	0.16780	0.15150
Magnesia	0.00380	0.01230	0.00880
Soda	0.01790	0.06810	0.05540
Iron	a trace	—	—
Loss	2.56900	2.60819	3.00943
	100.00000	100.00000	100.00000

How far some of these "constituents" may be the result of the analytical processes, or obtained from the utensils employed, we presume not to judge; but we think it probable that some in this long list may have their origin in one or the other of these sources.

The author enumerates the various diseases in which cod-liver oil has been found serviceable. These are rheumatism, gout (both chronic and acute), scrofula, glandular swellings, scrofulous ulcers, chronic exanthemata, strumous ophthalmia, atrophy of children, rickets, mollities ossium, caries, white swellings, phthisis (in reference to the latter disease, the author styles this oil "the grand restorer of health"), otorrhœa, tumors of various kinds, paralysis, and some other chronic and cachectic conditions.

The author had investigated, in three series of cases, the comparative merits of the three kinds of oil, the inferences from which we here subjoin :—

"The three kinds agree so, that all the patients which were treated with cod-liver oil were cured, there being, however, a marked difference with regard to the time in which the cures were accomplished; evidently showing that there is a decided superiority in the efficacy of one kind over the others."

That this variation in the period of

the cure is not unimportant is shown by the author in a table which he has had drawn up for the purpose. From this table it appears that the cures by the brown kind were accomplished in half the time that was required by the light brown or the pale oil.

Although it will be seen, in the table given above, that the chemical composition of the oils varies, it would be extremely difficult to point out that feature in its chemical composition to which the greater efficacy of the brown oil can be attributed. It would, in fact, be equally difficult to indicate the chemical constituents of any or all varieties of the oil to which its general therapeutic properties can be assigned. Dr. De Jongh has the merit of having discovered a new body, which he names *gaduine*, in his analysis. It remains to be seen, from comparative analyses and experiments, how far this substance may perform an important part as a remedial agent, and whether it be peculiar to cod-liver, or to any particular class of oils. Dr. Carey, to whom the profession is indebted for his very excellent translation of this work, adds a few cases and remarks in illustration of the alleged therapeutic virtues of cod-liver oil.

Mr. Rayner's pamphlet is simply a notice of the therapeutic value of cod-

liver oil, the diseases in which it has been employed, and the authorities by whom it has been recommended.

In conclusion, we return to a brief consideration of the statement already quoted from the Medical Report of the officers of the Hospital for Consumption.

The curative agency of cod-liver oil is therein so distinctly affirmed, that, in bringing that drug under consideration, we could not omit some notice of a statement so pertinent, so authoritative, and so likely to influence practice.

We could have wished that the out-patients to whom cod-liver oil was administered had been distinguished from the in-patients; for it must not be overlooked that the nature of the patient's avocation, the degree of exposure to air, his general circumstances, &c., greatly influence the result of any plan of treatment. Such a distinction would also have thrown some light upon the degree of influence exerted upon the progress of phthisis by confinement in the wards of a hospital.

From our perusal of the work of Dr. DeJongh, and our personal acquaintance with the results of the employment of cod-liver oil, it remains still doubtful how far the apparent remedial benefit may not be attributable to its nutritious properties simply. Chemical analysis does not indicate the presence of any active principle in sufficient quantity to effect any therapeutic action, except upon untenable homœopathic doctrines; and a sufficiently extensive series of comparisons with the effects of other animal oils is wanting, to prove the exclusive possession of a specific virtue by the cod-liver oil.

The Report of the Hospital for Consumption shows an increase in the weight of the persons taking this oil, in one instance to the amount of forty-one pounds in sixteen weeks. But it must not be concluded that an equal improvement of the phthisical symptoms takes place *pari passu* with the increase of weight, although it is stated that the decrease of weight and aggravation of symptoms were generally coincident. Professor Paget* has drawn the attention of the profession to the fact that atrophy of all the most important structures may be progressing concurrently with a vast accumulation of adipose

tissue—that, indeed, obesity is but a form of atrophy.

From attentive consideration, therefore, of the facts thus brought before us, and by the aid of our own experience of the action of this remedy, we conclude:—

1. That cod-liver oil is a real therapeutic agent of some value in cachectic diseases.
2. That its value as a remedy has not yet been determined by sufficiently extended observations.
3. That the therapeutic action of cod-liver oil is manifested more especially in its influence on the nutritive functions.
4. That it is not yet determined whether this therapeutic action be peculiar to the elements of which cod-liver oil is constituted, or whether it be common to several varieties of animal oils.

Proceedings of Societies.

ACADEMY OF MEDICINE, PARIS.

July 9, 1850.

Coexistence of Sweating Sickness and Cholera.

DR. BUCQUOY, "*médecin des épidémies*" of the district of Peronne, stated the following to be the relations that he had observed to obtain between these two diseases:—

1. That where the sweating sickness was most severe, the cholera did not appear; and conversely.
2. That if the two appeared simultaneously in any district, the cholera was first to cease.
3. That individuals who had suffered a regular attack of sweating sickness were thereby protected from cholera.
4. That when the two diseases coexisted in a district, the sweating sickness was milder than when it occurred alone.

* * The sweating sickness (Malwah), as met with in India, appears to be closely allied to the worst forms of cholera, and to bear a close relation to malignant congestive fever. Its name, Malwah, is taken from a district in India.—*Dunglison's Medical Lexicon.*

The Intellectual Powers of Deaf-mutes.

M. GERDY read a critical examination of the opinions of Itard on the moral and intellectual powers of deaf-mutes, by M. FERDINAND BERTHIER, himself a deaf-

mute, President of the Central Society of Deaf-Mutes, and Dean of the Professors at the National Institute of Deaf-Mutes, Paris. M. Berthier considered that Itard had incorrectly and unjustly estimated the endowments of deaf-mutes, and proceeded to examine the several points of Itard's judgments.

M. Itard had stated that, being cut off from conversation, the deaf-mute is a solitary being in society, incapable of participating in the thoughts and emotions by which it is occupied or agitated: while, at the same time, he admitted that the deaf-mute has the advantage of the blind-born in the contemplation of nature, since he can include in a single glance of the eye more wonders than the blind person can imagine in a life-time. M. Berthier asserts that, by the aid of gesticulatory language, and by their extraordinary powers of mimicry, deaf-mutes are as capable of perfect instruction as those who have the use of oral language. That, although deprived of hearing, deaf-mutes are alive to all the events of social life. That numerous examples prove them to be capable of the highest intellectual exertions, of deeds of courage, of sensibility, and of patriotism; and susceptible of love, affection, pity, ambition, glory,—on all which points Itard had declared that by their physical condition they were inevitably incapable.

July 16, 1850.

'On Turning in cases of Deformed Pelvis.'

DR. CHAILLY HONORÉ read a memoir, the object of which was the examination of the question, whether it is advisable to substitute version for the use of the forceps in cases where the head presents, the antero-posterior diameter of the pelvis measuring less than two inches and a half. The author discussed Dr. Simpson's proposal to turn in these cases, and concluded that it is in opposition to sound practice, and to all the experience of the greatest authorities in obstetric science. The proceeding, Dr. Chailly-Honoré observed, deprives both mother and infant of every other obstetrical aid, exposes the infant to certain death, and the mother to the risk of serious injury.

Fracture of the Temporal Bone.

M. DEONARD read an essay, in which he observed that fracture of this bone is not always attended with hæmorrhage; that, when the latter occurs, its source is frequently laceration of the carotid artery in the carotid canal; that deafness is a certain sign of this fracture; and, lastly, that it is not always a fatal occurrence.

SURGICAL SOCIETY OF PARIS.

July 10, 1850.

PRESIDENT, M. DANYAU.

Reduction of long-standing Dislocation.

M. FORGET related the particulars of a case in which the reduction of a dislocation of the shoulder-joint, of three months' standing, was accomplished by Lisfranc: the patient died suddenly an hour after the operation. The only cause of death which could be discovered was congestion of the brain.

MM. LENOIR and LABREY related instances of injury to the brachial plexus of nerves, from attempts at reducing long-standing dislocations.

M. HUGUIER stated that a patient to whom chloroform had been administered during the operation of reducing a dislocation of the hip-joint, having died a short time afterwards, from another cause, it was found, at the autopsy, that the dislocation had not been reduced, as was supposed, but that the bone had been fractured.

Some further discussion took place, in which the majority of the members expressed their disapprobation of long-continued attempts to reduce old dislocations. M. Forget expressed himself strongly, on the dangers attending the use of chloroform in such cases.

ACADEMY OF SCIENCES, PARIS.

July 15, 1850.

Electrical Phenomena produced by voluntary muscular contraction.

M. POUILLET read a report on a memoir on this subject, by M. BOIS-RAYMOND, of Berlin. The existence of an electrical current at the time of voluntary contraction of the muscles was admitted, and the following opinion on its cause given, by the commission, composed of MM. Magendie, Becquerel, Rayer, Despretz, and Pouillet:—

That the cause of the current is unknown; it is probable that it does not result from any chemical action. It is not demonstrated that it results from any internal chemical action. According as this problem shall receive an affirmative or negative solution, the ulterior consequences will assume a different character.

Analysis of the Blood of Animals after acute suffering.

M. CLEMENT showed that the blood of animals undergoes a modification in its composition, from acute suffering, and that

under these circumstances the activity of the organic functions is greatly increased. The results stated by M. Clément, from his researches, are—That the water and colouring matter of the blood are augmented in proportion to the decrease of the albumen and fibrin; that the albumen is decreased in the proportion of about a seven-thousandth, and of the fibrin in that of a three-thousandth. That under these conditions, chemical changes are effected rapidly on the soft tissues of the frame, to restore the composition of the blood.

Radical Cure of Inguinal Hernia.

M. VALETTE described a proceeding which he had contrived for this purpose, and which consisted in the reflection of a portion of integument into the canal, the production of ulceration, and the ultimate union of the opposed surfaces by cicatrization. M. Valette had tried this proceeding in five cases, and promised to submit further results for the consideration of the Academy.

Hospital and Infirmary Reports.

ST. GEORGE'S HOSPITAL.

Cases reported by A. WHITE BARCLAY, M.D., Medical Registrar to St. George's Hospital.

THE accompanying cases are of interest, from the fact of the circumstances being so nearly similar, and from the cases occurring within a few days of each other. The remarkable feature presented by both cases is this, that permanent disease of the kidney may derive its origin from stricture of the urethra, and may prove fatal after the urethra has been restored to an almost normal state, and its immediate consequences of chronic cystitis or nephritis are to appearance entirely removed. The second case forms a commentary upon the first, showing the affection at an earlier stage, when neither the stricture nor the chronic inflammation had been entirely removed by treatment.

CASE I.—Henry Skinner, *stat.* 34, admitted into St. George's Hospital on 12th April, 1850, under the care of Dr. Page. This man, a carpenter by trade, was engaged in his business when suddenly attacked by hæmoptysis, bringing up about a quarter of a pint of blood at once, and the sputa continuing mixed with blood at his admission. He said he had suffered from cough during six weeks past, with

pain in the chest and thickish expectoration; and there was dulness on percussion under the left clavicle, imperfect expansion of the chest, especially on that side, and moist râles to be heard at the upper part of both lungs. He believed he had spit a little blood on a previous occasion, but was uncertain. He was ordered a purgative of calomel, to be followed by a senna draught, and a mixture every four hours, with sulphate of alumina, sulphate of magnesia, and dilute sulphuric acid; and, as there was some return of the hæmoptysis, he was cupped next day between the shoulders to eight ounces, with relief. The expectoration diminished, and the blood disappeared from the sputa; and he was looking better, but had a sallow and unhealthy aspect. He complained that his mouth felt parched from the alum in his mixture, which was discontinued.

On the morning of the 18th his tongue was dry and parched, and very coated. He had had slight epistaxis, and was beginning to be confused in mind; was in a nervous and excitable state, and a good deal inclined to sleep: the pulse was quick and rather sharp. The moist and crepitant râles were found to have extended considerably down the back of the left lung, and the breathing to be pretty natural on the right, but was much hurried. His whole appearance led to the suspicion of the existence of disease of the kidney; and, accordingly, on examining the urine it was found tolerably abundant, clear, acid, but containing a very notable amount of albumen. He continued in the same rambling confused state of mind till towards evening, when he became decidedly delirious, attempting to get out of bed two or three times, and in the last attempt falling on the floor in a fit. He was from that time quite unconscious, lying in a state of partial stupor, recognising no one, tossing about a good deal in bed; with contracted pupils, and sordes about the mouth, breathing heavily, and sighing. He passed no urine during that day, but gradually sunk into a state of coma, and died on the evening of the 20th.

Sectio cadav. 55 hours after death:—

Head.—Not examined.

Thorax.—The surface of left pleura smeared with recent lymph, with old partial adhesions at upper and back part. The posterior part of left apex much punctured from contraction of old vomicae, containing chalky matter about size of a hazelnut. Both lobes were partly solidified from recent lymph infiltrated in structure, were soft and friable, breaking down under finger, and sinking in water. The right lung was congested at the lower and back

parts, but otherwise healthy. Heart large, and loaded with fat, slightly hypertrophied in substance, and small patches of atheroma at root of aorta; valves healthy.

Abdomen and pelvis.—The pelvis and infundibula of both kidneys much dilated; secreting structure expanded into thin bags, about quarter inch in thickness, with pale and smooth surface. Ureters distended throughout to about size of little finger. Bladder distended, healthy. Urethra contracted at its bulbous portion to about two-thirds of its normal size; prostate healthy. Other organs presented nothing remarkable.

CASE II.—John Middleton, æt. 43, admitted on 17th April, 1850, into St. George's Hospital, under the care of Mr. Tatum, alleged to have suffered for some time past from stricture, and with the view of having the catheter passed. He presented a rather sallow and unhealthy aspect, so as to suggest to his surgeon the idea that the case might be one of suppression from disease of the kidney, rather than of retention produced by stricture. He was ordered a purgative at once, and a warm bath in the evening; and, having passed water without difficulty in the course of the night, an attempt was made to pass the catheter, which was opposed by spasmodic action, in consequence of which it was not repeated for some days, and he continued using the warm bath, and taking a mild sedative every night, with an occasional dose of castor oil. The catheter was again introduced on the 27th, and passed without difficulty. Nurse reported that he had a slight rigor in the night; and, observing his breathing somewhat stertorous, and his sleep heavy, she called the attention of the house-surgeon to the case, by whom Dr. Bence Jones was asked to see him. It now appeared that some peculiarity of manner had existed all along, but had become much more marked this morning; and, at the hour of visit, he started up when waked out of sleep, seemed confused and flurried, and could not give a very distinct account of his feelings. His pulse was full and bounding; tongue coated, and inclined to be dry; the bowels had acted freely; and, on examination of the chest, nothing was observed, with the exception of sonorous and moist râles at the lower and back parts of both lungs. The urine was desired to be saved for examination, and he was ordered to be cupped to ℥iv . on the loins, had a saline draught with antimonial wine every six hours, and a compound jalap powder next morning. A smaller quantity of blood than had been ordered was obtained by cupping; and becoming comatose in the evening, he was bled, from the arm in a full stream, during

which the pulse sensibly flagged; but after a short time he rallied, said he felt better, and continued comfortable through the night, relapsing into coma in the morning. Blisters were applied to the neck, the bleeding repeated, purgative enemata, &c., administered; but there was no return of consciousness, and he died on the evening of the 30th. The urine was found to be very highly albuminous, but containing no fibrinous casts or blood globules.

Sectio Cadav. 17 hours after death.

Cranium: The sinuses and large veins distended with dark blood. A considerable quantity of serous fluid in the cavity of the arachnoid, and also infiltrated into the subarachnoid areolar tissue, over the surface of the hemispheres, and at the base of the brain. Substance of the brain wet, puncta numerous and dark. Ventricles distended with clear serum.

Thorax.—Ten old adhesions in left pleura. Both lungs gorged with frothy serum: a few tubercles at the left apex. Heart normal: a few patches of atheroma in the aorta.

Abdomen and Pelvis.—Kidneys large, and much surrounded by fat. Pelves and infundibula greatly distended, expanding the substance of the kidneys, and forming pouches which dipped between the pyramids; found to be lined by mucous membrane, which was very vascular, and filled with a smooth creamy-looking pus. The substance of the kidney was much wasted, soft, pale; structure indistinct, and in many parts scarcely a quarter of an inch in thickness. In the cortical structure of the right kidney there were numerous cysts, and some small abscesses of the size of pins' heads, which seemed to be uriniferous tubes distended with pus. The investing membrane was rather adherent; surface pale, smooth, and marked by vascular stellæ, and a few cysts. The ureters were distended to the size of the little finger. Bladder distended; filled with urine mixed with pus; considerably hypertrophied, and mucous membrane very vascular.

The bulbous portion of the urethra presented considerable irregularity, and some degree of constriction, with chronic thickening, but admitting a No. 6 catheter. The other organs appeared to be healthy.

APOTHECARIES' HALL.

NAMES of gentlemen who passed their examination in the science and practice of medicine, and received certificates to practise, on Thursday, 18th July, 1850:—James Edward Tuxford, Boston, Lincolnshire—Joseph Bray Gilbertson, Preston, Lancashire.

FELLOWS' PRIZE REPORTS OF

CASES OCCURRING IN UNIVERSITY COLLEGE
HOSPITAL,

Summer Session 1845.

By C. H. F. ROUTH, M.D. Lond.

CHARLES LEE, æt. 30; admitted Tuesday,
June 24, 1845, under Dr. Williams.

CASE.—*Tubæ Mesenterica*—*Tubercle of
Lungs*—*Gastralgia*—*Perforation of the
Intestines*.

(The particulars obtained from the pa-
tient himself.)

Of moderate stature, spare conformation,
scrofulous temperament, fair complexion,
blue eyes, and red hair. For the last
twelve months he has been engaged as a
painter; for the previous sixteen years as
a confectioner. Had continued at work
up to six weeks since, when he failed in
business.

He has resided for the last three weeks
at No. 14, Duke Street, a very damp and
close house, especially so on the ground
floor, where he lived. In London all his
life. Formerly his food was usually good
of its kind; meat every day; seldom
greens. He very rarely took any supper;
when he did, it was only bread and cheese.
His habits are also temperate; he generally
takes Oj. of porter daily; very rarely spi-
rits. Has not been intoxicated for the last
eighteen months. Lately he has been in
the habit of drinking largely of tea and
coffee, and that not of the best kind. Has
not eaten any solids for the last fortnight;
has been very hard-worked; sleeps gene-
rally eight hours out of the twenty-four.
Disposition very cheerful.

Hereditary predisposition.—Father died,
æt. 50, from bursting a bloodvessel. His
mother died of paralysis. Has one brother,
who had colic eight years ago, but is
otherwise healthy. He is not aware that
any of his family have disease of the sto-
mach, are scrofulous, or consumptive.

Habitual state of health.—Has always
been generally healthy and strong, though
thin. Had small-pox when a child. Six
years ago he had an attack of dyspepsia,
accompanied with severe abdominal pain
and headache; he was also very sick, but
did not vomit. He was told it was dyspep-
sia, but there was no pain at the stomach
with it. He fell out of a window when
17 years old, upon his face, and it was said
he had injured his chest at the time; he
was, however, only laid up for a fortnight
after the injury. He had rheumatic oph-
thalmia nine months ago, but it was unac-

companied with other rheumatic symptoms
about the body. Has not been liable to
coughs; never had hæmoptysis.

Present attack.—In addition to night
sweats, which have annoyed him for the
last three months, he has felt much uneasi-
ness at the stomach, and a sensation as if
it was filled with water and going to burst;
but no actual pain. For the last six weeks
especially, all food taken internally has felt
heavy upon his stomach; but there has
been no nausea or vomiting with it, head-
ache, giddiness, or a bad taste in the
mouth. His bowels were also obstinately
costive, and there was occasionally severe
spasmodic pain, as if he had colic. These
symptoms would usually last about an
hour, and be relieved by a cup of warm tea
or cold porter. This sensation of fullness
or pain was only induced by the ingestion
of solids, not liquids, which produced no
inconvenience. He continued much in the
same state up to a fortnight since, when
fluids also began to affect him, and there
was pain in addition, generally more or less
intense, which even when his stomach was
empty would be produced by walking,
with this difference, that it would then
partake more of the character of soreness
than of actual spasm. Six days ago the
pain became so much worse, that he was
obliged to keep his bed, and avoid all exer-
cise. There was also much dyspnoea, with
a considerable amount of tympanitis, but
no palpitation at the heart. He was under
the care of a medical gentleman, who gave
him some medicines which opened his
bowels freely, but nothing more. He had
always been costive, frequently going a
week together without a motion. On this
occasion his bowels had not been opened
before for three or four days, and what
came away in consequence of the purgative
was of a very dark almost pitchy colour,
and very offensive. How long his motions
had been of this nature before, he had not
observed. He has lost a good deal of flesh
during the last three weeks. He was able
to walk to the hospital, though he suffered
much pain and dyspnoea in so doing.

Present state.—The colour of the skin
generally over the trunk is sallow; but
there is no eruption or increased cutaneous
sensibility. He is much emaciated. He
states he prefers a lateral decubitus, as
then his mouth remains moist! No rigors;
skin cool and moist. He is so weak he
can hardly stand, and walks with the
greatest difficulty. His feet do not swell
at night. Feels very languid and restless;
perspires a great deal during his sleep;
expression of countenance anxious; cheeks
very sallow, hollow, and sunken in; malar
bones very prominent; lips pale; eyesight
not affected; pupils naturally dilated;

small and hearing natural; there is no headache. He had slight occasional giddiness prior to his admission, but not now. Sleeps very badly on account of restlessness. Intellect clear.

Thoracic organs.—The chest measures 31 inches round on a line with the nipple; both halves 15½ inches; respiratory movements equally costal and diaphragmatic; ribs very prominent; respirations 20 in a minute. The vocal vibrations somewhat stronger on the right than on the left side beneath the clavicles; both sides resonant; if any thing the right is duller than the left, and the pitch on percussion higher. The expiration is *too loud* under both clavicles, especially on the right side. Behind, this difference is much more marked over the right supra-spinous fossa, where the respiration generally is weaker, but the expiration is louder, than that on the left side. There is a slight hacking cough in the morning, with some trifling mucous expectoration. The voice is decidedly hoarse. No palpitation or pain at the heart; sounds healthy. There is a slight regurgitation in the veins of the neck. Pulse 88, small and weak. There is no spinal tenderness or pain.

Abdominal organs.—There is slight tenderness over the region of the stomach, especially at the epigastrium, and to the right side; here, also, he feels pain. There is also, generally, a sensation of pain, and a feeling of weight in the stomach when he takes food, or *warm* drinks. The pain, but not the sensation of fullness, is relieved by cold drinks. This pain after eating lasts sometimes an hour, sometimes less. It does not come on immediately, but after ten minutes or so. When he drinks there is nausea, but no actual vomiting. This pain is increased by walking. The abdomen is rather prominent, irregular, and resisting; but no distinct tumor can be felt. Liver reaches as high as fifth rib, and projects three inches below the margin of the ribs; it is also somewhat tender on pressure. The abdomen is tympanitic in all its parts, but not tender; passes a good deal of flatus by the bowels; no motion to-day; urine free, but very high coloured.

The tongue is furred, dry, lobulated at the apex, and redder than natural; mouth generally dry and clammy. No appetite; much thirst. The teeth generally decayed, with a distinct white line around the margin of the gums. Many of the teeth are also loose.

Supposed exciting cause.—He imagines the first attack was brought on by an excessive indulgence in confectionery, eaten at all hours of the day. At this time he was unable to eat his ordinary meals. When a painter he lived very well, but lat-

terly but very indifferently,—his brother's wife being a bad manager, and serving liver, kidneys, and sheep's fry, which he did not like; so that he lived almost exclusively on vegetables.

Treatment.—Low diet. *R.* Hydrarg. Chlorid. gr. iv.; *Ext.* Conii, gr. vj. fiat pil. ij. hac nocte sumend. *Cras* mane sumat. *Ol.* Ricini, *ʒss.* *R.* Acid. Hydrocyan. Dil. *℥v.*; Soda; Sesquicarb. gr. x.; Aqua Menth., Mist. Camph. aa. *ʒss.* ter die s.

June 26th.—The bowels not having been relieved by the pills and oil, these were again repeated. To-day they have been open twice. There is *no pain* whatever after eating, though the epigastrium is still tender on pressure, and he feels occasionally sick; but there is no actual vomiting.—*Pt.* Mist. Rep. *Ol.* Ricini, *ʒj.*

27th.—His bowels were moved only once by the castor oil. He continues much in the same state, only that he experiences no difference on taking warm or cold drinks. There is no pain at the stomach.

28th.—He is not quite so well to-day; the stomach feels very full, as if he had drank a great quantity of water. There is some tenderness at the epigastrium; no sickness, but a bad taste in his mouth after eating; occasionally heartburn. To-day he seems to find that warm drinks again hurt him. Abdomen continues tympanitic, but nowhere tender, except over the hepatic region. Pulse 96, full. Tongue still dry, and very red at the apex. Bowels not open since yesterday. Sweated a good deal last night, and feels very weak, especially across the loins; was unable to sit up to take his breakfast by reason of this feeling of weakness.—*R.* statim sumat. *Ol.* Ricini, *ʒss.*, et repet. omni mane. Middle diet.

July 1st.—He has continued to sweat profusely every night since last report: last night, however, he thinks it was less than before. Feels very weak. He can only walk across the ward in a tottering manner. His feet, however, do not swell at night. The face looks perhaps fatter than it did when he came in, and has lost the hectic flush. There is no headache; but he complains of a good deal of dimness of sight, so that after reading ten or twelve lines in a book, his sight becomes so dimmed, he is obliged to put the book down. This symptom he never before noticed. Slept better last night; and to-day he feels rather drowsy. The hacking cough is rather less frequent than it was: no expectoration; pulse 96. The blue line at the margin of the gums still persists: teeth generally black and loose. No pain whatever over his stomach, nor tenderness, even on hard pressure. No feeling of weight or pain after eating; no nausea or bad taste in the

mouth. His appetite is better; and he very much relished a chop ordered for him yesterday. Bowels open: stools light-coloured. Abdomen still resonant and tympanitic.

3rd.—To-day he has no pain at the stomach. Last night, however, between 11 and 3, he suffered with a dull pain at the epigastrium, which finally disappeared on the expulsion of flatus. After his dinner to-day he felt again the sensation of fullness, though he ate but little, and less potatoes than usual: it did not last longer than half an hour. Tongue is clean.—*R. Fiat haust. c. Infus. Calumbæ loco aquæ.*

4th, 2 P.M.—When he took his tea last night, he had a return of the gastric pain, which lasted about two hours. The castor oil this morning began to affect him. After taking it he felt very sick and cold, and became covered with cold sweats: the gastric pain also recurred. These symptoms passed off in half an hour. This morning he was very hungry; but the food he took brought on the same kind of pain he was suffering from before his admission. He ate only half a chop this afternoon, and the same pain recurred, and has continued ever since. Bowels open.

5th.—There are no general pains, no anasarca, no headache or giddiness. Sleeps very indifferently. Feels very cold, sick, and faint, and is covered with cold sweats, after taking the castor oil; the pain at the stomach is also reproduced. The other medicines he takes do not affect him in the same manner. The abdomen is still large and tympanitic, more particularly in some parts; it seems too full, though no actual tumor can be detected. *Vocal fremitus is felt transmitted through the parietes of the abdomen.* There is some tenderness on pressure in both iliac fossæ, and slight fluctuation in the right iliac fossa; but it is so circumscribed and confined, and not moving on pressure, that most likely it is contained in the intestine. The patient being turned on his belly, there was some superficial tenderness over the loins. It was not tympanitic on the right side, and parts dull before did not alter on making him change his position a little. The most inferior and depending side was also quite tympanitic, whatever side he was laid upon. The abdominal tenderness seems to be more superficial than deep-seated. The kidneys and mesenteric glands cannot be felt from pressure in front. There is a sensation of fullness that now comes on after every meal immediately, and persists for two hours. It is, however, scarcely as severe as it has been.—*R. Fiat. haust. ut antea c. aqua loco Calumbæ; Omitte OL. R Ext. Colo-*

cynth Co.; Ext. Conii, aa. gr. v. fiat pil. o. n. s.

7th.—The pain in the stomach yesterday was very severe, aching continually, with little intermission. The part is tender and sore on pressure. To-day he is much the same. Perspired a good deal last night, but could not sleep. No headache, palpitation, or sickness. He has not been able to eat his chop the last two days, but he gets it made into broth, which he finds agrees better with him. To-day, after his breakfast, the gastric pain recurred rather sharp in character. The same thing occurred after his broth, but it lasted only fifteen or twenty minutes. Tongue is again morbidly red and dry. Pulse 104, very weak.

8th.—Chest expands well. On percussion, the right side continues to be duller than the left. The right end of the sternum appears to protrude more than left. The breath sound, as also the voice sound, is tubular beneath the right clavicle: much more natural under the left. Behind there is dulness in both interscapular spaces, and some pain on percussion over the left side. The right supra-spinous fossa is decidedly duller than the left, and the breath and voice sounds strongly tubular. It is slightly so in both interscapular fossæ, but especially the right. The tongue continues morbidly red, deprived of its epithelium, dry in front, with a slight fur posteriorly. The pain in the stomach is no better. Bowels open.—*Auge Acid. Hydrocyan. dil. ad ℥vjij.; Empl. Lyttæ epigastrio.*

10th.—He is rather better to-day. The blister rose well: and to-day the pain in the stomach is relieved, and no longer induced by taking food. The sensation, also, of faintness has disappeared. He does not sweat so much at night as he did, but he sleeps little, owing to the blister, which externally is very sore.—*R. Auge Acid. Hydrocyan. dil. ad ℥ix. sing. dos.; omne nocte habeat Ext. Hyoscyami, gr. v.*

11th.—He again perspired a good deal last night, and feels very weak in consequence to-day. His countenance is remarkably free from any appearance of anxiety, but looks very thin. Pupils much contracted. No headache or giddiness. The tongue is still morbidly red. The blue tint on the margin of the gums persists, but is less evident. Cough trifling, no expectoration. There is no pain, feeling of weight, or slightest uneasiness, in his stomach after eating. He still takes broth instead of the chop. Pulse 104, rather fuller than it was. The bowels were open four times last night, and once this morning, and the morning motion, which he saw, had the appearance of pitch.

12th.—To-day there are a good many white pellicles, aphthous-looking spots, on the roof of his mouth and tongue. The epithelium is wanting in some parts, which have, therefore, the appearance of being excavated. There are, also, a few follicles on the gums. His mouth is occasionally very dry, and he feels very thirsty. There is no abdominal or gastric pain after eating. Takes chiefly fluids. Bowels open three times last night. The physical signs of the lungs continue much the same. On the right side the respiration is tubular: on the left side the expiration is too loud, but there is a good deal of vesicular murmur below. He cannot sleep at night. —R Auge in Mist. Sodæ Sesquicarb. ad gr. xv.; Omni nocte sumat. Morphine Hydroch. gr. $\frac{1}{4}$ loco Hyocyami.

14th.—This morning when he got up his mouth was so sticky he could not articulate; but after drinking a little he was enabled to do so. He has felt very sick, but has not vomited. The face looks very thin. Cheeks hollow and sunken. The tongue continues morbidly red, here and there deprived of its epithelium, with a few aphthæ still. There are, however, none on the roof of the mouth. The gastric symptoms are equally improved. Bowels open to-day, not yesterday, but he has not noticed the colour of his motions. 3iv. of urine saved. Tested with nitric acid it gave at first a deep blue colour, passing into a red, and finally into a dirty brown colour. Hydrochloric acid produced no effect on the heated urine. Acid; sp. gr. 1027. R. Fiat. Haust. c. Decoct. Cinchona, 3j. loco aquæ.

15th.—He was rather delirious last night, which he attributes to the morphia. He is looking very thin. The expression of his countenance, however, is quite free from anxiety; indeed, rather expressive of great indifference. Abdomen generally tympanitic; but in the most depending points there is also slight dulness on superficial percussion, and slight fluctuation detectable. Bowels open.—R. Omitt. pil.

16th, 10 A.M.—The patient looks very pale and thin: his face covered with cold perspirations. Extremities cold. There is considerable tenderness on pressure over the abdomen. Last night, towards morning, he began to suffer much pain in the abdomen, and he had what he calls an immense motion in his bed, which relieved the pain. About six or seven o'clock, feeling again an inclination to go to stool, he got up, but while in the act of walking to the closet he suddenly felt very faint, and fell to the ground, by reason of passing a large quantity of blood per anum, which was sprinkled on the floor of the ward. The nurse says that what he passed in the

bed was pure blood, but that on the floor was blood mixed with fecal matters. Altogether she imagines he lost about Oiv. of blood. It was of a dark colour, somewhat pitchy-looking, but here and there with clots of more florid coloured blood. It was very offensive, and resembled in odour carcinomatous discharges. He is very thirsty. No appetite. He has eaten only a piece of cake, and taken a little brandy and water, which was given to him at the time. Neither gave him pain, or brought on the sensation of fullness at the stomach. Pulse 130, very weak and tremulous.—R. Omitt. Haust. R. Plumbi. Acet. gr. ij.: Pulvis Opii, gr. $\frac{1}{2}$; Ext. Humuli, gr. iij. fiat pil. 6tis horis sumend. Wine, 3vj.

3 P.M.—His extremities are a little warmer, and he feels stronger than he did this morning. Has had no motion since. Pulse rather fuller, 124. Skin moist. In other respects he feels much about the same as this morning.

17th, 10 A.M.—His face looks extremely pale this morning, and bedewed with cold perspirations. Cheeks more hollow and sunken than before. The countenance still expressive of great indifference, as before remarked, but he says he feels stronger. No headache, giddiness, or tinnitus aurium. His tongue morbidly red and dry, so that he articulates with difficulty. Pulse 128, rather fuller. Yesterday evening, between four and five, he had another motion, consisting almost entirely of blood. It was not so pitchy as before, and consisted more of clots. At seven he had another motion of the same kind, but it contained some fecal matter also—both equally offensive. He was very faint at the time, but recovered under the influence of wine. He is very thirsty, and takes his wine with avidity, and which he appears to swallow with greater ease than yesterday. No perspirations last night. Slept well. The nurse supposes he lost about O $\frac{1}{4}$ of blood.

3 P.M.—He says he feels very faint, but his extremities are warm. Face looks paler, more sunken, and generally thinner than it did this morning. His head is rather hot. Pulse 124, very weak and compressible. There is no tenderness of the stomach, but a little in the right iliac region. On pressing deeply over the pylorus the hand comes against some hard substance much nearer than the spine. It was not the liver, for although this extended one inch below the margin of the ribs the tumor was much deeper than it. Over the right iliac fossa there is a slight layer of fluid to be detected on pressure, communicating to the hand a distinct feeling of irregularity and gurgling. Has not had his bowels open since yesterday at seven o'clock. There is considerable hic-

cough at present, with some abdominal griping occasionally. The sensation of weight returned to-day after he took his tea. Has eaten nothing the last two days but a small piece of cake. He does not fancy his chop, but seems to think he would like fish. Respiration easy.—*R. Auge Plumb. Acetatis*, ad gr. iij. s. d.; *Cras mane habent Magnes. Sulph. ʒij.*; *Acid. Sulph. dil. ʒjxx.*; *Aque Menth. pip. ʒiss.* To have fish instead of the chop daily.

18th.—By mistake the medicine was given last night instead of this morning: it produced great pain for two hours, which was then relieved by a copious motion, full of scybala, but containing no blood. Afterwards they were again opened. The abdomen is not so full to-day as it was, but still much too much so to the feel. Aorta can be felt pulsating in it. Slight tenderness in the iliac fossa, as over the situation of the right kidney. No cough, but the breathing is a little quicker than natural. Pulse remains quick, 120, very jerking and compressible. Tongue very dry, so as both to interfere with deglutition and articulation. No increase of the aphthæ about the mouth. No pain in the stomach, headache, or giddiness.

19th.—Perspired a good deal last night, and felt this morning very weak and restless. The countenance is still remarkable for the expression of complete indifference it exhibits. He is very thin and pale. The organs of the senses do not appear to be at all affected. No headache or giddiness. He is slow in giving utterance to his ideas, but in other respects perfectly rational. Mouth continues very sticky and dry. There is a slight hacking cough, rather more frequent than it was before, but without expectoration. Sleeps very badly. Skin hot and dry. Extremities warm. He has had a good deal of pain and spasm in the stomach to-day, which is, however, not tender on pressure. No pain in iliac fossa to-day. No appetite, and has not been able to eat any thing to-day. Bowels not open to-day. Last night they were so once: the stools slightly tinged with blood, and very offensive. Urine, sp. gr. 1027. Nitric acid producing the same black and brown colour when a few drops are added, but which is taken up by the addition of a little more.

21st.—Yesterday he was very ill, and much purged, and whatever liquids he took brought on a motion. The first motion passed was somewhat bloody, containing a number of small clots about the size of small peas floating in it. The remainder were fecal, but all excessively offensive, the odour bearing great analogy to that of cancerous discharges. He did not faint,

but was very nearly doing so once or twice, but recovered after taking some wine. He was ordered—*Tinct. Opii, ʒiv.*; *Mist. Camph. ʒj. 2da q. q. hora.* He took one dose of this, and felt greatly relieved by it.

To-day he says he feels much stronger than yesterday. There are no sweats, but he does not feel cold. Skin rather hot and dry. Extremities warm. The expression of his countenance continues the same. It is very pale and thin. His whole body is very much emaciated, so that the form of the skeleton can be traced through the skin everywhere. Eyes and jaws are prominent. No dimness of sight, but all the day there has been a good deal of tinnitus aurium. His smell is unaffected, but his taste quite gone. His mouth continues very dry, and his articulation is still very unintelligible. No headache. Slight giddiness. No dreaming, wakefulness, or throbbing in the head. He slept pretty well last night. The whole body, especially the hands, seem to be affected with nervous tremors. No cough or pain in the chest whatever. Pulse 120, weaker than before. Tongue still dry and red, but no more aphthæ. Some difficulty in deglutition. Abdomen is certainly less prominent to-day, but still too full. Vocal fremitus also felt in it. Some tenderness in the left hypochondrium on deep pressure, but more in the right iliac fossa. No appetite, but a good deal of thirst. Yesterday he had rather more appetite, and was enabled to eat half a sole for his dinner. To-day he has eaten one or two cheese-cakes. They did not bring on the sensation of pain or fulness as before. Bowels not open. No pain over liver.

From this time the patient continued to become weaker and weaker. About 10 o'clock p.m. he seemed to be in excessive pain until about 2 o'clock, and groaned a good deal. He died at 3 p.m.

[To be continued.]

ROYAL COLLEGE OF SURGEONS.

GENTLEMEN admitted members on the 19th inst.:—Messrs. F. C. Ronalds—W. F. Dixie—S. Morris—N. H. Stevens—G. F. Lane—W. Derbyshire—S. S. Roden—A. Johnston—H. R. Stevenson—J. S. Wills.

Admitted on the 23d inst.:—Messrs. G. Salter—R. C. Perkin—J. T. Williams—G. W. Bridgeman—J. W. Keyworth—W. H. Adley—T. E. Evershed—W. S. Steele—H. J. Stormont—H. S. Gaye—R. T. Buckle—W. T. Juxton—W. Dingley.

Correspondence.

COMPARATIVE LIBERALITY OF BRITISH MEDICAL COLLEGES.

SIR,—In one of the leading articles of your journal of to-day, you remark, in speaking of the liberality and illiberality of the various British medical colleges, that “the London Colleges have taken the initiative, in liberally throwing open their examinations to the possessors of British licenses or diplomas.”

This statement however, with all due deference to you, I beg to tell you is not strictly correct, as will appear from the following:—

A short time ago, before the passing of the new charter of the Glasgow Faculty, of which I am a Licentiate, I was desirous of passing the examination of the London Royal College of Surgeons, and for this purpose wrote to Mr. Belfour, stating my wishes, and asking him whether, by presenting my Faculty License, I could be admitted for examination at the above-named College. With that polite attention which Mr. Belfour always evinces when written to, he at once replied to my question by note, informing me “that I was not entitled to offer myself as a candidate for the diploma under the circumstances mentioned,—the Faculty of Glasgow not being a College of Surgeons.” I was thus at once unexpectedly checked in my hopes of obtaining the same liberality from the London College as that which I knew they allowed to Licentiates of the Royal College of Surgeons of Edinburgh.

It is true indeed, as Mr. Belfour says, that the Glasgow Faculty is not verbally a College of Surgeons, and I know that the laws of the London College rule, that the diploma, license, or degree, presented by a surgeon already qualified, must be from one of the legally constituted Colleges of the United Kingdom, and hence I cannot complain of any fault being made in carrying out the exact wording of the law; but I can complain, and that right justly, of the spirit of illiberality and partiality shown by the College in refusing to admit for examination the Licentiate of an examining body, whose curriculum and test of knowledge is as stringent as that of any other, simply because the term Royal College does not take the place of Faculty. Still more I can accuse the College of dealing unfairly in a distinction of privilege which they make between the Licentiates and Fellows of the Glasgow Faculty itself, since they accept the certificates of Hospital Surgeons or

Lecturers, who are Members or Fellows of the Faculty, whereas their laws clearly state that no certificates will be recognised unless the surgeons or teachers be members of some legally constituted College of Surgeons in the United Kingdom. Now why, I ask, do they not follow out the strict letter of their law in this instance also, and say to all the teachers who are only Members or Fellows of the Glasgow Faculty, we cannot receive the certificates signed by you Fellows, your body not being a College of Surgeons? But this is never done, under any circumstances, and we must therefore suppose that the London College looks upon the Faculty as a College in the person of a Fellow, but as a Faculty, i. e. no College, in that of a Licentiate.

I would not have ventured so far to trespass on your valuable space, but that I was anxious to correct your notion as to the extreme liberality of one of the London Colleges in the matter of examinations.

With a feeling of pride in having the honour to belong to that body of medical men, who, though of the oldest standing, are the first to cast away ancient and ridiculous privileges, and to set the excellent example of sacrificing individual rights for the general good of the profession,

I have the honour, sir, to remain,
Your very obedient servant,
B. W. RICHARDSON,
L.F.P. & S. Glas.

Mortlake, Surrey,
July 13th, 1830.

COFFEE A REMEDY FOR HOOPING-COUGH.

DR. JULES GUYOT considers the seat of hooping-cough to be exclusively the digestive organs, particularly the stomach and pharynx; the cough and convulsive movements of the larynx being caused by pruritic irritation of the pharynx, sympathetic of the disorder of the digestive organs. The same physician has found the administration of one or more spoonfuls of infusion of coffee, according to the patient's age, after each meal, sufficient to cure the most obstinate cases. This property of coffee was discovered accidentally by Dr. Guyot while attending a very severe case of the disease.—*L'Union Médicale*.

. It may be observed that Dr. Guyot gives no facts in support of his “pruritic theory” of hooping-cough. If his views be correct, it is at least remarkable, that so closely as pathological anatomy has searched for a cause of hooping-cough, it should have overlooked what lay at the very entrance of the air-passages.

Medical Intelligence

THE GENERAL PRACTITIONERS' INCORPORATION BILL.

ON Tuesday last Mr. Wyld moved the House of Commons for leave to bring in a bill to incorporate the general practitioners of surgery, medicine, and midwifery, and said, it would not be necessary for him to detain the house more than a few moments, as he was happy to state that he had obtained the assent of the Home Secretary to the introduction of his bill. The object of his motion was to incorporate the general medical practitioners, and to give them the power to institute examinations into the qualifications of those who wished to pursue the general practice of the medical profession. He merely proposed to lay the bill upon the table, as he did not intend to take any action upon it during the present session of Parliament.

Colonel Thompson seconded the motion, which was agreed to; and leave was given to bring in the bill.

THE CHOLERA AT MALTA.

LETTERS from Malta of the 12th instant announce that the cholera was rapidly declining. Quarantine had been established at Naples of 21 days, at Leghorn 7 days, and Corfu 10 days, on all arrivals from Malta, according to the last accounts. On the 10th instant, the Governor of Malta, as President of the Council, gave the following report on the attacks and deaths from the 9th of June to the 10th of July:—Of the population, 122 attacks and 89 deaths; in the fleet, 32 attacks and 18 deaths; in the garrison, 19 attacks and 19 deaths. Total cases, 173; total deaths, 126.

PROFESSORSHIP OF SURGERY, UNIVERSITY OF GLASGOW.

WE understand that Dr. James Adair Lawrie has been appointed to the Professorship of Surgery in the University, vacant by the demise of the late lamented Dr. Burns. Dr. Lawrie has for some time occupied a similar office in the Andersonian University. The appointment is in the gift of the Crown.

EPIDEMIOLOGICAL SOCIETY.

A GENERAL meeting of this Society will be held at the Hanover Square Rooms, on Tuesday next, the 30th of July, at 8 P.M., Lord Ashley in the chair. At this meeting the following gentlemen will be nominated:—*President*, Benjamin Guy Babington, Esq. M.D. F.R.S. &c.; *Vice-Presidents*,

Addison, Thomas, Esq. M.D.; Brodie, Sir B. C. Bart. F.R.S.; Burnett, Sir William, Knt. K.C.H.; Bright, Richard, Esq. M.D.; Clarke, Sir C. M., Bart. M.D. F.R.S.; Dale, Rev. Thomas, M.A., Canon Res. of St. Paul's; Grainger, R. D. Esq. F.R.S.; Haviland, J. Esq. M.D., Reg. Prof. of Physic, Cambridge; Hastings, Sir Charles, Knt. M.D., Worcester; McGregor, Sir James, Bart. K.C.T.S.; Nussey, John, Esq. L.S.A.; Probert, John, Esq. M.R.C.S.; Roupell, G. L. Esq. M.D.; Smith, Thomas Southwood, Esq. M.D.; Sykes, Col. F.R.S.; Watson, Thomas, Esq. M.D.; Bryson, A. M.D. R.N.; Busk, G. Esq. Surgeon, Dreadnought; Bird, James, M.D.; Bell, Jacob, Esq.; Bird, Golding, M.D.; Carpenter, W. B.; Gull, W. W. M.D.; Greenhalgh, R. Esq.; Hisham, A. M.D.; Hunt, T. Esq.; Headland, E. Esq.; Jenner, W. M.D.; Latham, R. Gordon, M.D. F.R.S.; Leeson, H. B. M.D. Greenwich; M'Williams, J. O. M.D.; Marson, J. Esq. Res. Surg. Small-Pox Hospital; Parker, E. Esq., M.D.; Percival, W. Esq. M.R.C.S. Vet. Surg. 1st Life Guards; Snow, J. M.D.; Sibson, T. M.D.; Simon, J. Esq.; Sieveking, F. M.D.; Seaton, E. C. M.D.; Walsh, C. R. Esq.

OBITUARY.

ON the 16th inst., at his house at Hackney, John Mordaunt, Esq., surgeon.

Selections from Journals.

ON THE MORBID APPEARANCES IN ARSENICAL POISONING, ETC.; WITH MEDICO-LEGAL OBSERVATIONS. BY T. G. GEOGHEGAN, M.D., FELL. AND PROF. FOR. MED., ROY. COLL. OF SURG., IRELAND.

[THE following communication has recently appeared in the Dublin Medical Press, and a copy of it has been forwarded to us by the author. The paper may be regarded as a very valuable contribution to Pathology.]

The increase of the crime of poisoning of late years has become not alone a matter of serious social import, but has tended to invest toxicological inquiries with an additional interest to the physician, both in his public and curative relations. Of the various substances resorted to for criminal purposes, the arsenical compounds rank undoubtedly foremost, both as to the greater frequency of their employment, and as relates to the delicacy, variety, and gravity of the questions to which they give rise in medico-legal practice.

The great importance of the study of arse-

nical poisoning clearly appears from statistical evidence collected in England and France.

Thus of 543 cases of poisoning which formed the subjects of coroners' inquisitions in England and Wales during two years, 186 were of this nature.

Of 285 cases which occurred in France in fourteen years, 199 were arsenical.

Of 97 poisonings in Ireland which have been brought under my notice, and in which the special substance administered was determined, no less than 72 were arsenical, or three-quarters of the whole.

A case of this nature having been lately submitted to me, I propose, in the first instance, exhibiting the morbid parts to the Society,* and afterwards to offer some general observations on the appearances which have presented themselves in other instances which I have investigated, together with some additional remarks on certain other points in the history of arsenical poisoning. The specimen laid upon the table is the stomach of a female past the middle age, who died from the effects of arsenic under the usual symptoms of gastro-intestinal irritation, after an illness of sixteen hours. The organ having been transmitted to me with a view to the detection of the poison, I observed the following appearances a week after death, at which period the body was exhumed, and found free from any trace of putrefaction.

The peritoneal coat presented a diffused, red, and towards the splenic end a brownish tint. There was a marked contraction of the muscular coat about three inches from the pylorus. An uniform reddish fluid (twelve fluid ounces), of the consistence of thin gruel, and depositing brown flocculi, formed the contents, which were of an acid reaction, indicated freely the presence of arsenious acid in solution, and included a considerable quantity of insoluble hæmatosine. The mucous membrane was found at the splenic end of a mottled reddish brown colour, covering a stratum of coarse ramiform vascularity (in the submucous tissue), soft, and presenting a few petechial ecchymoses, and one or two erosions (of the area of a fourpence), with an undefined border, which exposed the unaltered submucous coat. The body of the organ (as far as the contraction) exhibited a remarkable arrangement of dark blackish purple streaks of extravasated blood, deposited in the substance of a softened mucous membrane, and capable of ready removal by the nail, which laid bare the sound submucous texture. These streaks (which affected for the most part

the direction of the long axis) were of trivial thickness, scarcely elevated, flattened on the surface, and made up of a close aggregation of irregular blotches. Amongst these lines there were interspersed a few of the lighter-coloured *petechial* (fluid) ecchymoses, observed at the splenic end. The remainder of the mucous membrane was of a brown red, which abruptly ceased at the contraction, between which, and the pylorus, the membrane where untinged by bile was grey, and coated with viscid opaque mucus of the same colour. The mucous membrane generally was softened, not capable of being peeled off at the splenic end, but yielding on traction in the body and towards the pylorus, flaps of one quarter and half an inch respectively. The submucous coat, except at the great tuberosity, seemed free from injection. There was no trace of true ulceration in any part of the organ. The mucous membrane of the *duodenum* was reddish, and apparently a little thickened, and presented an oval ulcer of the size of sixpence, with a thick elevated edge, its surface exposing the submucous, and in the centre the muscular coat. As the patient had previously complained of pain in the region of the stomach, it is probable from this (and other considerations) that the above ulcer was the result of antecedent disease.* The softening of the mucous membrane in the present case seems referable to the prolonged action of the contents during a week's inhumation, and not to the influence of the poison, which, in my experience, has not been observed to produce that effect. The heart and lungs were reported in the present case to have been natural. The stomach at the end of four weeks was scarcely altered, as I shall further explain when alluding to the antiseptic influence of arsenic. I shall now proceed to the general consideration of the morbid conditions which obtain in arsenical poisoning, confining myself on the present occasion to the stomach, both as being the organ which presents the most striking and varied appearances, and being also that which I have enjoyed the most extended opportunities of examining.†

* I examined not long since the duodenum of a gentleman between twenty and thirty years of age, who died of perforating ulcer of that part under circumstances incompatible with the supposition of poisoning.

† It is much to be regretted that the post-mortem inspection in cases of poisoning is too generally confined by practitioners to the abdominal cavity, and in some cases to the stomach alone, and thus that much matter of great interest and importance in medico-legal history is lost. The appearances which I have had occasion to observe in the heart, lungs, and blood, have been in some cases remarkable. In the first, endocardial (petechial) ecchymoses in both ventricles; in the lungs, intense congestion, approaching in appearance to pulmonary apoplexy

* Read before the Surgical Society of Ireland, Jan. 26, 1850.

The morbid alterations produced in the stomach by the action of arsenious acid, arising as they do either from its local influence as an irritant, or indirectly from its absorption, and being independent of any corrosive agency,* cannot of course be considered distinctive. From this circumstance, however, they are well fitted to convey to the inexperienced examiner a correct idea of the appearances to be expected in irritant poisoning at large. To these common signs there are occasionally superadded some of a peculiar nature, arising out of the physical properties of arsenic, to be afterwards described. The following results have been arrived at from the examination of sixteen fatal cases:—

1st. All (with one exception, in which maceration had altered the parts,) exhibited signs of irritation in the form either of—*a*, vascular injection, *b*, ecchymoses, or *c*, colouration. The mucous membrane was engaged in fifteen; the submucous coats also, in five; the peritoneal in two; and the venous arrangement of the great extremity in one case.

The ramiform vascularity I have only found in the submucous coat, which seems also not subject to any of the other varieties of injection. The punctiform, or closely stellate, is by far the most common variety, occupying extensive tracts of the mucous surface, and either uniformly distributed, or disposed in sinuous lines or scattered patches. The striated vascularity I have met with but in one instance.

2nd. Diffuse redness, although more frequently present, according to my experience, than any other deviation from the natural state, I have not enumerated under the head of vascular injection, since, without denying its occasional morbid character, I feel disposed to view it as a pseudo-morbid change resulting from the influence of transudation and imbibition on the punctuated vascularity. Accordingly, I have

and general bronchitis, which latter I have more than once found indicated by physical signs during life. The condition of the blood I have found variable, sometimes coagulated in the heart, and fluid in the great veins (as happens in various other forms of death), and occasionally viscid in the pulmonary vessels. In one instance, examined ten hours after death, the fluid blood removed from the venous system coagulated in the receiving vessel, affording a milky serum.

* In the common acceptance of the term "corrosion," which is understood to imply such chemical action as issues in alteration of the colour or consistence of the part: it is not, however, to be inferred from the absence of the latter results, that no chemical influences are mutually exercised by the poison and the mucous membrane: such, in the case of arsenious acid, are most probably in operation, and for aught that can be affirmed to the contrary, may be the cause of its irritant action on mucous, and its sphaculating effect on ulcerated surfaces.

observed the latter to degenerate into diffuse redness by the influence of time and exposure to the action of the stomachic contents. The diffuse redness may exist alone or in combination with other results of irritation.

3rd. Ecchymosis as a consequence of arsenical poisoning has presented itself to my observation under the form of—*a*, well-defined blotches, oval, angular, or circular, and generally small; or *b*, of lengthy streaks, parallel or areolated. Both are formed of a thin stratum of altered, nearly black, and apparently coagulated blood, deposited in the tissue of the mucous membrane. They are scarcely elevated, and (the mucous membrane at the point occupied being much softened) are readily removed by gentle scraping, leaving behind an *erosion* of corresponding figure. A variety of ecchymosis which I venture to designate the *petechial* (seen in five cases) is essentially different from the foregoing. It consists of invariably small and rather florid blotches of fluid blood, thinly scattered for the most part on the summits of the rugae, and unaccompanied by softening of the membrane. This condition might be confounded by the inexperienced observer with the punctuated injection, from which, however, it is readily distinguished by the lens, which reveals the vessels of the latter.

The ecchymoses just described do not appear to me to result, as might be supposed, from the lodgment of small masses or particles of the poison on the mucous surface, which I have not observed in any instance. On the contrary, I have seen them produced by fluid poisons, as ardent spirits, &c., which can only act by creating a violent determination of blood to the entire surface, and consequent rupture of the weaker vessels. One at least of these conditions may moreover exist under circumstances in which the poison could have only reached the affected part by absorption; as in the external application of arsenic, or where the spots involve the endocardial membrane. The only effect which I have observed as strictly traceable to the *local* action of arsenic on the mucous coat, is a fungous thickening (forming an elevated ridge or circular raised patch), with or without the effusion of lymph, and surmounted by adherent arsenic.*

It is possible that the non-occurrence of sloughing of the mucous membrane, from

* In one case (fatal in twelve hours) numerous patches of tough, coriaceous fibrine, some of them of large size, were discovered strongly adherent to the mucous surface, which, to a corresponding extent, was intensely vascular and greatly thickened. In another there were two ridges much elevated (formed solely by the lining membrane), and crested with a mixed coating of arsenious acid and mucus.

the local contact of arsenic, is due either to its high vitality, or to a partial protection of its surface by these mucous or fibrinous effusions.

4th. Erosion of the lining membrane I have met with in one-fourth of the cases examined, and under two forms. 1. The circular. 2. Long narrow sinuous streaks. Both expose the submucous coat. The former, which generally occupies the splenic end, presents a soft undefined nonelevated margin free from red colouration. The margins of the eroded streaks, on the contrary, are sharp. A careful examination of these erosions has impressed me with the belief that they are produced by the removal of the mucous membrane where occupied by the black extravasation. Accordingly, they can be produced by gently scraping the latter; and in one instance (fatal in thirty-six hours) in which I found extensive linear erosion, a portion of the eroded surface was found still coated with the black matter.

I have never encountered true ulceration of the stomach in arsenical poisoning, although I have seen it extremely well defined on the posterior part of the buccal mucous membrane (in a case fatal in four days and a half.)

I have not been able to trace *softening* of the mucous tunic to the action of arsenic, not having seen it in cases examined sufficiently early to preclude the possibility of its pseudo-morbid origin. From the great tenuity of the membrane at the splenic end, a very short contact with the contents will suffice to produce considerable softening, particularly when the latter (as often occurs) have a decidedly acid reaction. In such I have witnessed from the latter cause, in arsenical poisoning, another pseudo-morbid change; namely, extensive brownish black staining of the mucous surface of the splenic end, similar to what is occasionally seen from the action of oxalic acid. This arises from the influence of gastric acid (lactic?) on the blood contained in the highly inflamed mucous surface; and a like colour is imparted to the contents when bloody. Although softening of the mucous membrane appears not an unequivocal result of the influence of arsenic, to the latter it may be referred.

4th. *Diminished adhesion* of the mucous to the submucous coat. This is best observed at the pyloric third, where traction will often furnish a flake of an inch and a half to two inches in length, being probably three or four times the natural amount.

Having spoken of the mucous and submucous membranes, I have only to state, that in two instances I have observed diffuse red colouration, and in two, capillary injection of the peritoneal coat, without effusion of fibrine or serum.

The examination of the physical characters of the stomachic contents is a matter of much importance in medico-legal inquiries, and will be often found to elucidate, in a most unexpected manner, questions which may involve the life of an accused party. Thus, in one instance which fell under my notice, the determination of the nature of a solitary seed found on the mucous surface became of serious moment. In another, the discovery of grains of shot in the colon, furnished me the means of affording sufficient legal evidence for the identification of a body buried several weeks, in which there had been no label affixed to the coffin, and where the countenance was too much altered to be recognized. Except where the contents deposit grains of arsenic (which I have met in two cases), they do not for the most part present anything to distinguish them from those observed in other forms of non-corrosive irritant poisoning. For the more accurate information, however, of the inexperienced inquirer, I subjoin* the numerical results of the conditions observed, premising that the most usual combinations of them are, probably—1. The thick, and turbid, and bloody (insoluble hæmatisine); and 2. The copious, viscid, and bilious (brown or green). In alluding to the pseudo-morbid changes produced in the stomach at ulterior periods, the modifying influence of putrefactive gases on deposited arsenic, and the consequent formation of sulphuret of arsenic, need scarcely be alluded to, being now well known to medical jurists. A condition, however, of the mucous membrane has presented itself to my notice, which is capable of leading to mistake—namely, a copious deposit of adherent yellowish white crystals of *phosphate of magnesia and ammonia* on its surface. This occurred in a case of arsenical poisoning exhumed after forty-seven days, and was evidently a post-mortem change, as it was present also in the lining membrane of the heart.†

The peritoneal surface also of the liver, in contact with the stomach, in some instances, presents a deposit of a white granular matter (which, in a case of arsenical poisoning, I have known mistaken for arsenic supposed to have transuded). This I have found to consist of *sulphate of lime*. Its mode of production is not very apparent.

Although it is no part of my present

* In sixteen cases the contents were—in 11, bloody; 9, turbid; 6, viscid; 6, thick; 5, bilious; 4, flocculent; 3, acid; 2, rancid; 2, foetid; 1, intensely so, without putrefaction, and apparently from stercoraceous regurgitation; 1, coffee-coloured.

† Professor Apjohn has described a similar condition of the peritoneum.

purpose to enter into the subject of the detection of the poison, I shall conclude the notice of the stomachic contents by observing generally, that the mode of combination of the poison (when discovered) is also liable to alteration through the influence of circumstances which have come into operation antecedent to death. Thus I have seen one instance, in which arsenious acid, taken by a suicide, became converted into arsenite of copper by the antidotal employment of sulphate of copper followed by albumen. Arsenious acid is, however, exempt from those alterations resulting from the action of animal fluids and tissues, which sometimes create difficulty in the case of other mineral poisons.

(To be continued.)

ABSTRACT OF THE FIFTH SERIES OF PROFESSOR MATTEUCCI'S ELECTRO-PHYSIOLOGICAL RESEARCHES.

PART I.—Upon Induced Contractions.

THE experiments detailed in Signor Matteucci's former researches, proved that no signs of an electric current are manifested during the contraction of the muscles, and therefore that induced contraction cannot be referred to that agency. The following new researches were instituted with a view to the discovery of the nature of the phenomenon of induced contractions, which is so obscure and at the same time so important.

Observing that the slightest discharges from a jar, inappreciable by the most delicate of our electroscopes, are invariably sufficient to excite violent contractions in a frog, it appeared agreeable to analogy to suppose that the cause of induced contractions might reside in a discharge similar to that of the jar, taking place in the muscle in the act of contracting. If that had been the case, it would no longer have been a matter of surprise that the galvanometer should give no indication during muscular contraction.

Very slight discharges, such as are elicited after a very small jar has been discharged two or three times with a metallic arc, were passed through an insulated galvanoscopic frog, the nerve of which touched the muscle traversed by the discharge.

Thus it is shown, that in spite of the good conductivity of muscle, a part of the discharge escapes to the surface and traverses the nerve of the galvanoscopic frog.

This phenomenon is still more remarkable when the nerve of the galvanoscopic frog is laid out on a metallic surface, through which the shock is passed. The same phenomenon was manifested when a non-conducting medium, as a layer of turpentine of such depth that a current from a pile of fifteen couples of plates could not penetrate

it, was interposed between the muscular mass and the nerve of the galvanoscopic frog.

The interposition of very fine plates of mica did not prevent, though they diminished, the frequency of the contractions.

By exposing the nervous centres of animals, and placing these in contact with the nerve of the galvanoscopic frog, and exciting muscular contractions and other manifestation of the nervous force, no contractions were excited in the galvanoscopic frog: while on the other hand, contact of the galvanoscopic frogs with the muscles of the same animal when in a state of contraction, induced contraction through their nerves.

Hence it is inferred, that the phenomena of induced contraction belong exclusively to the muscle in the state of contraction. But it is impossible to determine by direct experiment whether induced contractions be due to electric discharges produced during the contraction of the muscle.

PART II.—Upon the phenomena elicited by the passage of the current through the Nerves of a living Animal, or an Animal recently killed, according to the direction of the current.

The direct electric current transmitted along the course of a nerve exhausts its excitability; the inverse current increases it.

The following experiment illustrates this part of the subject, which has been studied in all its bearings by Signor Matteucci. The frog prepared in the usual way is placed astride between two little glasses in which the rephores of a Faraday's pile of fifteen or twenty elements were immersed. It is evident that one of the limbs is traversed by a direct current, and the other by an inverse current.

In the first place, the two limbs contract both on closing and on opening the circuit, after which there is contraction of the limb traversed by the direct current on closing the circuit; and the other limb contracts on breaking the circuit; finally, only one limb contracts, viz. that of the inverse limb on the cessation of the passage of the current. On keeping the current closed for some minutes, we invariably remark that the inverse limb, which contracts on breaking the circle, is seized with a permanent contraction of a decidedly tonic character. This phenomenon is of importance, as it indicates an intimate connection between nervous influence and the action of the electric current, according to the direction of the latter.

The phenomenon manifests itself after the current has been passed for twenty-five or thirty minutes. It occurs equally on passing the current through the nerves

without its traversing the muscles; but never when it passes through muscle alone. If while the circle remains closed, and it has been previously ascertained that the tetanic contraction will follow the opening of the circuit, the nerve be divided rapidly at the precise point where it enters the muscle, the limb is thrown into contraction without its remaining in a state of tetanus. If, instead of this, the nerve be divided higher up near to its issue from the spinal marrow, then the tetanic contraction takes place as usual.

The previous exhibition of narcotics to the extent of narcotism, prevents the tetanic contractions. The passage of the inverse current, the same by which the phenomenon is produced, puts a stop to the tetanic contractions in two or three seconds. If the direct current be passed, it ceases in two or three minutes. The same phenomena occur in warm-blooded animals, only of shorter duration. No electric current is in circulation in the nerves, neither is any electricity rendered latent by the passage of the inverse current. The phenomenon in question belongs to the yet undetermined relation which exists between nervous influence, and the action of the electric current according to the direction of the latter.—*From the Philosophical Transactions.* x

BOOKS & PERIODICALS RECEIVED FOR REVIEW,

DURING THE LAST TWO WEEKS.

The Development of the Retina and Optic Nerve, &c. By Henry Gray, M.R.C.S. Observations on Chronic Hydrocephalus, &c. By F. Battersley, M.B. T.C.D. Animal Chemistry. By Dr. Bence Jones. The Bath Waters. By James Tunstall, M.D.

The Baths of Rhenish Germany, &c. By Edwin Lee.

The Public Health a Public Question. On the Management of Infancy, &c. By Charles Hogg, M.R.C.S.

Observations on Cholera. By G. Gwynne Bird, M.D.

The British American Journal. July 1850. Casper's Wochenachrift. Nos. 24, 25, 26 —15 to 29 June.

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer 29.814
" Thermometer 65.6
Self-registering do. Max. 105.5 Min. 47.
" From 12 observations daily. " Sun.

RAIN, in inches, 0.78.—Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was 42.6 above the mean of the month.

BIRTHS & DEATHS IN THE METROPOLIS

During the Week ending Saturday, July 20.

BIRTHS.		DEATHS.	
Males.....	592	Males....	434
Females..	601	Females..	439
	1193		873

CAUSES OF DEATH.

ALL CAUSES	863
SPECIFIED CAUSES	858
1. Zymotic (or Epidemic, Endemic, Contagious) Diseases....	198
Sporadic Diseases, viz.—	
1. Dropsy, Cancer, &c.	52
2. Brain, Spinal Marrow, Nerves, and Senses	117
4. Heart and Bloodvessels.....	31
5. Lungs and organs of Respiration	89
6. Stomach, Liver, &c.	72
7. Diseases of the Kidneys, &c.	10
8. Childbirth, Diseases of Uterus, &c.	3
9. Rheumatism, Diseases of Bones, Joints, &c.	10
10. Skin.....	3
11. Old Age	37
12. Sudden Deaths.....	6
13. Violence, Privation, Cold, &c....	24

The following is a selection of the numbers of Deaths from the most important special causes:

Small-pox.....	3	Convulsions.....	34
Measles.....	16	Bronchitis.....	1
Scarlatina.....	13	Pneumonia.....	45
Whooping-cough.....	23	Phthisis.....	107
Diarrhoea.....	51	Lungs.....	4
Cholera.....	5	Teething.....	15
Typhus.....	31	Stomach.....	5
Dropsy.....	20	Liver.....	8
Hydrocephalus.....	36	Childbirth.....	3
Apoplexy.....	11	Uterus.....	4
Paralysis.....	27		

REMARKS.—The total number of deaths was 110 below the average mortality of the 29th week of ten previous years. The mortality of London exhibits a slight increase this week; this is found to have obtained in the zymotic or epidemic classes of disease, and more particularly in diarrhoea, among infants: thus, in 53 cases of this disease, 35 occurred in infants, and 8 under 10 years of age. Of five cases of cholera reported, four also occurred in infants. One case only is denominated "malignant," the other cases being English cholera. Other epidemic diseases have perceptibly declined.

NOTICES TO CORRESPONDENTS.

The First Report of the Metropolitan Sanitary Association has come safely to hand, and shall receive our early attention.

Dr. G. Merryweather.—We will find room in our next number for an extract from the paper forwarded to us. Our space will not allow us to do more than this.

We shall commence the publication of Dr. Jamieson's Lectures on the Medical Jurisprudence of Insanity next week.

Mr. Galtrey's third communication has been received. We cannot yet fix the time for commencing the series of papers; but due notice shall be given to our correspondent, and a proof sent to him a week before publication.

The papers of Mr. Balman, Mr. W. Addison, and Mr. Grantham, will appear in the following number. Proofs will be sent.

RECEIVED.—Dr. James Esdaile (Calcutta).

Lectures.

LECTURES

ON THE

MEDICAL JURISPRUDENCE OF
INSANITY.

*Delivered in the Medical School of King's
College, Aberdeen.*

BY ROBERT JAMIESON, M.D.

Lecturer on Medical Jurisprudence in the
University.

LECTURE I.

*On the relation of insanity to certain allied
conditions—Resemblance of insanity to
dreaming, febrile delirium, and delirium
tremens—these occasional causes of the
disease—Excited passion and insanity—
Psychological character of insanity—
Insanity contrasted with illusion, hallu-
cination, delusion, disease with mental
symptoms, intellectual dulness, eccen-
tricity, abused fancy, and moral de-
pravity—Pathology of insanity.*

MEDICAL and legal doctrines on the sub-
ject of diseased mind are even in this pre-
sent day far from harmonious, and there-
fore in teaching you what is termed the
medical jurisprudence of insanity, I shall
consider it right to occupy not a little of
the time with a detail of the features of
mental derangement, so as to afford you a
just notion of what the lunatic condition
actually is, and put you in a position to
form independent judgments on the points
which are referred to medical opinion. In
so doing, while I shall seek to avoid all
pathological and therapeutic matters as
unsuited to the objects of such a course, it
may happen, in regard to most of you, that
I shall be effecting something in the way of
filling up what is ordinarily an unfortunate
hiatus in the education of the profession.

Insanity is one of several states of im-
perfect action to which the organism of the
human brain is liable. Among these I may
enumerate, for example, the conditions of
dreaming, and the several varieties of
delirium, all of which are forms of mental
unsoundness, having certain common re-
semblances, and bearing undeniable rela-
tions to the state of mind which exists in
the insane. They are all conditions in
which certain faculties of the mind are in
abeyance, and in which, as a necessary
consequence, psychical manifestations are
perverted or imperfect.

Dr. Holland is of opinion that more

cases of insanity bear a similarity to the
mental phasis which constitutes dreaming,
than to any other cerebral condition what-
ever; and certainly there are no observa-
tions in our power to make, that seem to
convey so distinct an impression of the
sensorial state of the insane, as those which
an individual may attempt, by turning his
attention upon what takes place in his own
mind when merging into the sleeping or
the waking state. In the transition be-
tween these two conditions the mental op-
erations have many features in common with
delirium and intellectual derangement. As
the senses are gradually released from sleep
or overcome by it, the succession of thoughts
is slowly emancipated from control; the
ideas are combined in a manner which is
altogether involuntary and dissimilar to the
ordinary workings of the imagination. Con-
ceptions take the place of sensations, as
in the insane, and sensations become misin-
terpreted, as happens in cases in which the
judgment is diseased. If the train of
thought were expressed, it would be in
speech as rambling and incoherent as the
raving of the demented. The cerebral
condition in dreaming is so far allied to
that of insanity, that occasionally a state
of painful dreaming gives the first warning
of approaching mental disease, and the pa-
tient seems to suffer from the disorder in
"the howling wilderness of sleep," for some
time before it has gained sufficient energy
to influence the powers of volition and
judgment when he is awake. I have occa-
sionally heard an attack of insanity attrib-
uted to a frightful dream, the delusion
that afterwards haunted the mind traced
to an impression produced in this way, and
in some cases have known the state of in-
sane to spring apparently directly from
that of sleep.

The following seems an example of a
cerebral condition induced by sleep merg-
ing into that of insanity, the patient going
to bed sane, and awaking lunatic:—A
female servant in the country, a robust
young woman, about twenty-seven years of
age, in whom no mental peculiarities had
ever been observed, came down stairs from
her sleeping apartment one morning com-
plaining of headache, and inability to go
about her ordinary occupation. She exhib-
ited much excitement of manner, and
confusion in her talk, and repeatedly called
out for her master's son, who she insisted
had been with her during the night, and
had promised her marriage, or, as she
phrased it, had married her before God.
Circumstances were every way at variance
with the possibility of her story, and her
whole appearance and conversation were
manifestly those of a person labouring
under an insane delusion. Attempts were

made to reason her out of the notion, but in vain. Continuing to become worse, she was sent home to her friends, and put under medical treatment, with so much advantage, that in a few weeks she was allowed to return to service. Her old place, however, very speedily caused the delusion to be renewed in all its force, and she was again returned to her friends, with whom she continued for some months, constantly manifesting the same monomania. She then came under my charge; but it was some months before she recovered, and became aware of the extent to which she had been deceived by her own imagination. A year or two afterwards, when she was in a different part of the country, I am given to understand that the same fancy returned very suddenly when she was in a stooping position cleaning a grate.

The state of dreaming resembles that of delirium or insanity, in a similar disturbance of the laws by which ideas succeed each other in the mind, and a corresponding loss of voluntary control over the trains of thought. Hence the intellectual manifestations of memory and comparison are impaired in both states: conceptions become sensations, constituting dreams to the sleeper, and delusions to the lunatic. If the dreamer added the fancies of his sleep to his waking experience and belief, as an insane person not unfrequently does, and as appears to have happened in the case just narrated, he would in no way differ from the latter.

Between febrile and maniacal *delirium* the distinction seems to be entirely pathological: psychologically they are the same. In the former, the vital functions of the great nervous centre are more deeply implicated, and it is from this character that a diagnosis is best deduced. Either state may pass into the other, the delirium of fever becoming mania, and the delirium of mania assuming an appearance which corresponds to the nervous symptoms in the worst description of typhus. Such changes are effected not by any alteration of the mental, but of the physical symptoms of the disorder. If a fever patient continues to labour under delirium after all other febrile symptoms have abated, he becomes a lunatic patient, and will have to pass from the fever hospital to the asylum. This forms the occasional history of a case of insanity, and most commonly in young persons. The variety of insanity assumed is generally that of dementia, and the recovery, though tedious, is not improbable.

A carpenter's apprentice, aged seventeen, was attacked with the usual indications of fever of a low type. The case throughout was marked by the predominance of symp-

toms referrible to the nervous system; the skin was very hot; the pulse feeble and quick; the tongue dry and brown; there was much subsultus, and delirium was very persistent. In the course of three weeks all the febrile symptoms had abated; the skin was cool; the pulse at a natural standard, and the appetite restored; but the eyes had a very wild expression; he could not be prevailed upon to answer questions; he was violent and unmanageable, without indicating any reason, or making any complaint. Though his strength was daily improving, and his bodily health apparently restored, the mental symptoms became more and more urgent; and he was considered to be insane, and put under superintendence accordingly. The exact state of his mind could not be ascertained, for he scarcely spoke for several months, but his actions were very extraordinary, and apparently influenced by spectral objects and aural impressions. His recovery occupied a period of fourteen months.

You will find several interesting examples of delusion and mental disease combined with, or resulting from, fever, in Roupell's *Treatise on Typhus*.

The change from a state of maniacal excitement to one resembling the delirium of typhus, is familiar enough to the superintendents of lunatic hospitals, as of occasional occurrence towards the fatal close of cases of violent and acute mania. In such instances the patient seems to die entirely of the vital exhaustion which is produced by extreme excitement of the nervous system, and an individual, seeing the case for the first time at this stage, might from the symptoms suppose it to be one of malignant fever.

Between the mental condition of *delirium tremens* and insanity, there seems to be no other difference than what is made by duration. If the patient, labouring under symptoms of a certain sort, die or recover within a reasonable time, say two or three weeks, he is said to have had delirium tremens, but if the delirium persist beyond some such period, the case becomes one of mental alienation, and is dealt with accordingly. Such is the history of very many cases of insanity which have had their origin in intemperance.

Besides the above conditions, there are several other states and moods of the mind to which insanity has kindred features; in which there is in degree a similar loss of power over the thoughts, with inefficiency of judgment, and even illusions of sense. Anger, fear, grief, enthusiasm, and excitement of the imagination, may put a fool's cap for a time on the wisest head, producing states that differ from unsoundness only in the shortness of their duration, and

the possibility of their being rectified by voluntary effort.

Insanity bears resemblances to various conditions, and presents itself in a variety of forms. Sometimes it is congenital, and consists of undeveloped faculties; the powers of the mind have never been completed, or they have never acquired the energy necessary to manifest themselves in natural activity. Sometimes it mainly assumes the form of ungoverned passion and loss of moral control. Another time it resembles delirium, and consists in intellectual disorder and confusion; and lastly, it may arise from destruction or obliteration of one or more of the faculties of the intellect. In all instances the characteristic is defect of mental power, either congenital or the consequence of debilitating causes. The power which is deficient is the controlling power of the will, as exercised over the current of thought, the relation through which every mental operation is produced. All imperfect mental actions result from this cause, but none of these are denominated insanity unless that imperfect action which has either congenital defect or disease as its cause, and for its sign a relative or an universal incapacity of judging. Observe, I do not say an incapacity of judging correctly, but an incapacity of judging at all: the former would merely indicate imperfect thinking or error, but the latter indicates disease. The loss of the power of judging is exhibited in matters of sense, in matters of conduct, or, it may be, in every matter with which judgment should be concerned. Suppose it to be a diseased perception that cannot be canvassed by the judgment, then the phasis of the insanity is intellectual, and its leading symptom delusion; but should it be an emotion that is diseased, then the moral aspect of the individual is unsound, and the prominent feature a loss of control over actions; and instead of a calm judgment guiding the conduct, you may have an impulse as resistless as a necessity, unassailable by reasoning, and removable only indirectly by treatment.

To constitute insanity, then, it is necessary that a diseased condition of the mind interfere with the production of that result of the comparison of one thought with another which we term judgment. We cannot, in the estimation of Dr. Connolly, approach any nearer to a definition of the state, than by saying, that it is an impairment of one or more of the faculties of the mind, including or inducing a defect in the comparing faculty. Until the morbid action is thus powerful, or thus much extended, it is not insanity. Diseased perception may produce illusion or hallucination, but so long as the power of com-

paring it with other perceptions is intact, and entire, the false impression is corrected, and has no influence, but a voluntary one, upon the conduct. Even should the illusion continue, its inconsistency is more apparent to the judgment, than its existence is clear to the sense. In such a condition an individual possesses a phantasma, but is not possessed by it; it is under the control of his higher faculties: he can compass it about, and deal with it as he pleases. An insane person finds no curative incompatibility with other perceptions in his delusion, because he has no power of instituting the comparison. It controls his will, but his will has no reaction upon it; and he remains possessed by it, and insane, until such time as his volition is enabled so to handle his morbid idea, as to make it subservient to comparison and judgment.

Illusion, hallucination, and delusion, though usual phenomena in insanity, are not sufficient to constitute the disease. These terms are of common occurrence in medical writings on the subject, and are not always used very categorically. An *illusion* is a mistake of the senses, a false perception; a *hallucination* is a baseless creation of the fancy; a *delusion* is an illusion or hallucination misleading the judgment, and governing the conduct. Thus, if a person returning home at night mistook the trunk of a tree for an apparition, that would be an illusion; if he saw a ghost where there was neither tree nor anything else, that would be a hallucination; if he believed in the reality of this conception, he would be under a delusion; and if he continued to act in accordance with such a belief, he would be as one insane, yet not truly mad so long as his obedience to it remained a matter of his own free will.

A careful distinction must be made betwixt diseases having mental symptoms, and diseases of the mind; betwixt madness, and such affections as hypochondriasis and hysteria. Hypochondriasis consists of illusions and hallucinations regarding one's bodily sensations, and is therefore not insanity, until these become deep-seated and permanent delusions, compelling to irrational conduct. Hysteria, hypochondriasis, and various other affections, which often precede, cause, and accompany insanity, are not diseases of the mind, but more strictly speaking diseases having mental symptoms, until by generating delusions they pass into insanity, and afterwards continue as complications of the disorder which they have created. Thus we have hysterical mania, which is not hysteria merely, but hysteria and mania combined; so also there is hypochondriacal monomania, which is not hypochondriasis alone,

but hypochondriasis and insanity together.

Simple defect of intellectual power, *intellectual dulness*, must not be confounded with the imbecility which constitutes a variety of insanity, which is not merely a feeble intensity of mind, but a positive deficiency of faculties. Wherever there is a feeble exercise of attention, or defective memory, there will be imperfect comparison, and resulting inaccuracy of judgment; but not from these, or similar causes, any necessary tendency to be governed by delusions. In all cases of imbecility included in the term unsoundness of mind, there are both emotional and intellectual deficiency, a loss of control over conduct, and from the antecedence of judgment being in some part defective or diseased, a liability to confound the actual with the unreal. Delusion is not a prominent characteristic, however, for in such cases feebleness is the type of all the mental manifestations, whether healthy or disordered.

Eccentricity has also to be distinguished from insanity. All the insane are eccentric in their ideas, their language, or their conduct, but the merely eccentric have but a voluntary resemblance to the insane. Eccentricities differ from lunacies in not arising from a loss, but from an undue exercise, of the faculty of judgment; very often from a vanity or self-respect in the individual, that leads him to prefer his own judgment to all other judgment, experience, and authority. They offend against custom and experience more than against reason. The eccentric, if he cannot give a satisfactory reason for his *outré* conduct, can at least assign an intelligible motive: the lunatic has no explanation to afford that does not involve an absurdity. A certain individual behaves in all respects like other men, unless that he constantly walks about without a hat, or any other artificial covering for his head. When questioned upon this point of disagreement with the custom of his neighbours, he says, that nature did not intend the head to have any other protection than what she herself had afforded, that a bare head is more becoming, that he feels himself in every way more comfortable as he is, and that he is certain that he will live longer in consequence of acting in this rational way. In two instances in which lunatics adopted the same habit, the causes assigned by them were of a very different description. One did not have his head covered, because it had grown so large that he could not get a cap to fit it; and the other was so annoyed by certain mischievous tormentors of an invisible kind drumming upon the crown of his hat, that in general he preferred to carry it in his hand. The eccentric man had a rule of

conduct, the result of his own narrow judgment; the two lunatics were impelled by fancies upon which their judgment was entirely inoperative. Lord Monboddo, insisting that the human family were originally adorned with tails, showed himself an eccentric theorist: had he asserted that they actually retained them, he would have had an insane delusion, instead of a philosophic crotchet. He would have had a false perception on which his judgment was inoperative, whereas, he was guided by his judgment to a strange conclusion.

Eccentricities no more constitute insanity, than idiosyncrasies constitute disease; and as these are competent with a sound state of the body, so are those with even a vigorous judgment. For example: there was an old man well known in London in the last century, who was of an ungainly appearance, and subject to occasional attacks of a hereditary melancholy. So inconsistent was he in his habits, that sometimes he practised great abstemiousness, and at other times devoured huge meals, with brutish slovenliness and voracity; sometimes he would persist in drinking nothing stronger than water, but occasionally he drank wine by tumblerfuls. His income was far from large, and not of a certain amount, yet he kept a set of old men and women about his house, whose bickerings and disagreements now and then drove him out of doors. He was in general very loquacious, but had been known to sit in company and drink a dozen cups of tea, without speaking a syllable. When not engaged discoursing, it was his custom to keep muttering to himself. In walking, he performed strange gesticulations with his limbs, and would not go in at a door, unless he could effect his entry in a certain preconceived number of steps, and so as to introduce himself on a particular foot, turning back, and recommencing, until he succeeded as he desired. There was a row of posts near his house which he would not pass without touching singly, and if he found that he had omitted one in the series, he retraced his steps to remedy the neglect. He hoarded up orange-skins for some mysterious purpose which he would never divulge. He suffered remorse of conscience for once having taken milk with his coffee on Good Friday. He believed in ghosts, and went ghost-hunting in Cock-lane; and he maintained that he had heard his mother calling upon him by name from the other world. Yet Dr. Johnson was so far from insane, that his judgment commanded respect and admiration everywhere, and by the common consent of eminent contemporaries he was the most vigorous thinker and the greatest sage of his time.

There are, however, instances of eccen-

trick conduct resulting from *abuse of the imagination*, which verge closely upon madness, and occasionally pass into it. The distinction is, that the individual's voluntary power over his thoughts is capable of bringing the comparing faculty into efficient operation, when he chooses duly to exert his will. Thomas Hood speaks of one who, in consequence of exciting his fancy by German tales of diablerie, used to fly up stairs at his utmost speed from the street-door to the attics, because a sort of wager with the devil came into his head, that he would gain the top before counting a certain number, or forfeit eternal happiness. Every one possibly experiences moods which differ in no respect from insanity, than that they are neither permanent nor independent of the will. These may be common in imaginative minds, but instead of being indulged, they should be guarded against and restrained, for, though they exist at first by sufferance of the will, they sometimes gain a strength that defies control, and triumphs over the reason. Hoffmann, a master in fantastic fiction, suffered so much from intemperate abuse of the imaginative faculty, that solitude became terrible to him. He was never quit of a mysterious sense of danger; things the most cheerful became incongruously associated with thoughts the most dreadful, while monsters and spectres, which he himself had created, tyrannized over his reason. To appease his terrors, he had frequently to summon his wife from bed, to sit by him as he studied at night. He was constantly on the verge of insanity, and died of spinal disease, his mind being tortured by his fancy to the last hour of his existence.

Often it is not an easy matter to draw a distinction betwixt insanity and *moral depravity*. In every case of mental alienation there is disorder of moral as well as of intellectual manifestations. The moral disturbance is usually the earliest developed feature of the malady, frequently it is the most prominent, and occasionally no other is distinguished; so that the bad have been liable to pass as mad, and the mad for bad, according to the philosophy and fashion of the time. There is no problem in law, medicine, and ethics, of greater social importance, or of more difficult solution, than the discrimination of insanity and vice. A wicked deed may be the result of an undeveloped moral sense, as for example in a child or an idiot; it may be the consequence of such extremity of passion as impels to instinctive, instead of deliberative action; and it may be the indication of a conscience enfeebled by voluntary neglect, and the habitual gratification of evil desires. Any of these states may be simulated by disease; but it is with the two

last only that the jurist will have difficulty; for, in all cases of insanity in which the moral sense is non-existent, the powers of the understanding also are either undeveloped or destroyed. No hideousness of depravity can amount to proof of insanity, unsupported by evidence of a judgment incapacitated or a will fettered by disease. In those cases of mental disorder in which the emotions are perverted, and where there is no clear proof of deranged intellect, cases which do from time to time occur, the presumption of insanity, in regard to a criminal action, has to be upheld by evidence of suspension of the will. The actions of an individual in such a state ought to be impulsive, involuntary, and irreconcilable with the idea of a healthy state of the emotional faculties.

I have endeavoured to draw as plainly as possible certain distinctions betwixt insanity and some conditions which, as they are allied to it, are sometimes confounded in it. Let me now request your attention, in conclusion, to a few remarks on its essential nature as a disease. I have already spoken of its psychological character. As a pathological state it is easier to tell what it is not, than what it is. Some will have it to be a disease of the body, in which metaphysics are useless; others, a disease of the mind, to which physic is as inappropriate. Still we find it curable sometimes by moral treatment, sometimes by medical; but far more usually by a judicious combination of the two, and almost never remedied by unassisted nature: a truly impresible condition, and far from an opprobrium medicorum. It is not a purely corporeal disease, like one of the neuroses; it is not a nervous affection merely, but a neurosis and something more; neither is it a purely mental affection, like error or vice. Both mind and body are at fault. According to the views of Feuchtersleben, it is their relation that is diseased; of the body to the mind, so that perception is morbid; of the mind to the body, so that volition is disordered. From which, then, does that disturbed relation proceed, which, when established, becomes reciprocal? If from the organism, it is a physical disease; if from the mind, a mental one, although in a sense not to be defined. Were I to dogmatize to you on this abstruse matter, I would say that there are two kinds of insanity, one a mental disease, the other a bodily one, both acquiring the psychosomatic character. There is a form of insanity produced by mental causes, in which the physical symptoms appear secondarily, which is sometimes curable by moral means, and which, if uncured, either leaves no traces behind, or a morbid anatomy which is its result, and in no case its cause.

The other form proceeds from physical derangement, occasions sympathetic mental aberration disappearing with its cause and having a traceable pathology.

Of the allied conditions of which I have been speaking, hallucination, stupidity, eccentricity, excited fancy, and moral obliquity, would range with the former; and illusion, hypochondriasis, and hysteria, with the latter. But the essential state—the diseased reciprocal relation disturbing personality—would be fully represented only by those conditions which I have now omitted to enumerate—namely, delirium and dreaming. The others have a resemblance to insanity in their origin, but do not partake of its essential character.

I repeat, that insanity is not, strictly speaking, to be termed either a bodily or a mental disease; that it is a disturbed reciprocal relation of mind and body; but that in its origin it is sometimes a mental, sometimes a bodily disorder; and that in the allied states alluded to, sympathetic delirium—which, if we recognize the two kinds of insanity spoken of—is a representative of the second, and the delirium of dreaming, which is a representative of the first, are the only conditions that have the same essential nature with what is termed unsoundness of mind.

Original Communications.

BRIEF NOTES ON THE DISEASE, INDIAN VILLAGE CHOLERA, AND ITS TREATMENT.

By ASSISTANT-SURGEON MOORE, B.A.
Gwalior Contingent.

[Continued from last vol. p. 1107.]

PART V.

Treatment.—Emeticised antimonial powder,* prescribed in doses of from five to ten grains, after the lunar caustic has made an impression on the morbid inflammatory action in the stomach and intestinal canal, will in general effect a diversion of the sero-mucous secretions from the gastro-intestinal mucous membrane to the skin. The functions of the kidneys have also been restored. The first discharge of urine from the bladder has flowed in a turbid

stream, and has been loaded with a thick sediment. In the subsequent treatment, strict attention must be paid to supplying the patient with light nutritious food, and in regulating the bowels by mild purgatives. By degrees the inspissated and tenaciously-adherent mucus becomes detached from the surface of the mucous membrane, and forms the bulk of the dejections passed from the bowels. The stools are ashen-grey in colour: at a later period they become tinged with bile.

The General Board of Health in London have favoured the public and the profession with important notifications relative to the measures to be adopted in the premonitory stage of cholera. The following paragraphs have been extracted from one of these notifications:—

“Cholera!—Medical authorities are agreed, that the remedies proper for the premonitory symptoms are the same as those found efficacious in common diarrhoea; that the most simple remedies will suffice, if given on the first manifestation of this symptom; and that the following, which are within the reach and management of every one, may be regarded as among the most useful: namely—20 grains of opiate confection, mixed with two table-spoonfuls of peppermint water, or with a little weak brandy and water, and repeated every three or four hours, or oftener, if the attack is severe; or an ounce of the compound chalk mixture, with 10 or 15 drops of laudanum, repeated in the same manner: from half a drachm to one drachm of tincture of catechu may be added to this last *if the attack is severe*.

“Half these quantities should be given to young persons under 15, and still smaller doses to infants, &c.

“(Signed)

CARLYLE.

E. CHADWICK,
S. SMITH.”

Cholera must be a disease simple in its form, and mild in its symptoms, when it yields so readily to these, the simplest of remedies. What explanation can the members of the General Board of Health give, for 12,000 persons of all ages, male and female, having been swept away, in the course of six or eight weeks, in Great Britain alone? What explanation can the noble Earl, the President of the Board, and his colleagues, give, when the victims of

* Vide MED. GAZ.—Antimonial Powder, 100 grains; Tartar Emetic, 5 grains: rub together.

cholera lie numbered not by hundreds, but by thousands, in an island where opiate confection and weak brandy and water,—where compound chalk-mixture, laudanum, and tincture of catechu,—such efficacious medicines in subduing cholera in its early stages, were within the reach of every one? The rates of mortality prove too clearly that they have erred in opinion. The same rates prove too clearly that the public has been misled by the representations of the members of the General Board of Health. The harmonious agreement of opinion among medical authorities, proclaimed by the General Board of Health, is in reality the harmony of discord. The unanimity of opinion as to the treatment to be adopted in cholera, is the unanimity of antagonistic conflicting opinions. The unheard-of discovery made by the noble Earl and his medical colleagues is worthy of being recorded in the medical literature of Great Britain.

In all discharges from the bowels—serous, mucous, or sanguineous; sero-mucous, muco-sanguineous, or sero-muco-sanguineous; whether such be designated by nosologists as acute or sub-acute diarrhoea, acute or subacute dysentery, acute, or mild English, or Asiatic cholera—there is no plan of treatment which will bear comparison with the extensive abstraction of blood, by cupping, from the surface of the abdomen, and the action of lunar caustic brought into immediate contact with the surface of the inflamed and secreting mucous membrane.

CASE XXI.—Bhola Naick, 4th Company, 5th Infantry, Gwalior Contingent, was sent in from the out-station of Balahut, in the district of Chundeeree, on the 22nd November, 1849. He had been seized with violent purging on the morning of the 21st. On admission into the Regimental Hospital at Lullutpoor, the discharges from the bowels were clear and sero-mucous in appearance, mixed with flakes of lymph, and containing a thick sediment. He was sunk and exhausted. The skin of his face was pinched backwards, giving a sharp and peculiar cast to his features. Thirst was urgent. The pulse ranged between 120 and 125, and was small, sharp, and wiry. The extremities were cold; the abdomen was sunken. The number of stools passed since the attack were stated

by him to have been twenty-eight or thirty. Within an hour after his arrival four liquid stools were passed. He did not suffer from vomiting, nor from spasmodic contractions of the muscles.

The treatment adopted consisted of cupping over the surface of the abdomen, and the administration of caustic pill No. 3. The cupping was succeeded by the application of a blister, which was afterwards dressed with mercurial ointment. A second pill was ordered, and with each pill as much cold water as the patient could drink.

“The sero-mucous secretions from the bowels were checked; thirst was allayed; the pulse fell to 90, and expanded in volume; heat returned to the extremities; the absorption of the mercury into the system was quick and unexpected; the gums became spongy, and the breath fetid. Beyond this the mercurial action was not pushed. As the bowels were not moved, except by the aid of a mild purgative draught, he was pronounced convalescent on the fourth day after admission.

CASE XXII.—Sawut Sing, Sepoy, 6th Company, 5th Infantry, Gwalior Contingent, was admitted into hospital at Lullutpoor, on the 5th November, 1849, having been purged twelve times previous to his removal. The discharges from the bowels were thin, sero-mucous, and mixed with blood. The pulse was quick, small, and wiry, 180 in the minute; the tongue was red. He suffered from thirst. The extremities were cold; the abdomen was sunken. He was so completely exhausted that with difficulty he spoke. The voice was feeble. He did not suffer from vomiting, nor from cramps, nor from suppression of urine.

He was cupped extensively over the abdomen; blood flowed freely. Caustic pill No. 3 was washed down with a quart or more of cold water given at short intervals. The pill was repeated in the evening.

After the first pill he had four liquid discharges from the bowels, tinged with blood. On this account the pill was repeated. After the second caustic pill the discharges were checked. The bowels did not act until a purgative draught was prescribed. The pulse fell to 96, and expanded in volume. Heat returned to the extremities. His recovery was satisfactory.

ABSTRACT OF CASES OF SEROUS, MUCOUS, AND BANGUINOUS DISCHARGES FROM THE INTESTINAL CANAL, TREATED BY CUPPING EXTERNALLY, AND LUNAR CAUSTIC INTERNALLY.

Names and Ages.	Date of Attack.	Character and number of liquid discharges from the intestinal canal.	Chief symptoms attending the discharges from the bowels.	Treatment.	Results of treatment. Effects produced on the symptoms and state of the bowels.	GENERAL REMARKS.
Case 23. Bahadur Sing, Sepoy, 1st Co. 5th Infantry. Gwalior Contingent.	Admitted, with symptoms of fever, on 5th Nov. 1849. On the 7th violent purging set in.	Nine liquid discharges in the course of 12 hours—sero-mucous and deeply tinged with blood.	Thirst urgent. Pulse 120, small, contracted, wiry; tongue red and glazed. Burning heat of skin. Pain in the abdomen; gripping. Restless and anxious.	Cupping extensively over the abdomen. Lunar caustic pill No. 3 washed down with copious draughts of cold water, or warm water if preferred.	One sero-mucous discharge in the course of 24 hours, free from blood. Pulse fallen from 120 to 89, full and soft. Thirst allayed. No return of the slimy discharges from the bowels.	At the onset of the purging a rhubarb draught, with ipecacuanha, was given. This was followed by blue pill and Dover's powder. The discharges from the bowels, however, increased in quantity and in frequency. The treatment was then changed. He was discharged convalescent on the 11th.
Case 24. Lehahat Khan Sower, 1st Troop, 1st Cavalry. Gwalior Contingent.	18th Nov. 1848.	Nineteen copious, liquid, muco-sanguineous stools in 24 hours. Colour deeply dyed of a blood-red. Flakes of lymph and shreds of mucus floated in the fluid.	Extreme exhaustion. Pulse less. Spasmodic twitches in the muscles. Thirst insatiable. Eyes sunken in their sockets. Extremities cold. Skin of the fingers and toes shrivelled. Urine not suppressed.	Cupping Cautic pill No. 3 to be repeated. Blister to the abdomen, to be dressed with mercurial ointment.	Discharges checked, and the cramps relieved. On the 14th, two liquid stools, slightly tinged with blood, were passed. On the 15th, none. On the 16th, none. The pulse returned. A warm perspiration broke out over the body.	After the second gush of muco-sanguineous fluid, he was ordered 5 grs. of calomel, and 4 grain of opium, every 3rd hour. He swallowed 4 pills. A mustard cataplasim was applied over the stomach. The discharges from the bowels, rapidly increased. After the 19th stool, the treatment was changed. The mercury was rapidly absorbed into the system by the raw blistered surface. The gums became spongy. The breath fetid, and salivation followed.

Case 25 Umur Anesty, Sepoy 6th Company, 5th Infantry, Gwalior Contingent.	15th Nov. 1849.	Ten liquid sero-mucous discharges from the bowels in the 24 hours, mixed with flakes of lymph, and a muddy deposit in the pan for five consecutive days.	Pulse 100, hard, sharp, and wiry. Thirst urgent. Anxiety. Prostration of strength. Gripping and tenemus. Tongue red and glazed.	Cupping over the abdomen. Caustic pill No. 3 morning and evening. Hot or cold drinks to be supplied when desired by the patient.	Frequency of the discharges diminished from 10 to 3, after the 3rd. and 17th, blue pill and Dover's powders were ordered. No impression was made on the state of the bowels. The treatment was changed. The pulse fell to 84, and expanded in volume. A perspiration broke out all over the body. He was discharged convalescent.	When taken to the hospital he received a rubarb draught, with laudanum. On the 16th and 17th, blue pill and Dover's powders were ordered. No impression was made on the state of the bowels. The treatment was changed. The pulse fell to 84, and expanded in volume. A perspiration broke out all over the body. He was discharged convalescent.
Case 26. Mattra Tewarry, Sepoy, 2nd Company, 5th Infantry, Gwalior Contingent.	24th Oct. 1849.	The complaint commenced with 5 stools in the day, and 6 in the night. On the 24th 12 liquid slimy stools were voided in the course of 12 hours. They were tinged with blood, and loaded with flakes of lymph and an ashen-grey coloured sediment.	Thirst insatiable. Restlessness. Anxiety. Prostration of strength. No appetite. Urine scanty, and high coloured. Pulse 124, small and thrilling. Tongue loaded and clammy.	On the 26th he was cupped over the abdomen, and was ordered caustic pill No. 3, to be washed down with large draughts of cold water.	In the 24 hours preceding the cupping, 13 copious sero-mucous stools were passed from the bowels. From the 26th to the 28th, after the caustic pill the bowels were moved once.	A rubarb draught, with laudanum, blue pill, and Dover's powder, with astringents, were tried in the first instance. They failed in checking the purging. The results of cupping and lunar caustic were so far satisfactory that the Sepoy was discharged from the hospital, at his own request, on the 29th Nov. 1849.
Case 27. Ajoodha Sipahi, 3rd Company, 5th Infantry, Gwalior Contingent.	22nd Oct. 1849.	Complaint ushered in by 7 stools in the day, and 6 in the night. On the 23rd, 6 in the day, and 11 in the night; total, 17 liquid stools in the 24 hours. Sero-mucous, and of a reddish tinge.	Thirst moderate. Gripping and tenemus. Quick and wiry pulse. Restless and anxious. Loaded and clammy tongue.	On the 24th, was cupped; and was ordered caustic pill No. 3.	In the 24 hours preceding the cupping and pills, 17 liquid evacuations were passed. From the 24th to the 26th, one discharge of a sero-mucous character was voided.	The astringents employed to check the purging failed. The irritability of the bowels increased on the 23rd. The plan of treatment was changed. The results arising from the cupping and caustic pills were so far satisfactory, that "convalescent" was written opposite his name on the 26th November, 1849.

ABSTRACT OF CASES—continued.

Name and Age.	Date of Attack.	Character and number of liquid discharges from the intestinal canal.	Chief symptoms attending the discharges from the bowels.	Treatment.	Results of treatment.—Effects produced on the symptoms and state of the bowels.	GENERAL REMARKS.
Case 28. Ram Sing, 25 years. Regiment.	20th Sept. 1849.	Profuse alimy, sero-mucous stools, passed 20 times in the 24 hours. Examined with the aid of a lens, lymph and mucus, in considerable quantity, were noticed.	Prostration. Heat of skin. Foul tongue; thirst; restlessness and general fever. Pulse 110, throbbing. Pain in the iliac regions. Scanty and high-coloured urine.	Cupping. Lunar caustic pill No. 3, morning and evening.	Cessation of the violent purging after the 2nd pill. Pulse reduced to 80, soft. Perspiration over the body. Copious flow of urine.	Without wasting time, and trilling with the symptoms, by administering laudanum, &c. this man was cupped, and had the caustic pill. He returned to his village on the 3rd day.
Case 29 Jánari Pundit, prisoner. 60 years.	20th Nov. 1849.	Looseness of the bowels; three thin alimy discharges in the night, and thirteen in the day. Liquid passed the same as in Case 28.	Furred tongue; quick pulse. Thirst urgent. Pain and griping in the bowels. Scanty urine, high-coloured, but not suppressed.	On the 24th 1 caustic pill No. 3. On the 4th Dec. cupped and ordered caustic pills morning and evening, blistered.	The number of stools was reduced to 1 in the 24 hrs. From the 26th to the 30th the bowels were not moved once. On the 4th December purging returned, which called for the treatment noted.	From the 20th to the 24th Nov the treatment consisted of opiates and astringents. The caustic pill was substituted when the purging continued. For four days the bowels remained totally inactive. The return of the sero-mucous stools, rendered it necessary to cup and blister. Recovery soon followed.
Case 30. Jabra Senar, prisoner. 30 years.	28th Nov. 1849.	Seven stools in the day and 5 in the night: thin alimy, sero-mucous, and deeply tinged with blood. Flakes of lymph and shreds of mucus in considerable quantity.	Thirst insatiable. Dry and crisp tongue, glazed. Small and thready pulses, 126 to 180. Punched features; cold extremities. Griping pains in the abdomen. Urine secreted.	Cupped extensively, caustic pill, No. 3. Blistered, and dressed with mercurial ointment.	The evacuations reduced from 18 to one. This was passed on the morning of the 30th Nov. In the evening, a second evacuation, more healthy in colour and consistence, and free from blood, was passed. The pulse fell to 90.	No delay took place in the remedial measures employed. This prisoner was cupped at once, and was given the caustic pill, with large draughts of cold water. He was then blistered. Mercurial action set in on the 8th of December. His recovery was satisfactory.

The cases registered in the foregoing table may be classified as cases of diarrhoea or of dysentery; of choleraic diarrhoea, of cholera mitis, or of cholera Asiatica atrocissima, by those who cannot recognise nor prescribe for a disease except it possess a specific name. It is high time that medical men should prescribe for the diseased structures from whence arises a specific train of symptoms, and not trifle with disease in adapting medical treatment to the fine-drawn distinctions between the morbid conditions of the same structure, denoted by specific euphonistic names.

The serous, mucous, and sanguineous, —the sero-mucous, muco-sanguineous, and sero-muco-sanguineous discharges of fluid from the intestinal canal, mixed with flakes of lymph and with shreds of inspissated mucus, could not have been voided unless there had existed a fretted, an irritable, an inflamed condition of the whole, or of some one part of the gastro-intestinal mucous membrane, glandular bodies, and subjacent tissues. This is the point which must be borne in mind. The extent of mucous surface engaged constitutes the sole difference between diseases closely allied, which may be ranked in one and the same family. The limited extent of surface from whence the sero-mucous discharges were eliminated, rendered the cases recorded tractable to treatment.

The first gush of sero-mucous fluid from the stomach and intestinal canal has been called the premonitory diarrhoea of cholera: this is an error. The members of the General Board of Health in London have fallen into this mistake in their notification on the subject of cholera. If it can be proved that a few inches of the intestinal canal above and below the *cæcum caput coli*, involved in a fretted, and irritable, and sero-mucous eliminating condition of its structures, is the point of departure for the spread of an extensively diffused inflammatory action upwards towards the stomach, and downwards towards the rectum, then may diarrhoea be regarded as the forerunner of cholera.

This theory will not stand the test of examination. The first gush of rice-water fluid from the stomach, and the first gush of rice-water fluid from the bowels, may be regarded, not as the *avant courier* of an attack of cholera, but as the *transit of the first into the second stage of the disease*, and originate in the rapid, the almost instantaneous develop-

ment, of a bright scarlet, or deep crimson red efflorescence, diffused over the internal surface of the stomach, the duodenum, the jejunum and the ileum intestines.

The premonitory diarrhoea,—with which the noble lord the Earl of Carlisle, President of the General Board of Health, and his medical colleagues, Mr. Ghadwick and Dr. Smith, appear to have been fascinated,—bears to the first and second stages of cholera as strong a degree of relationship as the flame of a farthing rushlight bears to the fiery blaze of a potter's furnace.

To extinguish that blaze they propose to add fuel to the fire.

[To be continued.]

CHLOROFORM IN A CASE OF POISONING BY STRYCHNINE. BY DR. MUNSON.

MR. G—, aged about 40, of intemperate habits, took from among some medicines, on the 5th instant, a bottle of strychnine; and supposing it to be morphine, as he said, swallowed a dose supposed to be about one or two grains. In about twenty minutes afterwards Dr. Munson was requested to see him, as he was supposed to be in a "fit." He found him in the following condition:—The whole muscular system rigid; the muscles of the back, and of the upper and lower extremities, rigidly contracted; the head drawn back; articulation difficult; sense of tightness about the chest, perspiration flowing profusely from the face and chest. A number of the physicians of the place came to his assistance. The usual remedies recommended in such cases were resorted to, but without any mitigation of the urgent symptoms. The patient was failing rapidly under the increasing spasmodic action of the whole muscular system. It was now determined to administer chloroform, as death was apparently certain without some relief. One drachm of chloroform was put upon a silk handkerchief, and the patient directed to inhale it. The effect was decisive. The patient (who was at this time in a sitting posture, held so by assistants, who could not move him in the least degree without exciting the most frightful and alarming spasms) requested to be placed in a recumbent position, which was done without exciting the least spasm. The chloroform was carefully administered for some hours, the patient holding the handkerchief most of the time himself, in order, as he said, "to keep off the dreadful spasms." From this time he recovered rapidly, and on the 7th instant was able to leave for home—a distance of six or seven miles.—*Doctrs Medical Journal*, July.

SCROFULA IN ITS RELATIONS TO
PULMONARY PHTHISIS.

(OKALATE OF LIME IN THE URINE.)

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ABOUT twelve months ago I suggested to one of my colleagues that each of the honorary surgeons of the Dispensary should be at liberty to select some particular group or class of diseases as his speciality, and that the cases belonging to such group occurring in the practice of the other surgeons should, as far as might be agreeable, be transferred to the care of the surgeon who had adopted this particular speciality. In this request all my colleagues readily acquiesced: hence the origin of the present paper. To my colleagues,—Messrs. Nottingham, Padley, Taylor, and Harris,—therefore, I am indebted for the opportunity thus afforded me of seeing and treating a larger number of cases, embracing almost every form and variety of the scrofulous disease, than could have fallen to the lot of many other individuals under the most favourable circumstances. The plan I set out with, and which I have steadily pursued up to this time, has been to note down the particulars of each case in the following order:—

1. Everything relating to the age, sex, precise form and duration of the disease, together with any external peculiarity in any way worthy of notice. 2d. The diseases that appeared the most frequently to have prevailed in the family, as regards brothers and sisters, parents, their brothers and sisters, separating each family under two heads,—viz., the number living, and if healthy or otherwise; and the number of deaths, and the causes thereof. Lastly, the grand parents on each side. From information obtained in this way have been collected together the particulars of 100 cases. This might appear at first sight a very easy task, but in reality it is not so; and is due in part to the ignorance of a large class of persons who seek relief at a dispensary rendering them unable, if willing, to furnish the necessary information, and also to the circumstance that in a mercantile town like Liverpool the relations of the person are very often scattered over almost every part of the globe: and, therefore, anything like a connect-

ing link in the family, as regards their state of health, physical or otherwise, it would be quite impossible to establish. With these and many other almost insuperable difficulties with which such an investigation is beset at its very threshold, no alternative was left but to pass them by as useless for any special statistical inquiry; otherwise I could, with much less labour, have troubled the number of cases now recorded: added to which, I have confined myself to one individual symptom, for reasons presently to be stated, by which I choose to identify the disease—viz., those affected with enlarged cervical glands of at least two months' standing, excluding all those cases in which such swellings appeared in any way to have arisen from other causes than the scrofulous taint; such, for example, as the sympathetic glandular tumors, as a consequence of any irritation of the scalp, painful dentition in children, and all other sources calculated to throw any doubt as to the diagnosis of the case.

Tumid absorbent glands of the neck have been selected as a diagnostic symptom, because such indication has been looked upon by almost every writer, from the earliest periods, as the characteristic type of the disease. Abnormal deviations of many other tissues, though generally regarded as due to scrofula, might not be considered by every one as belonging to this particular catalogue.

From these 100 cases I have endeavoured to tabulate all the more important features of each case, so as to present almost at a glance an epitome of the whole. The second table shows a large relative proportion of cases between the ages of 2 and 15, which can hardly be considered as giving a fair average, since about a third of the whole of these cases were obtained from children of both sexes attending some of the national schools of this town.

Table 5 has appeared to me especially interesting in connection with the etiology of scrofula, bearing as it does upon a subject on which the profession appear to be somewhat divided. The existence of phthisis in the family of so large a proportion of scrofulous individuals must be regarded as one of the strongest external proofs that can be adduced in favour of the identity of the two diseases.

It is perhaps hardly necessary to mention that these numerical statements

can only be regarded as a very close approximation to truth, and must apply equally to every investigation having similar objects in view; but I have every reason to believe that they will be found to be as perfect, as far as they go, as they can well be made. In all cases where it was possible, the information was obtained from the parents. When under age, or disqualified in any way from furnishing what I considered to be a correct statement, and when there appeared to be the least reason for questioning the veracity of my informants, the case was invariably laid aside.

TABLE I.

Showing the relative number of persons affected with Scrofula, as regards Temperament, &c.

	Male.	Female.	Total.
Number having light hair and complexions . . .	31	15	46
Do. having dark hair and complexions . .	22	13	35
Do. having red hair	12	7	19
			100
Do. presenting what is commonly known as the scrofulous countenance. . .	21	6	27

The results, on the whole, go to show that scrofula is for the most part common alike to all temperaments.

TABLE II.

Showing the proportions affected with Scrofula at Different Ages.

	Male.	Female.	Total.
Number affected with scrofula from 2 to 15 years of age . .	52	27	79
Do. from 15 to 30	11	8	19
Do. 30 and upwards	1	1	2
			100

The numbers in the first column will probably exceed the average, from the circumstances already alluded to.

TABLE III.

Showing the frequency with which other regions are affected with Indurated Scrofulous Glands simultaneously with those of the neck.

	Male.	Female.	Total.
Number of cases in which the neck only was affected . . .	56	38	89
Do. neck and axilla	4	1	5
Do. neck and popliteal space . .	1		1
Do. above the bend of the elbow	2	3	5
			100

The fourth column refers to cases in which a lymphatic gland, always found a few inches above the inner condyle of the humerus, was enlarged, or presented other characteristic indications of being the result of a scrofulous taint. I found the groin affected in two cases only.

TABLE IV.

Showing the probable Exciting Causes of Scrofula.

	Male.	Female.	Total.
Measles, scarlatina, and whooping-cough . .	19	12	31
Exposure to cold or damp	3	3	6
Cause not evident	38	20	58
Blows and other external injuries	1	1	2
Syphilis	2	1	3
			100

Of the causes mentioned in the first column, by far the most frequent appeared to be measles, terminating in a profuse catarrhal discharge from the mucous surfaces of the eyes, nose, and ears: on the cessation of this discharge the glands of the neck became tumid. I have notes of many cases in which diarrhoea, disconnected with the above, seems to have preceded the first manifestation of the disease. These lumps or kernels, as they are vulgarly called,

may continue for a longer or shorter time,—may return in the spring of each succeeding year, to disappear during the progress of summer, or remain stationary, sometimes pertinaciously resisting every therapeutic means employed for their removal until the age of puberty, when these swellings may vanish completely, and the person attain an advanced age, and ultimately die of some other complaint; or, what I believe to be not uncommon, perish of pulmonary phthisis in the first epoch of manhood.

In connection with the fifth column, I have some particulars of five other cases (males) in which the venereal taint had undoubtedly been the exciting cause, making seven cases in the whole. They are not included in the list because their history could not be ascertained: but, as far as my inquiries extended, there had been in every instance a phthisical tendency either in the parents or collateral relations. In six out of the seven cases, the sores appeared to have assumed a phagedenic character, attended with phimosis.

If a given number of well-recorded cases could be found in which scrofula manifestly appeared for the first time after a true syphilitic sore, without any predisposition to tubercular disease in any part of the family, such a circumstance would, I imagine, be a strong argument in favour of the occasional syphilitic origin of scrofula. The glands of the neck became affected about the same period after the primary sore as constitutional syphilis usually appears—viz., from the sixth week to the third month; and slight symptoms of the latter sometimes co-existed.

Mercury appears to have been injudiciously administered in almost every instance.

This class of persons are benefited in a very marked way by the internal use of iodide of potassium, all other reputed scrofulous remedies proving of no service whatever.

The following table exhibits the number of instances in which one or more deaths from consumption occurred in the family of each respective person having the fore-mentioned symptoms of scrofula. The aggregate number of deaths in each family are not stated, because it might tend to confuse the reader without adding much weight to the argument. It would have been

more complete if the causes of death, as regards the brothers and sisters of each grand parent, had been added: that was my intention, had I not afterwards found the extension of the inquiry to these branches to be impracticable.

TABLE V.

Showing the number of instances in which deaths from phthisis occurred in the families of 100 cases of scrofula:—

Own or patient's brothers and sisters.	Father.	Mother.	Number of families on the parents' side in which have been one or more cases of phthisis.	
			Male.	Female.
6	6	7	42	32

TABLE VI.

Showing the number of cases in which Phthisis appears to have been transmitted to their immediate issue:—

On the Father's side.			
Grandfather.		Grandmother.	
Transmitted	7	Transmitted	4
Not transmitted	5	Not transmitted	8
	12		12
On the Mother's side.			
Grandfather.		Grandmother.	
Transmitted	7	Transmitted	8
Not transmitted	1	Not transmitted	6
	8		14

In the 100 cases of scrofula, phthisis is traced as above in 88 instances, leaving only 17 in which no deaths from that disease could be ascertained. The sixth table, though on a small scale, is of sufficient interest to be appended to the above, bearing as it does directly upon the hereditary predisposition to phthisis, and the proportion of cases in which the disease seems to be inherited respectively from the phthisical parents, male and female. Thus the disease appears transmitted by the father to his issue in seven instances, and not in five; by the mother in four instances, and not in eight, and so on. It follows, therefore, that the phthisical father more frequently transmits the disease to the offspring than the mother, in the ratio of 7 to 3; whilst on the mother's side the ratio is as 6 are to 7.

TABLE VII.

Shows the number of deaths from other diseases in parents and grand-parents, some of which have been supposed to be sometimes associated with the scrofulous constitution: the former are classed together:—

	Parents.	Grand-parents (male).	Grand-parents (female).	Total.
Nervous system, including apoplexy or paralysis, mania, and epilepsy	5	12	7	24
Cancer	1	—	5	6
Calcul. Comp.	—	2	—	2
Cholera	1	2	5	8

It is not an easy matter to determine, with any degree of accuracy, the previous existence of scrofula in the parents and collateral relations in a sufficient number of cases to warrant one forming any very satisfactory conclusions. A person, for example, may have had the lymphatic glands enlarged from a scrofulous taint in early life, and afterwards disappeared without attracting any particular attention. Again, scrofulous disease of the joints, caries, rickets, and such analogous affections, though commonly recognised as belonging to the scrofulous constitution, are but imperfectly understood by unprofessional people. Any injury of this kind must, I conceive, be on this account more or less imperfect. The following results are therefore submitted from my notes, without wishing to insist very strongly on their value:—In 30 per cent. of the cases examined, scrofula appears to have shown itself in one or more of the brothers and sisters; in 11 per cent. in the parents, and 9 per cent. in the grand-parents.

Urine.—I have examined the urine in thirty-two cases. The specimens brought me were generally observed to be of a pale amber colour, perfectly transparent, and deposited, on standing, only a very light filmy sediment. Occasionally, however, it was turbid from an excess of urates. The specific gravity ranged from between 1·005 and 1·030, the average being from 1·012 to 1·020. As a rule it was acid, rather

more weakly than in the healthy condition; and in two instances it was neutral to the blue litmus paper. It had often a strong odour of cod-liver oil, in cases where this medicine had been taken for any length of time. In two instances the urine appeared turbid, from containing oil-globules of some sort; at least, this was my impression on viewing under the microscope a minute portion of the densely connected cloud of this substance that appeared floating almost entirely on the surface of the liquid.

In considerably more than half of these cases—nineteen out of the thirty-two—I found, by microscopical examination, octohedral crystals of the oxalate of lime. In ten instances the oxalates were so abundant that, on placing a few drops, taken from any portion of the liquid, in a small glass cell under the microscope, and with a $\frac{1}{4}$ or $\frac{1}{2}$ inch* object-glass, the field seemed literally covered with large well-defined crystals. In the remaining nine specimens the oxalates were fewer in number, smaller, and not so easily recognised, though quite distinct when carefully examined. In two instances I failed to detect them until the urine had been concentrated by heat, when they were immediately visible in tolerably large numbers. The average specific gravity of the urine containing the oxalate of lime was from 1·018 to 1·025. In one case it was lower than I have found it in any other instance, viz. 1·005. The urine was generally of a pale, transparent, amber colour, seldom turbid. On allowing the former to stand a little time, a delicate cloud seemed to remain diffused through the liquid for a considerable time without falling to the bottom of the vessel. This appeared to me quite characteristic, and agrees, as Dr. Golding Bird has stated, from the oxalates being about the same specific gravity as the urine. Three of the specimens exhibited traces of albumen by the usual tests; and twice the crystals appeared marked by an excess of square and cylindrical-shaped crystals of uric acid, as, on the addition of a drop of liquor potassæ, the latter immediately disappeared, and brought the oxalates very beautifully into view. In no instance did there appear to be an excess of phosphates in combination with the oxalate of lime.

In the thirteen specimens in which the oxalates were not observed after the most careful examination, I find there are four in which the phosphates were in excess,—that is, the liquid was rendered distinctly turbid by heat, which turbidity was removed by nitric acid. Two were specimens of the urine obtained from persons with well-marked cases of tumid glands of considerable standing, where the exciting cause had clearly been venereal.

This class of cases must not, however, be considered as exceptional; for it is well known that the oxalate of lime has been very often noticed in connection with some of the more inveterate forms of syphilis: and one of the finest specimens I have observed occurred in a young man of 22 years of age, who presented one of the most disgusting cases of scrofulous disease, affecting the whole anterior part of the neck and axilla,—caused, no doubt, in a great measure, by the injudicious use of mercurial medicines.

Two were cases where the cervical glands had suppurated, and were rapidly cicatrizing; leaving five cases in which the lymphatic glands were in a more or less state of congestion or disorganization, in all probability from a scrofulous taint, without the urine exhibiting any traces of the oxalate of lime after the most careful examination. A question of a very interesting nature here presents itself to my mind—Has the presence of the oxalate of lime, which we have observed so frequently in the urine, or that condition of system, whatever it may be, by which it is formed, and afterwards separated by the kidneys, any connection with the origin, growth, and unusually lengthened persistence of these scrofulous tumors; or is the occurrence of this substance foreign to the healthy economy, accidental, or due to any particular system of diet, or otherwise? Before this question can be fairly met, a much more extended series of observations will be necessary than those now presented. If, for example, the oxalates should be found to appear in the urine simultaneously with the first manifestation of the swelling, and continue as long as any degree of activity is displayed in the gland, and finally disappear upon the subsidence of these tumors, a strong argument as to their mutual relationship might be esta-

blished. Without indulging in any speculations on this subject, without a sufficient number of well-attested facts to guide me, I will simply add, that it is possible that the oxalate of lime may more frequently occur in the urine in other diseases than is commonly believed; and perhaps, occasionally even, in minute quantities, in a comparatively healthy state of system.*

Treatment.—Most persons, I think, will admit that the treatment of these cases is, in the majority of instances, anything but satisfactory: hence the unfortunate sufferers too often become the prey of unprincipled charlatans and ignorant pretenders of every grade.

The difficulties with which these cases are surrounded are due to impaired nutrition of the system generally, and likewise to the anatomical structure of the organs locally affected. Unless we can succeed in arresting the deposit of tuberculous matter, which gradually destroys the gland, and with it the tissue by which it is more immediately invested (hence the burrowing of the matter in the adjoining cellular membranes causing sinuses and loss of substance very difficult to be replaced in the scrofulous habit), this disease must always remain tedious and harassing to the practitioner. Much relating to the treatment of scrofula, as affecting the lymphatic glands in their different stages of development and decay, I propose to make the subject of a more special inquiry at some future period. In the meantime I may state that, as regards internal remedies, I have found more benefit from the dilute phosphoric acid, given in some bitter infusion, such as Columbo, than any other medicine. Iodine in all its forms has, I believe, been much over-estimated; and cod-liver oil, so much vaunted of late, though of much service in scrofulous affections of the osseous tissues, has not appeared to manifest any very marked effects in this peculiar form of the disease.

I am indebted to Dr. Brett, whose contributions on the subject of urinary sediments will be familiar to the readers of this journal, for kindly examining many specimens of the urine with me, and otherwise affording me much valuable aid.

* When this was written I was unacquainted with the observations of Dr. B. Jones on this subject, recorded in the *Lancet* for Feb. 9, 1850.

I ought likewise to express my obligations to Dr. Drysdale, for throwing in my way many very interesting cases.

ON THE CONTAINING-TEXTURE OF THE BLOOD.

By WILLIAM ADDISON, M.D. F.R.S. F.L.S.

THE relations which have been established between the process of repair and inflammation, and between these and natural growth, have invested embryological researches with a new interest, especially with reference to the development, properties, and transformations of vascular tissue, or, as it may be more suggestively expressed, *the containing-texture of the blood*. In the primitive trace of growth in the embryo, sentient and motor forms (spinal cord, cerebral ganglia, and heart) are the first moulded, and visible, through a lens, in the incubated egg of the common fowl within the space of forty hours. At this early period there is neither a blood-circulation nor blood vessels. Blood is first seen, not in the embryo-body itself, but in the vascular area, where it primarily appears in irregular, and apparently unconnected red points or blotches, which, when examined with a microscope, are observed to be groups of red cells situated amongst the other cells of this embryonic appendage, and no special limiting or boundary tissue can be distinguished. As growth advances, the amount of blood increases, circulation begins, and the coats of the vessels then become recognisable as a distinct form of tissue interposed between the blood-current and the fixed and more solid portions of the structure. This constitutes the coats of the arteries, veins, and capillaries, as distinguished from the particular substances of the various organs through which blood is distributed, and is, at first, in accordance with the universal law of growth, a corpuscular, and subsequently a fibrous tissue, increasing gradually in thickness and strength, in the arteries and veins, in proportion to the increase and volume of the blood conveyed; but in the capillaries remaining, as at first, exceedingly thin and transparent.

These facts are, we believe, not disputed, and they are here briefly nar-

rated in order to state the case:—That, in original growth, forms of sentient and motor tissue precede the circulation of the blood; that the circulation of blood precedes the formation of the special coats of the blood-vessels; and that these are at first a corpuscular, and then a fibrous texture.

Experiment 1.—Having opened the shell of an egg after forty or forty-four hours' incubation,—some eggs being earlier than others,—divide the yolk membrane with a pair of scissors all round, just outside the vascular area; then sink the egg slowly in a vessel of water, and the separated portion, which includes the *vascular area*, *area pellucida*, and the *embryo*, will float at the surface, and may be removed upon a slip of glass for microscopical inspection, either as an opaque, or as a transparent object. Blood is seen in the vascular area, in apparently unconnected, irregular, pale red spots, which are larger and more numerous towards the outer circumference than towards the inner margin, where the area vasculosa bounds the area pellucida. The area pellucida is formed by a thick gelatinous sort of membrane, in the centre of which lies the embryo, with the cerebral ganglion, the two lateral visual ganglia, and the spinal cord, symmetrically moulded. The heart appears as a round transparent vesicle, outside and towards the middle of the body, and has been seen beating regularly, notwithstanding the manipulation to which the embryo and its appendages have been subjected by the removal. No appearance of blood can be detected in any part of the embryo-body: certainly there are no blood-vessels, properly so-called; nor at this time can any capillaries be seen traversing the area pellucida. The whole mass is so extremely soft and tender, that it will not bear the slightest degree of traction.

Experiment 2.—The same proceedings being adopted with an egg of from forty eight to fifty-two hours' incubation, the trunks of two large blood-vessels are seen emerging, one on each side, from beneath the body of the embryo, some distance below the heart, and one from the heart passes upwards by the head. They traverse the area pellucida, and, reaching the inner margin of the area vasculosa, divide and sub-divide; then, the smaller sub-divisions returning

to the inner margin of the area vasculosa, numerous capillaries are seen passing across the area pellucida towards the body of the embryo (figs. 1 and 2). Blood-corpuscles are now seen circling

within the heart upon each pulsation; and others are at the same time also seen moving within those portions of the trunks of the vessels which traverse the area pellucida.

FIG. 1.

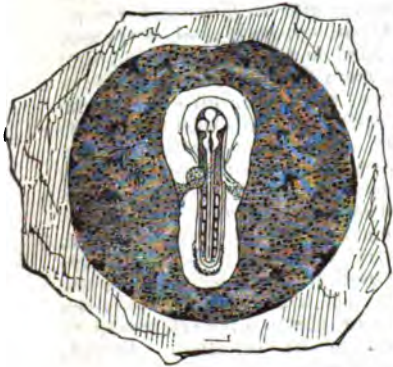


FIG. 2.

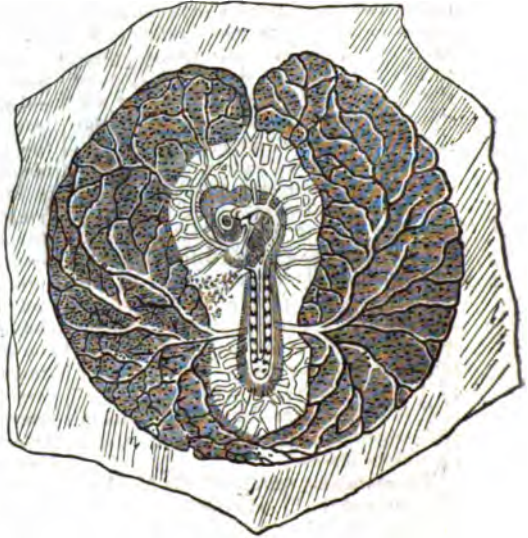


FIG. 1.—The embryo, area pellucida, and vascular area of the egg of the common fowl, after forty hours' incubation. Magnified by a lens.

FIG. 2.—The embryo, capillaries of the area pellucida, and vessels of the vascular area, after fifty hours' incubation. Magnified by a lens.

With respect to these vessels, a special blood-containing texture can scarcely yet be said to have formed, inasmuch as the blood-currents traversing the area pellucida are bounded only by the material of that membrane, which is quite transparent; and those ramifying in the area vasculosa are bounded by the material of that substance—the altered yolk-cells, and are therefore much more opaque. Moreover, the whole mass is still so soft and pulpy, that it will not bear the most gentle traction without obliteration, falling into a confused mass of cells; though it becomes more coherent after exposure to the air. This is not the character of vascular tissue. The correct anatomical description appears to be this (not only as respects the appendages of the embryo, but also as respects the embryo-body itself):—Whatever substance or texture blood first circulates in, the blood-channels are, for a brief space of time, formed or

bounded by that substance; so that, at first, no distinction can be observed between the coats of the blood-vessels and the substance lying in the intervals or spaces between them: whereas afterwards, when the body is more fully grown, and the volume of blood increased, a very important distinction arises between the containing-tissue of the blood, or the coats of the blood-vessels, and the *parenchyma*, or the particular substance of an organ. We cannot select a better illustration than the blood-vessels of the area vasculosa. The yolk of the egg consists of a very thin transparent membrane containing the yolk-cells. The germinal membrane is an altered state of the yolk membrane, and the vessels of the area vasculosa appear projecting into the yolk substance; so that, on their upper or *parietal* aspect, they are smooth and transparent, but, on their lower or *visceral* aspect, they are yellow and opaque. Three parts of their circum-

ference—that is, of their walls—are formed of cohering yolk-cells; the other part by the transparent and smooth yolk membrane.

In the increase and preservation of the human body we recognise three normal phenomena—growth, nutrition, and the process of repair; in its diseases, two prominent abnormal conditions—inflammation and scrofula. Respecting these we distinguish three things:—1, the blood; 2, the coats of the blood-vessels, or the containing texture of the blood; and 3, the particular substances of the different organs.

Of the blood we have on several previous occasions treated at length, especially with reference to its colourless elements—lymph and lymph-particles, or, as we have elsewhere more frequently termed them, protoplasm and colourless cells.* We now purpose to speak particularly of the containing-tissue of the blood—or, as we may more briefly term it, vascular tissue—with respect to the distinctive properties, reproduction, and transformations it exhibits in phenomena of repair, inflammation, and scrofulous disease.

The containing-tissue of the blood.—The coats of the blood-vessels, or vascular tissue, may be described as a sort of frame-work, in the interstices of which are deposited the various substances by which individual organs are characterised. It is these which impart the sensible qualities by which the organ is distinguished, and it is their constant and intimate admixture with the vascular tissue which prevents the special properties of the latter from being readily appreciable. In repair, inflammation, and scrofulous disease, we meet with new growth and new blood-vessels, the primary elements of which, all observers concur in stating to be forms of lymph and lymph-particles, capable of assuming, by a species of metamorphosis, a fibrous, tendinous, or osseous structure. A bone is composed not simply of bone-substance, but also of vascular tissue. Blood-vessels spread everywhere upon the bone-substance, which is full of hollow spaces and passages for their distribution. At the early periods of growth blood and vascular tissue predominate, and a bone

is therefore, at that time, soft and yielding; but afterwards the bone-substance preponderates,—yet so that a bone is never without an expanse of vascular tissue permeating its cavities. When a fracture occurs, the process of repair consists in the reproduction of bone-substance; and the agents and materials employed are blood and the vascular tissue of the injured bone.

The phenomena, for our present purpose, may be shortly stated as follows:

—Lymph is effused, forming a lymph-bed, which surrounds and envelopes the injured and irritated vessels; new blood-vessels form in the lymph, which is thereby converted into a species of lymph-fabric. This constitutes the primary form of the new vascular tissue,—a kind of granulation-structure, endowed with properties of growth and metamorphosis, and which, first changing into fibrous tissue, is finally converted into bone; the order of the transformations being here, and in all cases, lymph, vascular lymph or granulation-texture, fibrous texture, and bone.* Tendons consist of strong fibrous threads or cords, enveloped, and, as it were, bound together by vascular tissue. When a tendon has been ruptured, the process of repair appears, in all its primary stages, to be similar to that observed in bone. Effusion of lymph precedes the formation of new vascular tissue: analogously as in the embryo, the cells of the germinal membrane precede the vascular area. But here the transformations are limited to the fibrous type, not proceeding to the ulterior form of bone: and we readily perceive in this example, that whether the primary lymph-form persist, or whether the transformations exceed the natural fibrous form,—in either case the reparation of the tendon would be unnatural, and an abnormal or diseased condition would exist. The skin has a much more complex organisation than either a bone or tendon. It may be described as a web or frame-work of vascular tissue, in which are embedded, at variable depths from the surface, the particular substances of its very numerous perspiratory glands and hair-follicles. The phenomena of repair in the skin vary according to the nature and severity of the injury inflicted, or according as the before-mentioned

* MEDICAL GAZETTE, vols. i. and ii., 1840-41; Transactions of the Provincial Medical and Surgical Association, vols. xi. and xii.; and Provincial Journal.

* On Healthy and Diseased Structure, &c. 1840; also the Lectures on Repair by Mr. Paget.

organs embedded in it are more or less extensively and completely destroyed. The general results of scalds and burns, which exemplify phenomena of inflammation and repair, are well known. In the former, blood speedily accumulates, reddening the site of the injury, and phenomena of growth or nutrition are exaggerated. The cuticle is raised into a bladder filled with fibrinous serum, and lymph and lymph-particles form a new covering to the excited and distended vessels. These are succeeded by several layers of new cuticle, beneath which the vessels gradually resume their natural calibre and appearance, and the reparation is complete. In burns the injury is deeper, vascular tissue and its embedded organs being killed or destroyed. Hence a new growth of vascular tissue is required for reparation, the phenomena of which may be briefly summed up as follows:—At a certain depth from the surface vascular tissue regains its tone and properties, whereupon blood flows in increased quantity to the irritated vessels, lymph is effused, and thus a line of demarcation is drawn between the living and the dead textures. New blood-vessels form in the lymph, which thereby becomes new vascular tissue, endowed with properties of secretion and metamorphosis; and when, by the removal of the dead parts, this new vascular tissue comes into view, it is in the form of extremely red points, termed *granulations*, which bleed upon the slightest touch,—a fact indicative of the corpuscular or embryoniform condition of the coats of the new vessels. The granulations metamorphose into fibrous texture termed *cicatrix*, upon which *the cure*, in as far as natural operations are concerned, is completed. All these cases may be considered as examples typical of inflammation, repair, and cure: and it is to be observed that bone does not exude from bone, tendon from tendon, nor skin from skin; but that in each case a corpuscular growth, termed effusion of lymph, first appears. This, when permeated by blood-currents, becomes new vascular tissue, the metamorphosis of which effects the cure, whether that cure demand an osseous or a fibrous texture. Moreover, it is also to be observed,—at first, during the formation of new vascular tissue or granulations, that *the action is from the blood to the tissues*,—there is deposition,

or effusion, growth, secretion, and swelling: but subsequently, during the fibrous and osseous transformations, *the action is from the tissues to the blood*; these latter periods being marked by diminution of swelling, absorption, consolidation, a less amount of blood circulating in the part, and disappearance of a great many of the new vessels.

Respecting the relations subsisting between the lymph of repair and the lymph of the blood,—that is to say, between the colourless granular cells and protoplasm of new vascular tissue, and the colourless granular cells and protoplasm of the blood,—the following facts, which have elsewhere been discussed, may here be recapitulated:—Colourless cells are found in abundance in blood taken from vessels administering to repair or inflammation, and a delicate web of fibrous tissue may be seen with a microscope to form in the protoplasm or liquor sanguinis. The blood of the early embryo, when vascular tissue is in active growth, is scarcely more than lymph: the vessels of granulations are formed of lymph, and bleed upon the mere touch; and lymph and lymph particles adhere to the coats of the vessels upon irritating them. Moreover, when blood is withdrawn by venesection from a person labouring under an inflammatory disease, lymph separates quickly in a thick layer at the surface. This metamorphosis into a fibrous texture, the microscopical elements and physical properties of which appear to be of the same nature with these constituting the containing-tissue of the blood, or the coats of the blood-vessels, in the living body; allowance being made for the difference and disadvantage of the circumstances under which it is formed.

Lymph, lymph particles, fibrous tissue, and serous fluid, are thus demonstrably phases of blood elements, and the same elements are, more or less, components of vascular tissue. From these and other facts, which it is unnecessary here to recapitulate, we have derived the conclusion that *blood forms its own containing texture*; lymph, vascular lymph structures, and granulations, being the primary or proximate,—and fibrous, tendinous, and osseous textures, the more remote phases of the colourless elements of blood. Degraded lymph, and degraded forms of lymph texture, are termed pus, clots of pus, and tubercle.

or tubercular infiltration; persistent granulations are termed fungosities, fungous growth, or, in popular language, "proud flesh:" and it is these depraved and unnaturally persistent forms, together with the adventitious fibrous, tendinous, and osseous formations, arising from the metamorphosis of lymph and new vascular tissue, which constitute the diseased products of inflammation and scrofula. Ossification appears in the coats of the arteries, and *phlebolites* attached to the interior of the veins; inflamed mucous membranes discharge lymph and pus; and inflamed serous membranes assume the aspect of mucous textures, and then their surfaces become united, sometimes by soft lymph-structure, and sometimes by a dense fibrous tissue.* It does not seem necessary, in support of the conclusion we have drawn, to refer more largely to the organization of blood, or to the forms which its elements are capable of assuming, especially as numerous interesting facts may be found brought together in the lectures of Dr G. Burrows, from which we have just quoted. We therefore proceed to the other topic of our inquiry—the limitations of repair; for these appear to indicate the limits of the organization of blood, and explain the common characters of the products of inflammation.

It is well known that the extent and facility with which extreme injuries are repaired is directly as the age of the individual; so that in many of the lower classes of animals a whole limb may be removed at a very early period of life,

and be afterwards reproduced,—a phenomenon that does not happen when the full period of growth is more nearly completed. Now, in order to avoid difficult metaphysical questions, we may conveniently express the properties of growth in the embryo, when all the organs are evolving and expanding, by the term "*germ power*;" and say that, the younger the individual, the greater the proportion of unexpended or unappropriated germ power:† so that, until the maturity of growth, and in a ratio inversely to the age, there will always be the unexpended portion of the germ power co-operating with the properties of blood and vascular tissue in reparative actions; whereas, when the full maturity of growth has been attained, the reparation of an injury falls wholly within what, by analogy of language, we may call the *blood-power*:—that is to say, in a very young animal, specific forms not having been completed, there is a greater capacity of reparation than in an older individual, where these have been concluded. In the former example, growth is mingled with repair; in the latter, we have repair only.

If the scar or cicatrix of an extensive and deep burn be examined, it will be found that perspiratory pores and hairs are either very deficient or entirely absent in it; from which it would appear that the particular substance of the hair-follicles, and of the glandulæ of the skin, is not reproduced by the process of repair which has healed the wound.‡ In other cases, too, it appears that muscular fibrillæ, brain substance, liver substance, and the parenchyma of other organs, are not reproducible by the ordinary process of repair—the metamorphosis of lymph. Moreover, inflammation in the brain, in a muscle, in the liver, or in the kidney, does not issue in hypertrophy or increased natural growth of the particular substance of either of these organs, but in forms identical with those which characterise the process of repair—viz., lymph, new vascular tissue, lymph structures, granulations, and pus—and the metamorphosed forms of these—fibrous, tendinous, and osseous forms.

* Dr. G. Burrows relates an instance in which *phlebolites*, three in number, were examined by him and Mr. Stanley: two were of the size of almonds, and, upon being divided, exhibited several concentric layers of *fibrine*; in the other there was the same kind of structure, with osseous plates in the investing membrane. One of these bodies was attached to the lining membrane of the vein by *fibrous cords*; and a firm *fibrous band* connected this *phlebolite* with the others. Tiedemann, Otto, Lobstein, Cloquet, and Caruwell, all agree that at first *phlebolites* consist of a small coagulum of blood, in the interior of which the *fibrine* becomes pale and concrete; then assumes an *osseous appearance*; and this goes on, little by little, and layer after layer, towards the circumference. Dr. Reid, of Edinburgh, met with five instances of *phlebolites* within twelve months; they varied in number from two to a dozen; and in size, from a millet seed to that of a large pea. They were loosely attached to the coat of the vein, and nearly all of them were of a *stony hardness*. Two of these bodies were analysed, and found to consist of phosphate of lime, carbonate of lime, and animal matter, in proportions similar to those existing in *bone*. (*Pathological Observations on the Blood*: MEDICAL GAZETTE, Vol. xviii. 1834.—Edinburgh Medical and Surgical Journal, Vol. xliii. 1835).

† Vide Mr. Paget's Lectures on the Process of Repair: MEDICAL GAZETTE, 1849.

‡ Since this was written, the facts have been substantiated by Mr. Gray, who, in a paper published in the *Lancet*, speaking of the microscopical examination of the cicatrices of burns, states that "no hairs were observed covering the surface of these cicatrices, nor could the existence of sebaceous glands or perspiratory tubes be detected."

What are the inferences to be drawn from these facts? Simply, it appears to us, these: that phenomena of repair by the metamorphosis of lymph, and phenomena of inflammation, are both expressions, as it were, of exaggerated reciprocal action between blood and its containing-texture, and must be investigated apart from the properties of the particular substances prevailing in different organs; that the particular substances of different organs which are beyond the resources of repair, and which do not appear as results of inflammation, are not phases of blood-elements,—they are sustained and nourished by blood, but not derived from it:—whereas those elements and structures which do appear during the process of repair, or in the course of inflammation, are phases of blood-elements. These conclusions appear to derive additional confirmation from further embryological researches: for if the skin or integument of the early human embryo, before it has become vascular, be microscopically examined, cells of a peculiar character are seen grouped together, and disseminated through it in spots. These, from their symmetrical distribution, and the conformable spaces intervening, appear to be the foundations, or “*the germs*,” of what afterwards become follicles and glandulae. And as, in the most essential part of the embryo, the governing form (spinal cord and brain) is moulded before the circulation of blood, or the existence of vascular tissue,—so it appears in the skin that the particular substance of its hair-follicles and perspiratory glandulae is likewise established before the circulation of blood; and it might, *a priori*, have been concluded, that those substances, the germs of which exist prior to, or which are founded independently of blood and vascular tissue, would not be reparable or reproducible by the metamorphosis of the elements of blood, however necessary blood, in its aggregate capacity, may prove to be for their sustenance and growth.

[To be continued.]

MEDICAL APPOINTMENT—THE NORFOLK AND NORWICH HOSPITAL.

DR. RANKING has been elected Physician to the Norfolk and Norwich Hospital. The other candidates were Dr. Copeman and Dr. Wharton, the latter of whom retired early in the contest. The numbers polled were—Dr. Ranking, 364; Dr. Copeman, 236.

OR

SCARLATINAL VAGINITIS.

BY JOHN ROSE CORMACK, M.D., F.R.S.E.

Physician to Putney College; Fellow of the Royal College of Physicians of Edinburgh, and formerly Physician to the Edinburgh Royal Infirmary.

In 1844 I published in the *Edinburgh Monthly Journal* a memoir, entitled “Gonorrhoea and Syphilis with reference to Forensic Medicine and Therapeutics,” in which I directed attention to various topics, including non-venereal vaginitis. Since that period, several cases in my own practice, and others which have been mentioned to me by professional friends, have convinced me that exposure to cold, and other causes enumerated in the memoir referred to, are even more frequently the sources of inflammation and purulent discharges from the vagina than I supposed at the period of its publication.

At present, I only wish to say a very few words with reference to a paper which appeared in the *MEDICAL GAZETTE*, July 12, p. 65, by Dr. Robert Barnes, entitled “*On the occurrence of a muco-purulent discharge from the vagina in Scarlatina, and the importance of this symptom in relation to Forensic Medicine.*” Dr. Barnes attaches a much greater amount of importance to such cases than I was aware they possessed. He considers them very uncommon; and in support of this doctrine he cites two eminent authorities, Dr. Tweedie and Dr. Miller. I do not write in a controversial spirit, but simply with the view of inducing others for the future to inquire into the presence or absence of vaginitis in scarlatina. Dr. Barnes says—“I am quite prepared to accede to the opinion of Dr. Tweedie and Dr. Miller, that a muco-purulent discharge from the vagina in scarlatina is a very rare occurrence; but this very circumstance increases the importance of the symptom in relation to questions of legal medicine.”

During the last epidemic (1848-49) of scarlatina,* I had, under favourable circumstances, considerable, though not a very large, experience of the disease; and of all my cases I have preserved a

* Much information regarding this epidemic will be found in Dr. Miller's admirable monograph, lately published.

record. Vaginitis I frequently met with, and I regarded it in no way as an unexpected or rare occurrence; but, on the contrary, as a not unlooked-for extension of the *exanthematous* inflammation of the skin, analogous in its nature to what is often met with in the mucous linings of the nose, ear, air passages, and intestinal canal. In the epidemic of 1848-9 I had under my charge twenty-three female patients, all of whom were cleanly, well nursed, and in a respectable social position. In twelve of the whole number there was well-marked vaginitis; and so impressed was I with the importance of averting or preventing this affection, that in every female patient I directed, from the very first, careful ablutions of the parts to be performed at least twice in twenty-four hours. Of the twenty-three female patients, two only were above 14 years, and these were respectively 26 and 28, and both married. Now both of these patients had *acute* vaginitis, much more severe than any of the children. In one lady, for forty-eight hours the discharge was so abundant as to require the nurse to change the towels at least every hour, and it was of so acrid a nature as to excoriate the thighs and anus, notwithstanding every precaution being taken to protect these parts. The other had it more mildly; but the vaginitis was also in her a source of great suffering and discomfort. The first lady aborted: the second was not in the family way. The first was the most dreadful case of scarlatina which I have ever seen issue in perfect recovery: the second was one of moderate severity.

Speaking, as I now do, from a very limited number of facts, I have no right to give an absolute opinion as to the frequency or rarity of scarlatinal vaginitis; but I would venture to suggest that the non-observance of this affection by the practitioner is no proof of its absence; for patients suffering from scarlet fever are often too ill to make complaints: and in other cases the affection is managed by the nurse, without her thinking it necessary to trouble the doctor. The question must be decided by future experience of a large number of cases, carefully observed with a special view to its elucidation.

The whole treatment required is generally frequent ablution with tepid water, and, if pain be complained of,

the use of opiate fomentations. The children, with one exception, required very little special treatment; but one, of strumous habit, continued to have a profuse muco-purulent discharge long after convalescence was in all other respects complete. Chalybeate medicines, cod-liver oil, and astringent lotions, at last effected a cure. The two adults were both treated by injections of nitrate of silver, and the keeping apart the labia by a piece of lint soaked in the same. I believe in severe cases the nitrate of silver will be found the best local remedy: but the injury which it occasions to the linen of the patient, and of her bed, naturally occasions a prejudice against its use.

Essex House, Putney,
18th July, 1850.

HOMŒOPATHY IN NEW YORK.

AMONG the transactions of the New York State Homœopathic Convention, at their late meeting in Albany, was the adoption of a Constitution, which is to govern them, when in and out of convale. The preamble is as follows:—"We, the subscribers, physicians, residing in the State of New York, *believing* the law propounded by Hahnemann, '*similia similibus curentur*,' to be a fundamental truth in medicine, do agree to the following constitution" &c.—*Adapted without amendment.* We shall expect great doings in the Empire State among these innovators of medical science. Their flag is raised, and is given to the breeze; and the fact of their organization, it is supposed, will be wafted to the four parts of the globe. If any one can believe the mere doing of nothing to be a science, we have certainly nothing to say to such, farther than to advise the reading of Hahnemann on the peculiar and active properties of gold in certain diseases. He says, "after triturating one grain of gold with ninety-nine grains of sugar of milk, and continuing this trituration up to the *twelfth potency*, the remedial virtues of gold become roused to such an extent, that a man who was suffering with horrible melancholy, and who was impelled by intolerable anguish to take his life, needs but to *smell for a few moments* of such a preparation of gold, in order to recover his cheerfulness and love of life!"—*Boston Medical Journal*, July.

MEDICAL GAZETTE.

FRIDAY, AUGUST 2, 1850.

THE "Compensation in Railway Accidents" Bill, of the provisions of which we gave a short notice at p. 1117 of our last volume, was lost on the second reading by a very small majority of 2, in a house of 108 members. It was, of course, opposed by gentlemen connected with the railway interest for reasons which require no specification, and by the Attorney-General upon legal grounds.

We have already, at various times, fully considered the circumstances which led to the introduction of this bill. Our readers know, or ought to know, that, in the case of *Cox v. THE MIDLAND RAILWAY COMPANY*, it was decided by a majority of the judges that no action for the recovery of reasonable expenses or charges was maintainable by a medical man against a Railway Company whose servant had called him to attend a person who had sustained an injury. The bill proposed to alter the law, by giving a right of action against the Company to medical practitioners thus summoned to accidents by the Company's servants; and, to guard against injustice to a Company by the ignorance or officiousness of servants, the right of action was expressly limited to those cases only in which the accident arose from the negligence of the Company.

Mr. LABOUCHERE opposed the measure, because "it would provide that in all cases of accident the Company should be bound to call in medical aid, and be in the first instance responsible." This he described as a total departure from the law of the land. To this it may be replied, that every new Act of Parliament is a departure from the law of the land; the object of good legislation is to alter, amend, or suggest remedies for evils for which the current state of the

law does not provide: but the objection of the learned member may be tested by a case.

Through the negligence of a guard, a train is set in motion before the passengers have received fair warning. One is knocked down while stepping into a carriage, and he sustains a compound fracture of the thigh. There is no medical practitioner within five miles, and assistance is urgently required. Under the present state of the law the Company are not bound to call in medical aid, and, after the decision given in Mr. Cox's case, their servants will of course decline interfering for the benefit of the sufferer. If such an accident happened to any one of the 55 members who voted against the bill, it would no doubt be in his power to procure medical assistance at his own cost, and on his own responsibility, in the first instance. On his recovery he would claim compensation for his leg, or bring an action against the Company, and retain the best counsel,—say the learned Attorney-General, at a fifty-guinea fee,—to plead his case. On the assumption that there had been negligence, there would be a verdict with heavy damages, of which "the surgeon's bill would form an essential part."

But the victim of railway negligence may be a pauper; and, before considering the results in this case, we will quote the objections of the learned Attorney-General to the proposed bill. He said—

"he did not consider that the mere fact that conflicting judgments had been delivered could justify the house in adopting the principle of the bill. *He thought that medical men would willingly, on being sent for, render their services to a sufferer, even though they had no power of recovering payment from the Company, being assured he would pay them if he were able; and if he were unable to do so, having the certainty of remuneration, if he were an honest man, on his recovering damages, of which the surgeon's bill always formed an essential part.*"

It is pretty well known that the fees of barristers do not depend on any such series of contingencies as the learned gentleman would throw in the way of *medical remuneration* in railway accidents. The Attorney-General supposes that a hard-worked country practitioner would willingly leave his home and practice, and make a journey of five or ten miles, to take on himself the responsibility of attending a pauper seriously injured by a railway accident, with the full knowledge that the Company repudiated all liability, that the man was a beggar, and that the only chance of procuring payment for a heavy sacrifice of time and practice would be the "honesty" of the patient, and his recovering damages, by suing, *in forma pauperis*, a powerful Company, who would probably retain the learned Attorney-General himself, as well as other eminent barristers, for the express purpose of defeating his claim! The pauper might turn out an honest man, and might recover damages, in spite of the eloquence of the learned gentleman; but, on the other hand, the Attorney-General exerting his ingenuity for the Company might, notwithstanding the evidence adduced for the plaintiff, induce a jury to believe that the negligence was on the part of the man and not of the Company! In this case the remuneration for medical services rendered to the sufferer would be *nil*. Then, again, the pauper might die during the treatment. His widow might bring her action under Lord Campbell's act, and, under the same series of contingencies, she might or might not recover damages; and if recovered, she might or might not remunerate the medical man who had used his best endeavours to save the life of her husband. It is very true that the surgeon's bill always forms an essential part of the damages, but the party must obtain the damages before the bill could be paid; and the Attorney-

General might perchance be the advocate who would defeat the claim. The pauper may die, and not leave a survivor to make a claim: in this case, also, the remuneration would be *nil*!

As the law therefore stands, by the rejection of this bill, it is to be inferred that in railway accidents a medical man must be prepared to render his services gratuitously, at whatever cost or risk to his ordinary practice, in all cases except where *the party sustaining the accident is capable of paying the charges*.

If it be the case of a destitute person, the surgeon's chance of remuneration, in the event of his patient's recovery, depends—1. on clear legal proof of negligence on the part of a wealthy company sued by a pauper; 2. on the honesty of the plaintiff if he succeeds in the action; and, in the event of his death, 3. on his leaving a surviving relative who will succeed in an action against the Company, and be honest enough to discharge his bill.

The remuneration of gentlemen in the profession of the learned Attorney-General does not depend upon any such contingencies as these, and we are at a loss to know why they should be thus thrust on the members of another profession. As the law stands, medical men must often be called upon to accept them when accidents occur to the poor and friendless, or they must incur the odium of acting with unkindness and inhumanity to a fellow creature.

Mr. NEWDEGATE, in introducing the bill, related the following case, which had occurred on the Yorkshire and Lancashire Rne. "A dreadful accident had taken place, by which a man had his leg cut clean off, and the Railway people declined to send for medical aid, offering, however, to send him on to Bolton for admission into the infirmary; but when he arrived there he was dead, and so apprehensive were the railway parties of incurring liability, that the

poor man's leg was left by the road-side. Was such a state of things consistent with common humanity?"

It is possible that the measure which has been so recently introduced and rejected, may not apply the best remedy of which the case admits; but it is clear that, when under a railway accident to a poor person, a medical man must either abandon his other patients to give gratuitous attendance, or have imputations thrown on his character for humanity,—such a state of things requires to be immediately remedied.

Reviews.

On the Use and Abuse of Alcoholic Liquors, in Health and Disease. Prize Essay, by WILLIAM B CARPENTER, M.D., F.R.S., F.G.S., &c. &c., 8vo. pp. 283. London: Gilpin; and Churchill, 1850.

A PRIZE of one hundred guineas having been offered "for the best Essay on the use of Alcoholic Liquors in health and disease," the award was made to Dr. Carpenter's Essay.

The questions proposed were:

1st. What are the effects, corporeal and mental, of alcoholic liquors on the healthy human system?

2nd. Does physiology or experience teach us that alcoholic liquors should form part of the ordinary sustenance of Man, particularly under circumstances of exposure to severe labour or to extremes of temperature? Or, on the other hand, is there reason to believe that such use of them is not sanctioned by the principles of science, or the results of practical observation?

3rd. Are there any special modifications of the bodily or mental condition of Man, short of actual disease, in which the occasional or habitual use of alcoholic liquors may be necessary or beneficial?

4th. Is the employment of alcoholic liquors necessary in the practice of medicine? If so, in what diseases, or in what forms and stages of disease, is the use of them necessary or beneficial?

We shall endeavour to show our

readers the mode in which Dr. Carpenter has dealt with these questions, which embrace every point from which the subject of *teetotalism* can be viewed. It appears to us that Dr. Carpenter's answers are not so conclusive as he supposes.

We should not have thought it necessary to bestow much space upon this production, if the author, a well-known writer on physiology, had not assumed that a special obligation is laid upon the members of the medical profession to advocate a total-abstinence from the use of alcoholic liquors. We dissent altogether from this proposition. The duty of the medical adviser is to warn others against the abuse thereof,—and abuse, we may observe, does not consist alone in excessive indulgence, but also in the total disuse of this as of any other dietetic or prophylactic agent; and in using the latter term we claim for it a far wider meaning than the author assigns to it, in discussing the subject now before us.

Dr. Carpenter considers that the argument drawn from the abuse instead of the use of a thing, is completely defeated in reference to total abstinence by the late Archdeacon Jefferys, of Bombay. The theologian, seeing that the abuse of alcoholic liquors is the source of much crime and misery, makes use of the words of St. Paul; "It is good neither to eat flesh nor to drink wine, nor anything whereby thy brother stumbleth, or is offended, or is made weak;"—and thence proceeds to argue that this text is perfectly conclusive against even the most moderate use of alcoholic liquors. But that such is not in any degree true may be inferred from the same teacher's advice to Timothy in another passage: "*Drink no longer water, but use a little wine, for thy stomach's sake and thine oft infirmities,*" (1 Tim. v. 23). Here the moderate use of wine, it may be said, is positively enjoined. Moreover, upon this rendering of the text, the author should join the vegetarians, and henceforth forswear "flesh." Considering, therefore, that Dr. Carpenter has in no degree removed the objection—that the abuse of a thing good in itself, does not afford a valid argument against the right use of it—we proceed to the examination of his answers to the proposed questions.

1. The first answer consists of an

examination of the physical and chemical effects of *alcohol* on the animal tissues and fluids. With these our readers have no doubt become familiarized, in their attempts to preserve pathological specimens; but its action on the mucous membrane of the stomach of *living* animals yet remains to be accurately observed. With the effects of *alcohol* on the living structures without the body, our readers are equally familiar in the experiments of pathologists on the nature of inflammation.

The phenomena of alcoholic *intoxication* have been so often described in books, that we scarcely required Dr. Carpenter's fourteen pages of quotations to repeat the oft-told tale. In Dr. Carpenter's enumeration of the remote consequences of the *excessive use* of alcoholic liquors, we find the usual catalogue of diseases known to all clinical students. Dr. Carpenter borrows largely from the reports of officers in the Indian service, on the fatal effects of intemperance in tropical climates.

So far, then, as answer No. 1 is concerned, we discover nothing that was not known before, with regard to the *abuse* of alcoholic liquors, and it is on their *abuse* only that the author argues in this chapter. It will be observed that the word *alcohol* is almost universally employed by Dr. Carpenter. Now we need scarcely remark, that pure and undiluted alcohol never enters the system as a beverage. Even the most inveterate drunkards do not get beyond proof spirit. These, too, are exceptional cases, and cannot therefore be reasoned upon in the general argument. Dr. Carpenter, we may add, states no experimental researches in support of the chemical views he borrows from Percy, Liebig, and others.

2. The second answer founds its negative conclusion on the "indubitable fact in organic chemistry, that there is not the slightest relation of composition between *alcohol* and muscular tissue; and all our present knowledge of the subject tends to prove, that the albuminous matters of the blood, which constitute the pabulum of that tissue, cannot be generated within the body of man, or of any other animal, but are derived immediately from the food." Therefore, argues Dr. Carpenter, nothing can afford strength, or aid the nutrition of the body, except it contain albumen. But we think there are

other forces, besides the chemical, which perform an important part in the nutrition of animal frames. We have heard of a certain condition of organization, called *vitality*, which has usually been supposed to exert some influence over chemical actions going on in the living body. Moreover, we do not forget that the human frame does not consist solely of, and depend solely upon, the muscular system for its existence—there is a system closely connected therewith, and this is the nervous system. Dr. Carpenter has not disproved the beneficial effects of moderate doses of alcoholic liquors on this system.

Dr. Carpenter observes in reference to the use of alcohol in sustaining the vital powers:—"It may be safely affirmed that the introduction of alcohol into the blood cannot stand in the place of the oxygen which is essential to the functional activity of the nervous and muscular systems." Undoubtedly this may be very *safely* affirmed, for no one can dispute the statement. It appears to have as little bearing on the argument, as the assertion that the vapour of chloroform cannot supersede atmospheric air in the lungs.

It may suit Dr. Carpenter's purpose to throw overboard the "universal experience of former generations," by remarking, that it "might be quoted in favour of a multitude of absurd notions which we now treat simply as ridiculous." But this remark does not, in the present case, dispose of *universal experience*, the inference from which, we assert, remains still untouched by the instances of bodily effort put forth by "temperance" men, quoted by the author from various writers.

Dr. Carpenter argues, from the facts of organic chemistry, that *alcohol* cannot either enable the human body to endure cold or contribute towards the maintenance of its heat; but "universal experience" refutes the speculation; and we may observe, that experiments which Liebig performed directly with reference to these points led him to the very opposite conclusions. In a previous portion of his essay Dr. Carpenter relied much on Liebig's researches; but here his observations are laid aside. This *ch* further enumeration of facts of the *exce* liquors on variou

8. In his third answer, however, Dr. Carpenter admits that there are cases,—such as those “in which there is a demand for some extraordinary exertion of the animal powers, and in which the occurrence of subsequent depression may not be an adequate objection to the employment of a stimulus that enables the system to meet it”—in which the effect of the stimulus is merely to keep the functions “up to par.” In the same chapter the author admits that the moderate use of alcoholic liquors is very beneficial in contributing to the endurance of bodily labour under circumstances peculiarly trying, and under the disadvantage of a deficient allowance of animal food. Where there is a deficiency of constitutional vigour, also, “the author is by no means disposed to deny, that after all other practicable means have been taken for the invigoration of the system, the habitual use of a small or moderate quantity of alcoholic liquors may be found beneficial in some individuals.” The author again, at the conclusion of this chapter, argues against the use of alcoholic liquors from their abuse. Because in pregnancy, lactation, old age, &c. their use has been abused, no sound argument, we reiterate, can be drawn for their total disuse. In the existing state of society, the exceptions here admitted must embrace sufficient numbers to invalidate the rule.

The fourth answer enters into the consideration of the use of alcoholic liquors as medicinal agents. With these the majority of our readers are experimentally acquainted; and, as we discover no new information in Dr. Carpenter's remarks, we may here close our examination of the author's answers to four plain questions.

We confess that we regard these answers, at best, as unsatisfactory if not evasive. They deal throughout with the *abuse* of alcoholic liquors, the evils of which have long been too well known. In many instances throughout the work, the word *alcohol* is used for *alcoholic liquors*, whereby the non-professional reader is misled as to the results. Lastly, the author himself admits sufficient exceptions to invalidate the arguments in support of *total abstinence*.

We expected, from the author's reputation, to have met with a new series of experiments in support of his views;

but we find nothing more than the old arguments drawn from old facts—whence the sole legitimate deductions are, that temperance is wisdom, teetotalism a delusion, and the abuse of a thing good in itself is no valid argument against its proper use. Therefore, under the conviction of the accuracy of these conclusions, we arrive at the inference, that it is not the duty of medical men to advocate total abstinence.

In conclusion, we cannot think that *teetotalism* has very strong claims upon the attention of any class of society, if the treatise before us be the best among those which were presented in competition for this hundred-guinea prize. It is clear, from Dr. Carpenter's essay, that teetotalism has not much to be said in its favour, when its professed advocates virtually abandon its pretensions, by failing signally, on a special occasion, to adduce either facts or arguments in its favour.

We may commend this essay, nevertheless, as containing much useful information, and many powerful warnings to those who have not the strength of mind to be “temperate in all things.”

—
On the Diseases of Menstruation, and Ovarian Inflammation, in connection with Sterility, Pelvic Tumors, and Affections of the Womb. By EDWARD JOHN TILT, M.D., &c. 8vo. pp. 250. London: Churchill. 1850.

THE principal diseases (derangements?) of menstruation, as enumerated by Dr. Tilt, are—amenorrhœa, or suppressed menstruation; dysmenorrhœa, or painful menstruation; menorrhagia, or profuse menstruation; leucorrhœa, or vicarious discharges; and hysteria. These names, Dr. Tilt is of opinion, have been very vaguely used, since, among their causes, various contradictory conditions may be mentioned, and are pointed out by the author, whose object, however, is not to treat of the several organic diseases by which they are frequently produced. The work before us is confined to the consideration of inflammation of the ovaries and oviducts. From this limitation it is evident that the author has adopted the ovular theory of menstruation proposed by Dr. Robert Lee and Dr. Power. This is made still clearer by the author's definition of menstruation, which is—“A sero-sanguinolent secretion propelled by an ova-

rian influence from all or different parts of the generative intestine, and principally from the womb." The author has not, however, lost sight of various facts which are strongly opposed to the theory.

Dr. Tilt enumerates the British and foreign writers who have furnished materials for a history of ovaritis; "but it has been of acute ovaritis," the author remarks, "that these have treated, while the sub-acute variety, judging by the inflammatory lesions found in the ovaries and their serous covering, is of very common occurrence." (p. 15.) It is more especially to give due prominence to this form of ovaritis, with its consequences, that the author has published his views, for which so far he claims originality. But we will here quote his words:—

"My aim has been to perform for the ovaries, the principal organs of menstruation, what has been successfully done for other organs by many eminent men; and I feel assured that, although some of my deductions may be contested, my practice will be admitted by all to be indubitably safe, and necessarily destined to diminish the number and intensity of female complaints.

"I can lay claim, *unfortunately*, to no discoveries; but from an acquaintance with the literature of that branch of the profession to which I have devoted my chief attention, I feel justified in affirming, that in no other work will the reader find so complete an account of the various ways in which sterility is produced by the action of inflammation in the ovarian tissues, of the great importance of ovarian peritonitis as a cause of disordered menstruation, or of the influence of ovarian inflammation in the production of uterine disease,—facts forcibly exemplified, and proved to be not mere conventional possibilities, but events of frequent occurrence."

Such, then, are the principal points under consideration by Dr. Tilt; and we now proceed to show the author's manner of expounding his information on these topics.

Subacute ovaritis is defined by Dr. Tilt thus:—"Swelling of the ovary, with increase of heat and pain upon pressure, accompanied by intermittent or permanent pain or uneasiness in the ovarian region, radiating to the loins and thighs, and producing, according to the constitution of the patient, an arrest of menstruation or its profuse flow, intense local pain or hysterical symptoms." (p. 89.)

"By *subacute* inflammation, as distinguished from *acute*, we do not so much imply a difference in the intrinsic nature of the morbid phenomena, as a limitation of the inflammatory action to certain distinct parts of the ovaries, as the ovarian follicle, and to portions of the ovarian tissue so small that they will give rise to no swelling and to little febrile action."

Among the predisposing causes of ovaritis, Dr. Tilt places the anatomical relations of the organs, constitution, excessive sexual intercourse, and the impulse of unsatisfied desire. Among its exciting causes are enumerated falls on the feet, knees, or sacrum, violent horse exercise immediately after menstruation, instrumental labour, adhesion of the placenta, styptic and other injections into the cavity of the uterus, retention or suppression of the catamenia, cold, peculiar condition of the nervous system at the time of menstruation, extension of inflammation from uterus to ovaries by the Fallopian tubes, cauterization of the cervix uteri, and the employment of certain mechanical contrivances.

The symptoms, as described by Dr. Tilt, are those of a local and of a general character: these are often referred to uterine disease or to inflammation of the bowels. Dr. Tilt distinguishes also special symptoms, which he distributes under the several types—amenorrhœal, dysmenorrhœal, menorrhagic, and hysterical.

The terminations of sub-acute ovaritis, according to Dr. Tilt, are sterility and uterine inflammation. Sterility is in these cases produced by—1, the accelerating the shedding of imperfectly developed ova; 2, by the retention of blighted ova in the ovary; 3, by placing mechanical impediments to their transmission from the ovaries to the uterus. The transmission of the ovarian inflammation produces congestion and hardness of the womb, with consequent retroversion, and perhaps peritoneal inflammation. Cases of this kind are related in illustration by Dr. Tilt. One case, at page 116, is deserving of notice, as containing a candid record of erroneous diagnosis on the part of the author. Practical medicine would make substantial progress if its professors would more frequently record their errors; and certainly in no department would the benefit be so great as in that of uterine pathology, in which we hear

now-a-days of a thousand successful cases without a single reverse!

On the subject of *treatment*, Dr. Tilt remarks that it must be observed as a fundamental point of practice that the treatment must be conducted in the intervals of the menstrual periods. With regard to depletion, which the author enjoins to be of a local character, he remarks:—

“Whether, in these cases, any particular advantage follows the application of leeches to the os uteri, or its scarification, we very much doubt, on account of the uncertainty of the results; we think that, even if it did afford sometimes any slight relief by the immediate depletion of the uterine vessels, such an advantage would be purchased at the risk of uselessly offending the patient's feelings by the untimely interference of a surgeon, by whom the application of leeches must generally be made.”

The author also states other objections to this mode of depletion, as also to that of leeching the rectum, as practised by Dr. Rigby. We feel thankful to every writer who lends his help to stay the current of professional indifference, or worse, which at present treats female delicacy with utter disregard.

We commend the author's remarks on the treatment of subacute ovaritis as judicious, and (with the exception of the recommendation of the medicated pessary) as being free from the prevalent vice of too active manipulation on the female sexual organs.

Acute ovaritis forms the subject of the concluding portion of Dr. Tilt's treatise, and presents a full account of that form of the disease in its different varieties. The several modes of evacuating the pus of acute ovarian tumors are also related. *Acute ovaritis* is, however, not so much the object of Dr. Tilt's work as *subacute*;—to point out the relation of this to derangements of menstruation has been his direct aim. In so doing the author has assumed, as we have before observed, the ovular theory of menstruation as his basis. Whether this be universally received as correct, or hereafter rejected as pure hypothesis, in accordance with some facts mentioned by Dr. Tilt, as well as other considerations, the practical result will remain, that a close functional relationship exists between the uterus and ovaries. That a close relationship also exists in disease has, we think, been here shown by Dr.

Tilt; and while we fully appreciate the value of the information contained in his work, we would remark that, in our opinion, he has scarcely meted out a fair share of praise to the previous researches of British pathologists.

Instinct and Reason: deduced from Electro-biology. By ALFRED SMEE, F.R.S. 8vo., pp. 320. London: Benham. 1850.

THE inconclusiveness of Mr. Smees' electro-biological theory was shown in our notice of his “Elements of Electro-biology,” at p. 590 of our last volume; it is therefore unnecessary now to occupy our space with any remarks upon that part of the present work. We shall simply enumerate the contents of the present volume, premising that the author explains all the instances of instinct and reason which he adduces, by assuming as their causes the occurrence of certain voltaic changes in the nervous system.

The first chapter contains general observations on the “relation of mind to life,” or on the common phenomena by which organized beings are distinguished from inorganic substances. In this chapter we find mention of the *œmæ* of Messrs. Croese and Weekes, on which Mr. Smees observes—“I am of opinion that the facts should be neither believed nor disbelieved, but kept in abeyance for fresh experiments.” For ourselves, we thought that this “act of creation” had been disposed of some years ago in an article published in the *Edinburgh Review*, and which has been attributed to Professor Sedgwick.

In the next chapter we have a description of the organs of sense, with their degree of perfection in different animals, and Mr. Smees's invention of the Optometer. The author observes that he “never remembers to have seen, amongst the thousands of books now published, any work upon the science of odours.” We may refer Mr. Smees, for a short notice of this “science,” to a book of travels, very genuine and authentic doubtless, lately published, and entitled “*Kaloolah*,” in which the author, an American surgeon, having arrived at one of the kingdoms of Central Africa, finds people's noses cultivated with all the care for the appreciation of odours, with which the ear is cultivated among ourselves for the en-

joyment of musical sounds. Mr. Smee might insert this *fact* in his next edition.

Among the author's illustrations of the physiology of the organs of sensation we find the following:—

"The estimate of heat and cold is highly important, and man possesses this feeling to a great extent. Whilst I am writing, a blazing fire is casting its radiant heat upon my body, and comforts it; but, were I to cross my threshold, a cold, biting north-east wind would chill my skin and make me sensible of the change of temperature."

Again:—

"The skin is endowed with nerves for the purpose of estimating heat and cold. In this respect the human species is not singular; for, as far as I can observe, all other creatures are sensible to heat and cold: and, whilst I write, my cat is lying before the fire, and the dog is standing with his feet on the fender, the better to warm his chest." (p. 36.)

The third chapter expounds the existence of pain, and its uses.

Memory forms the subject of the fourth chapter, in which instances of its manifestation in the lower animals are related. We may quote the following instance of its exercise in man:—

"I was once at an important meeting where no reporter was present, and it was desirable for a report to appear. Upon application two or three days afterwards, I wrote out such of the speeches as were required, in such a manner that the substance was so correctly given that no person found out that his very words had not been taken down in the room by a shorthand writer. Those proceedings happened to interest the public, and have been copied from paper to paper, and from newspapers to standard works. When I have seen the extracts I have often smiled at the utility of memory which could thus accurately have given the result." (p. 52.)

The fifth chapter, on Reason, contains some examples of memory in animals, and of the degree to which they are capable of education or training.

The superiority of the operations of man to those of animals constitutes the subject of the sixth chapter. This is shown at considerable length by an account of the author's process of electro-metallurgy, voltaic chemistry, and other applications connected with electricity; also by railways, photography, &c. &c. Extracts are also given from the reports in the daily papers of an exhibition of

various kinds of bread by Mr. Smee, at a *soirée* at his house, in February 1847.

The great distinction that Mr. Smee draws between man and animals, in reference to the subject of the chapter, is, that the former makes and varies his own tools, the latter uses only those with which he is furnished ready-made by nature.

The chapter on Instinct contains accounts of the construction of the nests of insects and birds. This is followed by a chapter on intuitions, or "intuitive instinctive ideas." The ninth chapter, "On Words and Language," includes the consideration of how far these have been imitated by animals, and treats of the means of communication among animals, as well as the extent to which they comprehend words addressed to them by man.

The tenth chapter treats of the limitation of the operations of man as compared with those of nature.

The next chapter contains the author's theory of Instinct and Reason, comprising, first, the theory of electro-biology: these are followed by "Principles of the Human Mind," expounded in sixty-eight aphorisms, or propositions, expressed in the language of electro-biology, and quoted from the author's "Elements of Electro-biology." The nature of this theory of instinct and reason may in some measure be judged of by the following extract:—

"Upon this theory man only differs from the dog inasmuch as he has a higher organisation—a more elaborate structure; but, whilst man lives on this earth, there is no reason to suppose that any other faculty than this elaborate structure comes into operation."

A very material theory! The twelfth chapter, however, "On Reason and Faith," reconciles electro-biological theories of reason with revelation:—

"I have much cause (the author states) to rejoice that the system of electro-biology which I have developed, after many tedious hours of investigation with the midnight oil, and after many years of anxious thought, should now be found to be a system deduced by reason, and in perfect accordance with that religion which I hold by faith."

In the next chapter, on "Perverted Reason," the author notices errors of sense, delusions, mania, and the potato disease, with its relation to the *aphis vastator*.

We extract the following paragraph in reference to homeopathic globules:—

"Perhaps nothing is more remarkable than the nature of these globules. Each weighs about the 1-25th of a grain; and they are sold in little bottles labelled 'arsenic,' 'camphor,' 'musk,' &c. Now it has been stated that a grain of musk will scent a room for twenty years; yet some labelled musk which I examined had no odour. With regard to arsenic and copper, the refinements of modern chemistry can detect extremely minute quantities; yet the most eminent chemists have failed to obtain indications of the slightest trace of these materials: and be it remembered that the globules which have been examined have been taken from chests which, according to the statements of their owner, have produced wonderful cures. It is the common belief that all the globules are made alike; but it cannot be denied that occasionally those who practise homeopathy contrive to substitute active doses of the more deadly poisons when they are persuading their followers that they are but giving them harmless globules" (p. 273).

We believe that the author is quite correct in stating that the globules are all made alike. They probably contain nothing but sugar of milk. It is a curious fact that, whether they are labelled arsenic or mercury, camphor or cayenne pepper, each globule weighs the same, i. e., the 1-25th part of a grain, and all are of the same size, as if formed in the same mould. Hence it follows that there is either no medicinal substance in the globules, or there must be a wonderful accommodation in the doses—i. e., the lighter the medicine the less the quantity required, or it would affect the bulk!

The work concludes with a notice of the classifications of the various families of man and animals, and the proposition of a new natural classification on electro-biological principles. The proposed classes are to enjoy a very classical nomenclature; such as *Aisthemic*, *Syndramic*, *Noemic*, *Pneuma-noemic*, and *Dynamic*. The classification and its subdivision appear, however, to us, quite as artificial as any other ever proposed.

The work is furnished with well-executed coloured and plain engravings of some of the various objects brought under notice in its pages. We have discovered but little that is new in the volume: for this, however, we should not have looked, as we perceive that

the author informs us in his preface that it has been compiled as an amusement rather than a labour—a relaxation from the fatigues and anxieties of previous occupations. The author also states that he has preferred selecting his illustrations from facts of his own observing, "in preference to those which, although more remarkable, have been observed by others." It has appeared to us, that if Mr. Smee's observations and reasonings had been diligently compared with those of other investigators, his theories would have been more intelligible,—he would have been enabled to place a more correct estimate on his own labours, and science would have derived greater benefit from the contributions of so zealous and indefatigable a cultivator of the phenomena of nature. Apart from the author's peculiar theories, the book contains much that is interesting and suggestive in reference to the instinct of animals.

Proceedings of Societies.

ROYAL SOCIETY.

June 20th, 1856.

On the Structure of the Membrana Tympani of the Human Ear. By JOSEPH FOYNSSE, F.R.S., Fellow of the Royal College of Surgeons in England, and Senior Surgeon to the St. George's and St. James' Dispensary, London.

In this paper the membrana tympani is described as consisting of the following layers, which are quite distinct from each other, both as regards their structure and functions:

1. Epidermis.
2. The proper fibrous layer, composed of
 - a. the lamina of radiating fibres
 - b. the lamina of circular fibres.
3. Mucous membrane.

One of the principal objects of the paper is to describe the structure and functions of the fibrous laminae. Since the time of Sir Everard Home, who pronounced the layer of radiating fibres to be muscular, anatomists have differed in their views of the nature of the fibrous element of the membrana tympani. The lamina of radiating fibres, the outer surface of which is covered by the epidermis, is described as continuous with the pericottum of the external meatus. With the exception of the uppermost fibres, which, on account of their

being somewhat floccid, have been considered as a separate tissue under the name of "membrana floccida," the radiated layer is composed of fibres which extend from the circular cartilaginous ring to the mallens, and there interlace in their course. These fibres are from 4000 to 6000 parts of an inch in breadth.

The lamina of circular fibres consists of circular fibres which are firm and strong towards the circumference, but very attenuated towards the centre. The fibres are so attached and arranged as to form a layer of membrane, which, in a quiescent state, is saucer-shaped.

The fibres composing the circular are smaller than those of the radiated lamina, being from 6000 to 10,000 parts of an inch in breadth.

The facts that appear to be adverse to the idea of the fibres of either layer being muscular, are:—

1st. The absence of distinct nuclei in the fibres.

2nd. Their great denseness and hardness.

It is next shewn that the four laminae forming the membrana tympani are continuous with other structures of which they appear to be mere modifications, and that not one is proper to the organ.

The tensor tympani ligament, which had not been previously noticed by anatomists, is particularly described; it is attached externally to the mallens, close to the insertion of the tensor tympani muscle, and internally to the cochleariform process.

The latter part of the paper is occupied by observations on the functions of the fibrous laminae and of the tensor ligament of the membrana tympani, and it is shewn that, by these two antagonistic forces, the one tending to draw the membrana tympani outwards, the other inwards, this organ is maintained in a state of moderate tension, and is always in a condition to receive ordinary sonorous undulations.

ACADEMY OF MEDICINE, PARIS.

July 22, 1850.

Cholera in Packet-ships.

M. GYRARDIN read a report from M. Renault relative to the appearance of the cholera on board the packet-ship *Luxor*, in October and November 1849. The *Luxor* having remained thirty-five days at Marseilles, where the cholera prevailed, left that port with crew and passengers in good health. During the first few days several men had choleraic symptoms. On the seventh day one of the sailors was seized

with malignant cholera. From this time an epidemic focus—(focus of contagion?)—was formed, under the unfavourable influence of damp, want of ventilation, fatigue, crowding, &c., and cases of cholera and diarrhoea multiplied. On the eleventh day another sailor was attacked with cholera. On the arrival of the vessel at Smyrna the passengers, crew, and cargo, were landed; and every hygienic precaution being adopted, no fresh case occurred.

M. PELLERIN related a large number of facts proving the transmission of cholera by contagion, which he had observed in the districts of Finisterre and Côtes-du-Nord during the late epidemic.

SURGICAL SOCIETY OF PARIS.

July 17, 1850.

Cutaneous Affection of a doubtful character.

M. HUGUIER presented the following case to the notice of the Society:—A young negress having had an irritating application on the chest for some internal affection, there appeared on the surface several small, hard, painless tumors, about the size of a small nut. The skin on these tumors did not differ from the rest of the integuments. A slight depression existed on their summits.

M. Huguiet had removed several of these growths, and a smooth cicatrix was left. In external character they resemble the cancerous disease termed *Keloids* by Alibert. M. Huguiet regarded them as consisting of an hypertrophy of the follicles of the hair,—a form of disease which he had before described under the name of *acanthriopsia*.

MM. LABREY and GOSSELIN concurred in the opinion.

M. GERALDES thought it analogous to the form of disease known in England as chimney-sweeper's cancer.

M. CHASSAIGNAC exhibited an exostosis removed from the tibia.

M. MALBONNEUX also presented a specimen of disease of the elbow removed by operation.

Degeneration of the Structure of the Heart.

M. VERNEUIL exhibited a specimen of ossification of the pericardium covering the heart, and of rupture of the right auricle, found in a female body which had been appropriated for dissection. The wall of the auricle had undergone a cretaceous degeneration over a considerable extent. The heart was hypertrophied, and its substance flabby. Blood was found extravasated under the pericardium of the auricle.

Case of Utero-placental Hemorrhage.

M. BIOT exhibited an ovum which had been expelled at two months, about the size of a hen's egg, of a livid red colour, and presenting all the appearance of a clot of blood. It was found to consist of an extravasation of blood filling the interspace of the decidua and chorion. The coagulum was thicker and denser at that portion of the chorion from which the placenta was developed, the vessels of which appeared to have been the source of the hemorrhage. No communication had existed with the cavity of the amnion. An aperture was detected in the latter membrane; whence it was ascertained the fetus had been expelled entire three days previously. The umbilical cord was found floating in the cavity: no trace of umbilical vesicle could be discovered.

M. BROWN-SQUARD also exhibited several fetal rabbits, with which hemorrhage had taken place into the cavity of the amnion. The mother had died of acute pleurisy.

On the Constant Presence of Cystoeceri in Rabbits.

M. BROWN-SQUARD stated that he had examined upwards of eighty rabbits, and always found cystoeceri in the abdominal cavity: he therefore considered their occurrence to be constant. An example of their presence in the rabbit's abdomen was exhibited.

Inoculation of Secondary Syphilis.

M. VIDAL (of Cassis) communicated to the Society the results of his experiments, showing—that the matter of the ecthymatous pustule may be transmitted by inoculation from one patient to another, and from the diseased to the healthy individual.

Fibrous Polypus of the Rectum.

M. HUGUENI related the particulars of a fibrous polypus, which he had removed from the rectum by means of a double ligature. The fibrous character of this polypus was doubted by some members, who regarded it as a fleshy substance, resulting from hypertrophy of the mucous membrane of the rectum.

BIOLOGICAL SOCIETY OF PARIS.

PRESIDENT, M. RAYER.

Monthly Summary, April 1860.

Explanation of Crossed Paralysis of Sensation. By M. BROWN-SQUARD.

THE author stated, that, from his investi-

gations and experiments, he had arrived at the conclusion that the interlacement of the sensitive fibres of the spinal cord throughout its whole length completely explains the phenomenon of crossed paralysis of sensation; and that the various points of interlacement usually referred to the medulla oblongata, pons varolii, &c. are insufficient to explain the phenomena.

Mucous Membrane expelled from the Uterus during Menstruation.

M. LEBERT described a membranous sac, of the shape and size of the cavity of the uterus, expelled during a paroxysm of painful menstruation. This substance measured four centimetres (=1.74 Eng. inch) in length, and from two and a half to one centimetre (=.983 — .893 English inch) in breadth, and about one centimetre (=.893 Eng. inch) in thickness. It presented three apertures corresponding with the os uteri and orifices of the tubes. Internally, its surface was lined with pavement epithelium, the cells of which were from an eightieth to a ninetieth of a millimetre in diameter, enclosing an ovoid nucleus, and these again containing nucleoli.

M. Lebert considered that this pathological specimen lent confirmation to the opinion of those physiologists who consider that menstruation is normally attended with the formation and expulsion of a false membrane, analogous to the decidua of pregnancy.

ACADEMY OF SCIENCES, PARIS.

July 22nd, 1860.

New mode of removing Wens—their Composition.

M. A. LEGRAND having several times witnessed fatal consequences following the removal of wens by incision, had been induced to practise the following operative proceeding:—The skin around the base of the tumor was divided by repeated linear applications of pure potash; by the continued employment of these, the line of eschar becomes deeper, until the tumor is detached. It is necessary in this way, as with the knife, to destroy the whole growth, or it will re-appear. M. Legrand had put this plan into execution thirty-two times, without erysipelas or any other ill result having followed.

M. Legrand added, that having examined two wens, one from the scalp, the other from the forehead, he had found that the one consisted of an hypertrophied sebaceous follicle, and that the pathological basis of

the other was a hair follicle. The contents of both were chiefly fatty matter, epithelium cells, and granules. In that removed from the forehead, crystals of cholesteroline were found in abundance.

Employment of Adonis vernalis in Ague.

MR. SIMON ST.-PIERRE stated the results of seven trials with this remedy. Three doses of thirty grammes (nearly an oz. Eng.), boiled in about a pint of water, had proved sufficient to arrest the disease in each instance.

Hospital and Infirmary Reports.

KING'S COLLEGE HOSPITAL.

Operations.

SEVERAL operations of interest have been recently performed by the surgeons of this hospital, and we shall give a short description of each of them.

The first case was one which is of particular interest, inasmuch as it presented an instance of the power the surgeon possesses of removing by manual proceeding the deformity which results too often after the cicatrization of a wound caused by a burn. The patient, who was a little girl about ten years of age, had been burned some time ago in the left arm. The injury affected the parts just in the flexure of the elbow-joint. It is probable that so much precaution as was necessary in these cases was not taken; for as cicatrization had ensued, the elbow became contracted by the firm puckering of the integuments. In order to set this free, Mr. Fergusson performed the following operation:—By two nearly parallel incisions he included the old cicatrix, which was about three inches in length, and dissected it cleanly away from the tissues beneath. Another incision, at right angles to the former, about an inch in length, was then made, for the purpose of taking off the tension of the parts, and the edges of the wound were brought lightly together with thin sutures. This operation, although simple in appearance, requires great neatness and care in its execution; and much precaution is necessary in the after-treatment of such cases, as there is always a very great disposition in the parts to contract after they have been liberated. Mr. Fergusson stated that the plan he had just adopted was very similar to the one recommended by Sir James Earle, and that it was the most likely to be successful in the removal of such contractions.

The next operation was one of a somewhat peculiar nature. A young child was brought into the theatre who had the vulva entirely closed, no orifice being seen but that of the rectum, which was marked, in fact, more by a protrusion of the mucous membrane of the gut than by any very discernible outlet. From the upper portion of this the urine came away when the child made efforts to pass water. Above, in the situation of the vagina, was a long cicatrix, on a completely smooth surface, the effects of sloughing which had some time ago ensued after an attack of measles, and had ended in complete obliteration of the female external organs. It was for the object of restoring the passage that Mr. Fergusson operated to-day. He first distinctly made out the situation of the rectum, and that of the opening of the urethra; and then, with a bistoury, carefully laid open the whole length of the cicatrix. This was readily effected, so that the finger could be passed with ease up the vagina. The septum between it and the rectum was found to be entire, and the hymen untouched. The parts were kept patent by pledgets of lint introduced between the lips of the newly-made wound.

The next operation was performed by Mr. Bowman upon a young man who had a very curious tumor situated upon the forehead; chiefly, indeed, implicating the substance of the upper eye-lid, and extending from thence for more than two inches up along the integuments of the frontal bone. It appears that this tumor had been growing there ever since the patient was two years of age, at which time he had received a blow on the spot where the abnormal growth was situated. It was so prominent that it partly closed the eye, and it gave to the touch the feeling somewhat similar to that which is experienced on handling a bunch of enlarged veins of the spermatic cord. The tumor could also be partly pushed upwards from the orbit to the frontal region. There was no discolouration, and no pulsation particularly could be felt. Both Mr. Bowman and Mr. Fergusson considered it to be a vascular tumor; and it was deemed advisable to obliterate it by means of an operation, partly performed by the knife, partly by the needle and ligatures. It was not deemed safe to extirpate it entirely by the knife, as it was considered to be of so vascular a nature that serious hemorrhage might result. The ligature alone could not well be used, as this would cause a considerable loss of skin, which would produce a greater deformity than the tumor itself. A modification, then, of both these plans of treatment was very skillfully and judi-

ciously used by Mr. Bowman. He first incised the integuments freely over the mass, and laid them on one side, so as to expose the greater portion of the swelling situated in the orbit. He then, with needles and strong thread, surrounded the base of the tumor, and firmly strangled it. Whilst making the incision with the knife, smart bleeding took place from many arterial vessels which were divided. This, however, was easily checked by the assistant compressing each of them with his finger as they were divided. Mr. Bowman then laid bare the upper portion of the tumor, and used the ligature: he also removed a portion of the tumor. After the mass was firmly strangled all bleeding ceased, the skin was brought lightly over the wound, and the patient was carried to bed. Mr. Bowman stated, as regards the precise structure of the tumor, that it was impossible, without a minute examination, to speak with certainty about it. Both Mr. Fergusson and himself had considered it to be a vascular tumor, composed chiefly of veins; but it was evident that it consisted chiefly of arterial ramifications.

Mr. Partridge had an interesting case in which he operated. A patient of his had lost about an inch of the integument of the penis, and the urethra had been laid bare and perforated. The consequence was that all his urine came away by this artificial opening, and of course this condition was a source of great discomfort to the patient. Mr. Partridge determined to endeavour to close the opening, and he performed the operation in this manner:—He first carefully dissected back the skin, by making an incision on each side and at each extremity of the wound, thus forming a somewhat quadrangular flap. The edges of this flap were then pared: this constituted the first part of the operation. The main point, however, in the process is to bring the parts accurately together, and to keep them in this position until the wound may have healed. There are various methods of doing this. Mr. Partridge made use of the *dead suture*, which is well adapted for operations of this kind. He used it according to the manner, and by means of the very ingenious instruments invented by Mr. Charles Brook. By means of these sutures the wound was accurately closed, and the operation was concluded. The catheter which was placed in the urethra prior to the operation was allowed to remain there.

Tumor of the Upper Jaw.

On Saturday, June 22, Mr. Fergusson operated on a patient who had disease of the upper jaw, and whose case presented

features of great interest. A few weeks ago we reported a case where there was disease of the upper jaw, which was considered to be of a malignant character, and requiring excision of the whole of the affected bone. Mr. Fergusson, however, doubted the nature of the disease; and, prior to performing the operation which had been considered necessary, he deemed it prudent to puncture the swelling. On this being done, it was discovered to be a collection of viscid fluid in a much dilated antrum. A portion of the wall of the antrum was removed, and the cavity was kept open. A free discharge of the fluid was thus allowed. The parts contracted much, and the patient went out well, or nearly so. We here refer to this case because there is a great resemblance between it and the one now under notice. In the former the patient was a young woman, and the disease had not been noticed for more than a twelvemonth. In the latter case the subject is a man between 40 and 50. He first noticed the disease seven years ago, but did not have any surgical attendance until four years ago, when, as the swelling of the jaw had increased, he applied to a surgeon, who punctured it, and let out a quantity of reddish-looking fluid. This continued discharging some time, when the wound closed up; but some time ago it broke of its own accord, and has continued discharging ever since. On his application to the hospital there was a considerable prominence of the superior maxilla on the right side, but there was no protrusion of the palate. An orifice existed in the upper alveolar border, from which a yellowish discharge took place. This came evidently, as could be ascertained by a probe, from the antrum. By pressing firmly on the lower wall of the antrum, it could be felt to be slightly moveable; and a creaking noise was heard, indicating the entrance and exit of air: at the same time, this movement increased the flow of the fluid from the cavity in which it was contained. It was very evident what was the nature of the disease. Mr. Fergusson pronounced it to be not a solid tumor of the jaw, but a dilated antrum secreting constantly a peculiar fluid; and, as the enlargement disfigured the patient much, and he was very anxious about it, he determined to remove the disease by operating in the same manner as he had done in the former case. The patient was placed under the influence of chloroform, —a proceeding which is objected to by some surgeons in the operations on the upper jaw; but Mr. Fergusson always uses it in his operations on this locality. We have seen him remove the superior maxilla in several instances when the pa-

tients have been under the influence of chloroform, and there has appeared to be no more danger or distress connected with it than if the operative proceedings were being carried on on any other part of the body. There is some difficulty in keeping the patient long under its influence, as the surgeon's hands are in the way, and prevent the application of a handkerchief or mouth-piece; but Dr. Snow manages to keep up the anæsthetic influence by applying a sponge with some chloroform on it every now and then to the nostrils and mouth.

Mr. Fergusson first laid open the upper lip, carrying his incision a little to the right through the nostril. He then dissected it from off the palate, and fully by this means exposed the expansion of the upper jaw. He then, with a sharp pair of curved bone forceps, removed about a square inch of the anterior wall of the antrum, which was lined by its membrane, of considerable thickness. The divided lip was then brought accurately together by thin hare-lip pins, and the operation was concluded. There was very free bleeding from the divided vessels, which was, however, checked by the fingers of an assistant compressing the lip.

In the remarks which Mr. Fergusson made after the operation, he drew the attention of the pupils to the great resemblance between this case and the one which had only a few weeks ago been in the hospital. It was somewhat remarkable that they should have met with two cases close upon each other, as instances of this nature were by no means of common occurrence. He had himself only met with one of the kind previously. A cast of this was shown by Mr. Fergusson. In this case the antrum had been very much dilated, and its anterior wall was, as it were, thrown upwards, so as to form a most remarkable prominence.

In the case where he operated some few weeks ago, they had seen that the antrum had contracted much afterwards, and that there was very little disfigurement when the patient had left,—although, of course, the deformity was not entirely removed. He had performed the same sort of operation in the present instance, but with this addition: he had laid open the soft parts, so as to get freely at the diseased portion. He could no doubt have managed to remove the portion of the front wall of the antrum through the mouth; but it would have been an awkward and troublesome matter, and a most unsightly one. He thought it better in this instance to lay the cheek open, so as to be able fully to expose the expanded bone. They had seen that he had made such an incision as

would leave hardly any deformity behind from the scar. He had merely laid open the upper lip in the mesial line, and had then dissected the parts up from the gums. This was the incision that he preferred in operations upon the upper jaw. The whole of the bone may be removed through this limited incision; but it is one particularly applicable to cases in which there is no necessity to meddle with the upper wall of the antrum. By carrying the external cut through the upper lip, and into the nostril, as he had done in the present instance, the surgeon is enabled to get at the expansion of the jaw without making the very free incisions in the face, which leave behind them very extensive scars.

Correspondence.

THE CASE OF ANNA MERRITT. DR. LETHEBY'S EVIDENCE.

SIR,—When I read the following sentence in the conclusion of Dr. Letheby's letter inserted in the "Medical Times" of March 23rd, 1850, wherein he states—"It is my duty to admit that it is within the range of possibility, nay it is even possible, that the poison (arsenic) might have been taken, as the condemned woman asserts it was taken, early on the morning of her husband's death,"—I thought, on mature reflection, Dr. Letheby had seen his error, and honourably admitted it, and such indeed was, I believe, the general feeling of the medical profession. Had it not been so, I should then have told Dr. Letheby, as I now do, that it was not only Sir Benj. Brodie, Dr. Leeson, and Dr. Ure, who were consulted by the Home Secretary, who disagreed with him, as mentioned by Mr. Bright in the House of Commons, but that several of the first toxicologists of the day did not hesitate to say Dr. Letheby was wrong; and the above retraction only was the cause of my silence. He has, however, again come forward, not in the "Medical Times," where he admitted his error, but in the "Times" newspaper of Monday last, and attacks Mr. Bright, who, in the performance of a public duty, in his speech on the abolition of capital punishments, alluded to Anne Merritt's case, and stated that which was a general impression,—that she was convicted on the rash and unscientific opinion of a medical witness, which was the cause of Mr. Bright's allusion to it as one of the many cases showing the great difficulty of carrying out the sentence of the law after conviction.

In Dr. Letheby's reply to Mr. Bright in the "Times" of Monday last, he repudiates his former admission, and says—"The more

I think over this case, the more I bring it in relation, not only with the results of my own experience, which is not small, but also with the published accounts of other cases,—the more fully am I convinced that my conclusions were founded on facts; that my evidence was the expression of truth: and, since the publication of my letter in the 'Medical Times,' I have not heard one word in opposition to the soundness of my judgment. Therefore, as the opinions on which Sir George Grey acted have not been published, and as none of our toxicologists have even referred to the subject in any of the medical periodicals, with all due deference to Dr. Letheby's vaunted experience, I humbly beg to state the reason why I doubt the soundness of his judgment; feeling deeply the awful consequences which may ensue, in future judicial investigations, if such evidence is admitted in criminal prosecutions.

The question at issue is—How long before death was the arsenic taken? Dr. Letheby, unsupported by any other evidence, circumstantial or otherwise, states, "not more than two or three hours before death;" because he found three-quarters of a pint of undigested gruel in the stomach, which contained $8\frac{1}{2}$ grains of white arsenic, not in a solid state, but in a state of solution. In his letter of March 28d, he asks—Is it probable that a dose of arsenic taken early in the morning would have remained in the stomach for sixteen hours? This is a hypothetical and irrelevant question, because he only found $8\frac{1}{2}$ grains in the stomach; and it is quite consistent with experience that this small residuum of a dose swallowed by the deceased, should have been found in the stomach in spite of all the vomiting, &c.

2. He says, supposing for one moment that such a result could have happened, then the fluids found in the intestines ought to have contained at least as much of the poison as those found in the stomach, after all its retching, vomiting, &c.

Dr. L. seems to forget that in the intestines the poison becomes diffused over a much larger surface, and that it is very frequently carried off by purging, and bases his very strong opinion on the allegation that the eight grains of arsenic which he found in the stomach were really dissolved in a quantity of undigested gruel; but he does not state how he ascertained it was held in solution, and not mechanically diffused as a fine powder in the gruel; nor, in fact, has any proof been adduced, to my satisfaction, that the arsenic was really dissolved; nor has the whole of the arsenic been proved to have been dissolved by any medical fact, to my knowledge. Indeed, I cannot understand how such an inference was arrived at; and, on looking over Dr. Letheby's evidence, I find

he states there was a small portion of a whitish powder adhering to the inner lining of the stomach, but too small a quantity to ascertain what it consisted of: this was no doubt arsenic, and could easily have been proved to be so. This fact renders it probable that all the arsenic was not dissolved, and nothing short of the separation of the $8\frac{1}{2}$ grains which he found in the stomach in a clear solution in water, would have justified Dr. Letheby in making the assertion that the poison was entirely dissolved, and not mechanically diffused as a fine powder through the gruel. It is extremely probable that the gruel taken before death might have become mixed with the poison which had been taken in powder several hours before, and in four or five hours might dissolve a portion of the poison already existing in the stomach at the time it was swallowed: hence, even if a small portion of arsenic was actually proved to have been dissolved, this would not show that the poison must necessarily have been administered in the gruel in from two to three hours before death.

Dr. Letheby concludes there were about two grains of arsenic in the liver, and says—"My observation in reference to the time the poison had been taken, has reference both to the stomach and liver." How this fact would enable him to draw such a conclusion I know not, for I cannot find a single case where two grains of arsenic were found deposited in the liver in either two or three hours; nor have I, on inquiry, found any toxicologist who has met with one.

That arsenic should remain in the stomach even for several days, in spite of copious vomiting, &c., it is not necessary, as Dr. Letheby states, that it should be administered in *large granules*. It so remains in many cases, when taken in fine powder, as in Merritt's case; and if a small quantity, say a few grains, were left adhering to the mucous coat when the gruel was swallowed, this would account for all Dr. Letheby found, without supposing the arsenic was administered dissolved in the gruel, either two or three hours before death, or at any other time. Finally, Dr. Letheby appears to think that gruel, if found undigested in the stomach, must have been taken within two or three hours: this inference, which may apply to healthy digestion, is inapplicable to the case of a person whose stomach is irritated or inflamed by arsenic; and it is quite consistent with experience that the gruel might have been in the stomach for seven or eight hours, and yet not be perfectly digested. The inference, therefore, so far as the undigested state of the gruel is concerned, is not in accordance with physiological facts.

It appears to me that Dr. Letheby has

either committed the great mistake of supposing that gruel taken subsequently to the swallowing of arsenic, cannot mix with or dissolve a very small portion of that poison,—say, as in this case, eight grains to twelve or sixteen ounces of gruel; or he has committed a more serious mistake in inferring that the gruel so taken was necessarily the vehicle in which the poison was administered.

If this reply to Dr. Letheby is not too long for your columns, I shall feel much obliged by its insertion, and am, sir,

Yours most respectfully,
G. B. WADSWORTH.

19, Golden Square,
July 23rd, 1850.

Medical Intelligence.

THE APHLOGISTIC LAMP APPLIED TO ECONOMICAL USE.

THE following suggestion respecting the employment of the aphlogistic lamp for the purpose of procuring heat, was made many years since by Dr. G. Merryweather, of Whitby. As it may be new to some of our readers, we here reprint an extract from a paper which has lately reached us.

"Let the reader imagine a tin lamp of the ordinary form, the cavity of which is filled with strong spirits, instead of oil. A cotton-wick lies among the spirits. One end of it is brought through the orifice or mouth of the lamp in the usual way, and the different threads of which it consists are separated, and spread out a little, so as to leave a sort of cavity in the middle. In this cavity is placed a coil of platina wire, of a cone shape, formed by five or six spiral turns of wire. The wire is about the 100th part of an inch in diameter, and the coil forms a sort of cup, about three-eighths of an inch wide, in the middle of which lies a small piece of *spongy platina*. The lamp being filled with spirits, the wick is to be inflamed: in about a minute the platina becomes red hot; the flame is then blown out, and the glass cover placed over the lamp. The use of this cover is to confine the heat, and prevent all cross currents, so that no air may reach the platina, except what passes in by air-holes below, and escapes by the chimney or opening in the cover above. If the lamp is standing in a chamber somewhat darkened, the platina will now be seen constantly presenting that glowing or incandescent appearance which we observe in red hot iron. The light is strong enough to enable one to see the hour on the dial-plate of a watch at mid-

night, but produces no annoying glare. The spirits are decomposed and fly off, forming, as Dr. Merryweather thinks, water and acetic acid.

"Such is the lamp in its simplest form,—that is, when consisting merely of one burner. If a greater heat is wanted, three, six, or more burners may be placed upon one lamp. In the lamp exhibited to the Royal Society, Dr. Merryweather employed fifteen burners, placed near one another; and with these he maintained a perfectly constant temperature of 396° of Fahrenheit. We call it a lamp; but, when fitted on this scale, a *furnace* is the more appropriate appellation. He was informed that this was a very desirable heat for many chemical or pharmaceutical purposes; but he could, if necessary, raise it to 1000°. He found that the platina wires and sponge undergo no sensible waste, and observes that, by connecting a reservoir of spirits with the cavity of the lamp, the heat might be kept at the same uniform elevation for years. Pure alcohol is not necessary. He finds that whiskey, 22 per cent. over proof, 9s. per gallon, answers perfectly well. With fifteen burners, producing a temperature of 396°, the quantity of spirits consumed in twelve hours costs 1s. 3d.; with three burners, yielding a heat of 160°, the cost is 3d., which is not great, if compared with the expense of attendance when the same object is obtained by means of common fuel. It may be proper to mention, that the glass cover placed over the lamp with fifteen burners was a cylinder about seven or eight inches wide, and ten or twelve inches high; and the heat alluded to was kept up within a space of this extent, which was empty, and of course available for experiments."

CHOLERA AT MALTA.

LETTERS from Malta of the 22d instant state that the cholera was still prevailing there; and, though the cases were not quite so numerous, the mortality was as great as ever. The men and women of the 44th Regiment still suffered, and, unfortunately, not one attacked has been saved. The fleet was still cruising on and off the island, daily communications being kept up by boats. The amount of deaths in the fleet was small compared to those on shore, only 14 having occurred out of more than 300 cases.

THE CHOLERA IN MEXICO.

LETTERS from Vera Cruz, *via* New Orleans, dated the 1st instant, announce that the cholera was devastating the capital. From May 17th to June 16th several hundreds had died. At St. Luis Potosi also the disease prevailed.

COLNEY HATCH LUNATIC ASYLUM.

DR. J. D. DAVEY, formerly one of the physicians of the Hanwell Lunatic Asylum, has been appointed one of the resident physicians at the Colney Hatch Asylum. Dr. Hood is to be Dr. Davey's colleague.

MEDICAL BELIEF IN IRELAND.

THE total number of dispensaries in Ireland is 660; fever hospitals, 63; and Infirmarys, 41. The presentments made for their maintenance in 1849 were £78,011. 18s. 5d.; and the subscriptions amounted to £29,470, 18s., making a total of £107,482. 18s. 5d.

THE JACKSONIAN PRIZE.

THE following subject has been announced by the Council of the Royal College of Surgeons for this prize, for the ensuing year:—"The Causes, Diagnosis, and Treatment of Obstructions of the Intestines."

CASE OF PROFESSOR WEBSTER.

AFTER a hearing before the full bench of the supreme Judicial Court, upon the application for a writ of error, in the case of Dr. Webster, the judges have decided that their former proceedings were strictly legal, and in conformity to the statute; and the application was therefore dismissed. It is expected that the Governor and Council, with whom the whole matter now rests, will act upon his case in a few weeks.—*Boston Medical Journal.*

ROYAL COLLEGE OF SURGEONS.

GENTLEMEN admitted members on the 26th inst.:—Messrs. C. A. Newnham—F. Martin—W. Cockcroft—H. W. Lobb—A. Kitching—C. O. Rowley—J. A. Cox—E. W. S. Davis—G. G. L. Williams—S. A. Chaldecott.

Admitted on the 30th inst.:—Messrs. W. Bourne—T. D. Everet—G. S. Rhodes—J. W. Beaumont—J. Marriott—T. Brown—J. P. Scowcroft—R. E. Fitzgibbon.

APOTHECARIES' HALL.

NAMES of gentlemen who passed their examination in the science and practice of medicine, and received certificates to practice, on Thursday, 26th July, 1850:—George William Pettinger, Sutton-upon-Trent, Notts—Henry Goode Wright, Hereford—Thomas Carey, Midhurst, Sussex—William Helps, Gloucester—Harry William Lobb, Southampton—John Kenyon Wright, Stockport, Cheshire—Frederick Hudson, Stockport, Cheshire—Francis Godrich.

OBITUARY.

On the 25th instant, at Hampstead, Middlesex, aged 73, Mr. Thomas Davis, surgeon.

At Bombay, of malignant cholera, Dr.

Larkworthy. This gentleman was cut off by the disease during his active attendance on the sick.

On the 30th ult., John Miles, Esq., surgeon, 78, Gracechurch Street, deeply lamented by all who knew him.

Selections from Journals.

ON THE PRESERVATION OF THE BODY IN CASES OF ARSENICAL POISONING. BY T. G. GEOGHEGAN, M.D., DUBLIN.

[Concluded from p. 175.]

In the judicial examination of cases of arsenical poisoning, much attention has been excited of late years by the singular condition of *preservation* in which the digestive tube and (in some cases) the entire body has been discovered at comparatively remote periods after death. As such inquiries are of some importance to the practitioner in the exercise of his medico-legal functions, I shall proceed to relate the observations which I have been enabled to make.

A. In the cases in which the *whole body* has been the subject of inspection, the following conditions have been noted:—

1st. Complete preservation of the body and viscera (during periods of observation of from sixty-three to seventy-two hours) under circumstances *highly favourable* to putrefaction.

2d. Rapid putrescence of body and viscera (under conditions extremely adverse to decomposition). This occurred in the case of an individual who fell a victim to the accidental administration of arsenic after an illness of thirty-six hours. The weather at the time was decidedly cool; the subcutaneous cellular membrane, notwithstanding, was emphysematous (from putrefaction) twenty-two hours after death; and the intestinal canal, in eight hours more, was much advanced in decomposition, of a dark green colour, and rapidly evolving gases of extreme fætor.

3d. Good preservation of stomach and intestines; the body decaying as usual (at periods of forty-seven and seventy-five days after death, the illness having been of eleven days duration).

4th. Rapid decomposition of the alimentary canal; the body remaining unaffected. (Examination thirty-four hours after death; illness of fourteen days.)

B. *The digestive tube*, or parts of it (removed from the body), has exhibited the undermentioned phenomena.

1st. Almost complete preservation of stomach (emptied of its contents) during

four weeks, followed by modified decay during eight weeks, with neutral state of tissue, and termination in ammoniacal decomposition. The case to which this observation refers is that recorded in the first part of this paper. The poison appeared to be altogether in solution, and when modified decay commenced, the surface of the organ became coated with a pulpy matter. The morbid appearances, however, remained little altered. At this date (thirteen weeks after death), the black extravasation is quite distinct, the walls of the organ firm on traction, and the peritoneal surface retentive of its lustre. The odour is different from that of common putrefaction.

2d. *Rancid putrefaction*.—I venture to apply this designation to a singular condition which I believe to have been not hitherto described, and which I have witnessed in at least three cases. It is characterized by a *peculiar odour* (somewhat like that of fatty matter when undergoing slow decomposition), and by a *persistent and strong acid reaction*, which, in a stomach still under observation, has been retained undiminished during more than three years and a half. I have found in these cases that the tissues retain their physical characters and morbid appearances unimpaired for some weeks, but finally become softened and discoloured. The absence of ordinary ammoniacal putrefaction is not confined to cases in which death has been produced by arsenical poison. I have found a stomach, which contained a quantity of vegetable matter (apparently apples), undergo the *acetous fermentation* in its tissues, and retain it (although frequently washed) for several months. It is still more singular that a fluid so prone to decomposition as urine, is occasionally imputrescible. Thus I have had a specimen (containing abundance of urea) which remained perfectly fresh for years, although kept in a vessel by no means air-tight. It perhaps included more saline matter than usual. I have observed much retarded putrescence in some other specimens.

In arsenical poisoning, as sometimes happens in other forms of death, the decay of the alimentary canal is not equal throughout: thus in a case (fatal in less than a day) the intestines commenced to putrefy twenty-four hours after death (in cold weather), and on the third day the osæum was advanced in putrefaction, the stomach remaining unchanged—a circumstance perhaps due in that instance to the presence of free æsæic on parts of its surface; yet the uterus and a portion of muscle which could have only contained the small quantity of poison that reached them through the blood, were quite fresh on the fifth day.

In comparing, however, the preservation of different organs, due allowance should be made for diversity of tissue. A careful examination of the cases which have occurred in my experience has led me to the belief that, with the exception possibly of those instances in which the poison in quantity is in direct contact with an organ (*e. g.*, the stomach), *the preservation of structures, or the contrary, is not influenced by its presence or absence in their tissue*. Thus I have found arsenic absent in the membranes of a stomach, which, after an inhumation of forty-seven days, was in a state of considerable preservation, and the great intestine, which afforded not more than one-twentieth of a grain of arsenic, was in an equal state of integrity after seventy-five days. Arsenic was discovered freely in the liver, which was less preserved.* On the other hand, in recorded cases, in which the whole body has putrefied rapidly, the presence of the poison has been indicated in the fluids and tissues, either by chemical examination or (as in a case which I have witnessed) by the short duration of the illness having been incompatible with the elimination of the portion absorbed. The quantity of the latter, moreover, even in those organs which appropriate the greatest amount (*e. g.*, the liver), is absolutely small; not perhaps exceeding, on an average, from one and a half to two grains; and hence incapable of effecting their preservation by direct chemical union. I have been hence led to infer, that arsenic, in most cases, produces its antiseptic effects, local or general, by means of a *catalytic* or disposing influence, in virtue of which such changes are produced, either in the molecular condition of the tissues, or in the atomic grouping of their ultimate elements, as confers on them a character of unusual stability.

In concluding this notice of the post-mortem changes in arsenical deaths, I wish to allude to the alleged evolution of an *alliacæous odour* during the decay of the body. Although I have not met with this condition in the body at large, I have perceived it most distinctly during the slow decay of organs which have received the poison by absorption—as the liver. I have no doubt that it is due to the evolution of arseniuretted hydrogen.†

* In this case, although the patient survived eleven days, the poison seemed present in as great quantity in the liver as I have found it in others which were fatal in a few hours. In another instance, I detected the poison in the same quarter after an illness of fourteen days. It was, however, in much less quantity than usual.

† It was curious to observe that numerous minute insects which had attacked fragments of these organs (contained in a closed vessel), seemed in no wise incommoded by the atmosphere in which they were placed.

Some of the facts already cited, in association with a variety of others, are, I conceive, incompatible with the hypothesis offered by Liebig, as explanatory of the *modus operandi* of various mineral poisons (arsenic included), which assumes their deadly action to be due to a chemical union with the tissue of the different organs, and consequent arrest of those vital transformations within them which are essential to the performance of their functions. That chemical combination of the poison with the tissues takes place, I have satisfied myself with respect to the liver. The liver of a patient poisoned by arsenic (the organ having been divided into small fragments) was macerated in successive portions of distilled water, till all the blood and soluble matters were extracted; almost the entire of the poison was found in the insoluble residuary tissue. We are not, however, from hence legitimately entitled to infer, that such union is the cause of the usual symptomatic manifestations, and for the following reasons:—

1st. The quantity of the organic tissue involved in such combination (compared with the mass of the organ) is excessively small, as already shown. Were the quantity of absorbed poison sufficient to chemically saturate the proximate principles of our organs, the latter should in *all* fatal cases be imputrescible, which is contrary to experience. It is remarkable that, in poisoning by corrosive sublimate, which has such a powerful affinity for animal matters, the preservation even of the digestive organs is not effected by it, except at those points of the mucous membrane corroded by its direct contact: such, at least, is the result of my experience.

2d. The lesion of function is often inversely as the amount of poison contained in the tissue (as in the liver, muscular system, &c., compared with the brain and heart).

3d. Life is often compromised in cases where the entire of the compound, which may have been formed by the union of the poison with the tissues, has been eliminated by the excretory organs.

The foregoing considerations, together with the rapid disappearance of poisons from the blood,* lead to the inference that

* In a case of poisoning by acetate of lead, in which two drachms were administered twelve hours before death, I was unable to detect a trace of the poison in the blood collected from the heart, although the heart itself gave indications of its presence; while in the liver, kidneys, lung, and tissue of the intestine, it was readily discovered. The deposit of poison in the osseous system has been interestingly shown in some recent cases of arsenical poisoning. From this system, however, it is ultimately eliminated; thus I could not discover any trace in a portion of the femur of a girl who died on the fourteenth

the depositions of the latter in the tissue of the secretory organs and in the muscular and osseous systems, so far from being the cause of the formidable manifestations observed, is, on the contrary, a beautiful provision for the allocation of those substances in parts in which their presence is productive of the least disturbance. Even in the now advanced state of physiological knowledge, it must be confessed that, however definite our conceptions may be of the local action, absorption, circulation, distribution, and final elimination of poisons, the intimate nature of their influence on remote organs has yet to be determined. Thus, notwithstanding the ingenuity of modern theorists, we are still ignorant of the precise *modus operandi* of strychnia on the spinal cord, of tobacco or arsenic on the heart, or of opium on the brain.

There seems, meanwhile, to be little reason to doubt that arsenenic acid produces a profound disturbance in the molecular constitution of the blood—a disturbance which perhaps (through the agency of operations to be hereafter discovered) influences certain classes of vessels, while others remain exempt. It may be also readily conceived that such disturbance may be perpetuated after the withdrawal of the exciting cause, and that hence an explanation may be afforded of the occasionally fatal effects of poisons at periods posterior to their separation from the blood, or their final elimination from the system.

The late experimental investigations of Dr. Blake, and of Frierichs and Wöhler, as to the absolute or comparative innocuity of arsenic acid (which is isomorphous with, and hence capable of replacing the phosphoric acid of the blood, without subversion of the molecular constitution of that fluid), and the harmless nature of that singular organic compound of arsenic, the cacodylic acid, afford some countenance to the views above stated, which I desire to be considered in the light of suggestions only.

CHOLERA PRODUCED BY EFFLUVIA FROM THE DEAD.—BY DR. REEB, U. S.

DURING the epidemic by which our city and its vicinity so severely suffered, there were but very few cases at Bellevue, notwithstanding the large number of patients here, and of a class signally predisposed to become its victims. The preventive measures relied on were a strict attention to cleanliness in and about the entire

day, although I found it in the liver. The recent researches of M. Millon (*Annales de Chimie*) on antimony, demonstrate that the latter substance remains (in some cases of tartar emetic poisoning) in the structure of the bones for several weeks, and also occasionally in the subcutaneous fat. I searched for arsenic in the latter quarter in the above case without success.

premises; thorough and persevering ventilation of all the wards; the extensive use of ice and iced water; and a careful regulation of the dietary habits of all the inmates. Good dry sugar was at once substituted for molasses, and better supplies of food, both in quantity and quality, were forthwith provided. Very little medication of any kind was permitted, and all cathartics were forbidden. When the premonitory diarrhoea began to exhibit itself, it was made the duty of the orderly or nurse of every ward to notify the assistant-physicians of every instance in which looseness of the bowels was observed; and, at stated hours, a medical inspection of all the patients was made both day and night to detect the earliest symptoms, which there was a disposition to conceal. The prescribed antidotes were placed in every ward, to be in readiness at all times; and all the inmates were instructed in the importance of early treatment in case of an attack.

For several weeks, and even after the neighbouring institutions in Blackwell's Island were visited with the epidemic, the hospital at Bellevue had wholly escaped; nor is it probable that we should have had a single case had not the atmosphere of the entire premises been rendered impure and offensive to the last degree by the contents of the dead-house for the whole city, which is most improvidently located within a few yards of the hospital. At the climax of the epidemic, more than a hundred bodies were deposited here in a single day, whence they were to be transported in boats to "Potter's Field;" but the means of removal being inadequately provided, they were allowed to accumulate; and the effluvia became so offensive, that it was necessary to close all the windows opening towards the dead-house; and even then the stench was intolerable. All ventilation of the wards being thus precluded, one, and then another, case of cholera began to appear, and the premonitory diarrhoea now became almost universal throughout the house, a score or more being thus seized in a single night.

It was not until a number of deaths had occurred, including the keeper of the dead-house, and the gate-keeper who was located near it, that our remonstrances were heard; and the necessary provision to empty the dead-house, by removing the bodies every evening, could only then be obtained. From that time the cases were few, and occurred at intervals of several days, so that all apprehensions of its becoming epidemic here were at an end. The whole number of cases during the summer did not exceed thirty, and several of these were brought hither. The persons attacked were chiefly broken down previously by age, intempe-

rance, or chronic disease of long standing, so that a majority of all the collapsed cases were fatal. Several of them had no premonitory symptoms whatever, while others concealed their symptoms until in a hopeless condition. The few recoveries were in persons who were treated internally by large doses of calomel, repeated every two or three hours, varying from ten to twenty grains, and usually in combination with capsicum, or camphor, or both; large quantities of ice were eaten meanwhile, and the surface of the body covered with heat and stimulants. In other cases, however, it must be conceded that this identical treatment failed; but without it none recovered. A slight pyralism followed in three cases, one of which afterwards died of consecutive fever. Only two of our cases were bled, and one of these recovered. I have notes of all the cases, numbering twenty-nine, of whom eight only were saved.

Our success in arresting the diarrhoea was very nearly universal; and, indeed, only one case of death occurred when the premonitory symptoms were discovered. There was no novelty in the treatment of these, our reliance being chiefly on rest, ice internally, with heat to the skin, and an occasional dose of ether, *sp. lavend. c.*, and vinum opii, mixed in suitable proportions, which was found sufficiently astringent, cordial, and anodyne for our purpose. The tinct. capsicum was occasionally tried in various combinations, but was found to offend the stomach, so that even landanum was rejected when mixed with it, and retained only when given alone.

Whole number of cases from June		
10th to July 26th	137	
" " recoveries	47	
" " deaths	90	
Rate of mortality, 67 per cent.		

—*American Journ. of Med. Sciences*, 1850.

CARIES OF THE CRANIUM.

A WOMAN, sixty-three years of age, suffered from caries of the cranial bones, the consequence of syphilis. The upper and under surface of the bone was deeply furrowed, as if worm-eaten, and the os frontis was perforated. The diploë was thickened and indurated. On the inner surface of the bone there was seen an exostosis—the *osteophyte* of Rokitsansky. This patient had probably formerly been the subject of rickets.—*Casper's Wochenschrift*. X

RELATIONS BETWEEN ANGINA PECTORIS AND ASTHMA.

THE intimate relations between asthma and angina seem to have been perceived by most writers on the latter, as the diagnostic marks of both have been given together.

Darwin has given angina the name of painful asthma.

Both come on by sudden paroxysms, at irregular intervals. In both there is a sense of painful constriction and suffocation, of epigastric tension with eructations, yawning, coldness and paleness of the face, the pulse comparatively unaffected, difficulty of speaking, the health good in the intervals, until the arrival of secondary causes of dyspnea (usually emphysema, or other symptomatic asthma). Both are alike brought on and influenced by atmospheric changes; by the action of a cold dry wind; by mental emotions; perhaps also by hereditary predisposition. Both have been ascribed to the same lesions of the respiratory and circulating systems, even to ossification of the costal cartilages, coronary arteries, and cardiac valves: both are relieved by the same treatment, narcotics, antispasmodics, and revulsives. Many cases of angina are recorded in which symptoms of asthma first appeared, and disappeared in the course of the disease: viz., fits of nightly suffocation, after the first sleep, which compelled the patient to fly to an open window for relief.

The differences between angina and asthma may be explained by the different functions of the filaments of sensation and motion in the par vagum. In asthma the dyspnea is more characteristic than pain; every muscular power is brought into action to introduce air into the lungs; the diaphragm and pectorals contract convulsively; the desire for air is extreme, and the patient seems continually on the point of suffocation. During the paroxysm there is more or less absence of the respiratory sounds, from the difficulty the air finds in entering the pulmonary vessels; hence the violent and noisy laryngeal respirations. When the fit ceases there is copious expectoration, with mucous râles, and cough. The muscular fibres of the bronchi and their ramifications are animated by the spinal accessory nerve; the dyspnea of asthma depends on the spasmodic contraction of these fibres, the partial obliteration of the air-passages requiring violent respiratory efforts to secure the necessary introduction of air; in this constriction the larynx is involved, as evinced by the whistling of the inspirations; the non-introduction of air into the pulmonary vesicles accounts also for the diminution or even absence of the respiratory murmur.—*Dr. Kneeland; in American Journal of Medical Sciences, 1850.*

BOOKS & PERIODICALS RECEIVED FOR REVIEW,

(The List will be given in our next No.)

BIRTHS & DEATHS IN THE METROPOLIS

During the Week ending Saturday, July 27.

BIRTHS.		DEATHS.	
Males.....	554	Males.....	439
Females..	680	Females..	459
	1234		898

CAUSES OF DEATH.

ALL CAUSES.....	898
SPECIFIC CAUSES.....	405
1. Zymotic (or Epidemic, Endemic, Contagious) Diseases....	273
Sporadic Diseases, viz.—	
1. Dropsy, Cancer, &c.....	27
2. Brain, Spinal Marrow, Nerves, and Senses.....	104
4. Heart and Bloodvessels.....	33
5. Lungs and organs of Respiration	74
6. Stomach, Liver, &c.....	58
7. Diseases of the Kidneys, &c.....	14
8. Childbirth, Diseases of Uterus, &c.	8
9. Rheumatism, Diseases of Bones, Joints, &c.....	6
10. Skin.....	1
11. Old Age.....	31
12. Sudden Deaths.....	1
13. Violence, Privation, Cold, &c.....	28

The following is a selection of the numbers of Deaths from the most important special causes:

Small-pox.....	5	Convulsions.....	29
Measles.....	21	Bronchitis.....	30
Scarlatina.....	23	Pneumonia.....	30
Whooping-cough.....	25	Phthisis.....	121
Diarrhoea.....	104	Lungs.....	4
Cholera.....	13	Teething.....	13
Typhus.....	35	Stomach.....	3
Dropsy.....	12	Liver.....	7
Hydrocephalus.....	26	Childbirth.....	2
Apoplexy.....	19	Uterus.....	4
Paralysis.....	18		

REMARKS.—The total number of deaths was 723 below the average mortality of the 30th week of 44 previous years, although 55 above the mortality of the preceding week. The deaths from diarrhoea have undergone a great increase; but of the 104 recorded, 84 occurred in infants under 1 year, and 6 among persons above 40. Of the 13 deaths from cholera, 3 are marked as "Asiatic." The deaths from this disease were not, however, so numerous by one-half as in the corresponding week of 1846.

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer.....	29.49
Thermometer.....	63.4
Self-registering do.	Max. 59° Min. 63°
From 12 observations daily. Sun.	

RAIN, in inches, 0.83.—Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was 2.4° above the mean of the month. The week was chiefly remarkable for the quantity of rain which fell.

NOTICES TO CORRESPONDENTS.

Dr. A. H. Hassall.—We much regret that, owing to the length of the extract accompanying it, we cannot find room for the letter this week. It shall appear in the following number.

The papers of Dr. E. J. Tilt, Mr. Jennette, Mr. S. Bate, and Mr. Grantham, with the letter of Mr. R. H. A. Hunter, next week.

The valuable communication of Mr. Pearson and Dr. Curtis, on the Poisonous Properties of Lobelia inflata, stands next for insertion.

Dr. Snow's paper on Chloroform has been received.

Mr. Kodalle's contribution on Mesmeric Operations will be inserted.

Lectures.

COURSE OF LECTURES
ON
DISEASES OF THE HEART.

*Delivered at St. Vincent's Hospital during the
Session 1849-50.*

By O'BRYEN BELLINGHAM, M.D.

Fellow of, and Member of the Court of Examiners
of, the Royal College of Surgeons in Ireland, and
one of the Medical Officers of the Hospital.

LECTURE IX.

EXAMINATION OF THE HEART IN DISEASE,
CONTINUED.

Signs furnished by Auscultation—Alterations in the intensity and tone of the sounds of the heart—Alterations in the duration of the sounds of the heart—Alterations in the limits within which the sounds of the heart are audible—Alterations in the frequency of the sounds of the heart—Alterations in the rhythm of the heart's sounds—Alterations in the number of the heart's sounds.

Signs furnished by Auscultation.

AUSCULTATION, like percussion, may be either immediate or mediate. Although immediate auscultation is preferred by some, there are several objections to it. Thus, in the case of females it is indelicate; in dirty persons it is disagreeable; while in contagious diseases it is not without risk: besides, there are some situations in which either the ear cannot be applied, or in which the stethoscope is much more convenient. In examining the præcordial region in cases of valvular disease, or where there is a suspicion of it, mediate is always to be preferred to immediate auscultation: indeed, the exact situation, or the limits of an abnormal sound, can in the majority of cases be satisfactorily determined only by the assistance of the stethoscope.

As the sounds heard in a healthy state of the heart must be the standard of comparison in judging abnormal sounds, it is of course necessary to be familiar with them before commencing the examination of cases of disease; yet this is a matter frequently overlooked by the student. He begins by examining the cases of disease of the heart which come under his observation in hospital, without, perhaps, any previous knowledge of its normal sounds. Now it is scarcely necessary to observe that, unless the character, duration, intensity, and other qualities of the normal sounds of the heart, are known, the student is not in a condi-

tion to appreciate the numerous modifications or alterations which these sounds undergo in disease. It would, in fact, be almost as reasonable to commence the study of anatomy by entering at once on pathology, without any knowledge of the healthy appearance of the organs, as to commence the practice of auscultation by examining cases of disease.

In cases where the heart is diseased, or suspected to be so, it is often advisable, before concluding the examination, to make the patient walk quickly up and down stairs, or backward and forward in the room, with the view of accelerating the circulation and increasing the heart's action; by which, abnormal sounds will be often rendered more evident, or a murmur which was inaudible previously may become distinct. Thus, a musical murmur is sometimes audible at the mitral orifice only, when the action of the heart is increased by exercise, a simple bruit de soufflet replacing it as the circulation becomes tranquil. When a murmur, which was inaudible as long as the circulation was tranquil, becomes evident when the heart's action is increased, it is highly probable that there is, as Dr. Latham* observes, "a mechanical obstacle at an orifice of the heart, but that it is of small amount, not enough to cause the requisite degree of vibration when the current of the blood is slow and undisturbed, but quite enough when it is more rapid and forcible."

Again, in some cases it may be necessary to examine the patient in the sitting or erect as well as in the recumbent position, or to make him lean forwards. Thus, in a case of pericarditis which we had in the hospital, the friction-sound at one period became nearly inaudible in the recumbent posture, although sufficiently evident in the sitting posture, and still more so when the patient leaned forward. Or the friction-sound may diminish in intensity in the sitting posture, and become well marked in the recumbent position; obviously depending on the difference in the situation in which the lymph has been deposited.

It may, in addition, be sometimes necessary to make the patient hold his breath for a few seconds at a time during the examination, particularly if a murmur is not well marked, or if the auscultator is inexperienced, or if the sonorous or sibillant râles of bronchitis are loud. Lastly, if any difficulty should be experienced in distinguishing between the first and the second sound, or in determining to which a murmur should be referred, the finger ought to be kept on the radial pulse, or, better still, upon the carotid artery, during the exami-

* Lectures on Diseases of the Heart, vol. i.

nation; and the stethoscope should be applied on the right side of the sternum, and the sounds of the right cavities traced towards the left side.

Ismaeac has made the remark that, when a portion of lung is interposed between an enlarged heart and the parietes of the chest, the impulse of the organ, by compressing the lung and expelling the air from it, may give rise to an abnormal sound resembling some of those heard in cases of disease of the heart. "A strong impulse of the heart on a portion of lung may (as Dr. Williams* observes) forcibly press the air from it; and, if there happen to be any partial obstruction or mucus in its tubes, a short sibilant or mucous rhoncus may accompany each beat." "The character of these additions, and the circumstance that they accompany this breathing also, and are more or less diminished by holding the breath, or by posture altering the manner in which the heart beats on the lung, may serve to distinguish them from the true cardiac sounds."

Again, it sometimes happens in the advanced stage of phthisis that each impulse of the heart is accompanied by a tinkling sound, resembling the tintement métallique. This occurs in cases in which a large tubercular cavity, or two or more cavities communicating with one another, containing a certain amount of fluid, occupy the greater portion of the upper lobe of the left lung, its lower lobe being at the same time solidified by tubercular deposition, and when the action of the heart is strong. The sound here may accompany both the impulse of the heart and the respiratory movements. In a case which was in the hospital some time since (where this abnormal sound was sufficiently loud to be audible without applying the stethoscope, and at some distance from the patient's bed), it accompanied only the impulse of the heart—not the respiration, during a portion of the time that the patient was under observation.

In diseased states of the heart, its valves, or orifices, the sounds are variously modified or altered. Thus, in some instances, their intensity is greater or less than natural; in others their character or tone is altered; and in others their duration is affected. Sometimes the extent of surface over which they are heard is increased or diminished, or they may become more frequent, sometimes less frequent than natural; or their rhythm may be variously disturbed. Lastly, and in a very numerous class of cases, new sounds are superadded, which either accompany the normal sounds or take their place.

Alterations in the intensity and tone of the sounds of the heart.—The intensity of the heart's sounds varies in different individuals, and under different circumstances: varieties are likewise met with in the character of its sounds, some of which are observed in cases of disease, while others occur independently of any morbid change in the heart. Thus, in persons in whom the parietes of the thorax are loaded with much fat, or where these parts are cedematous,—in muscular subjects in whom the chest is largely developed, and whose lungs are ample,—the sounds of the heart have less intensity than in individuals in whom the chest is narrow or deformed, in whom its coverings are thin, or in whom the cartilages of the ribs are ossified. Whenever, likewise, the energy of the heart's action is increased, the sounds, as a general rule, become loud. Thus, in nervous and hysterical subjects,—in palpitation from any cause, under the influence of mental emotion, or after exercise,—its sounds become more intense than natural. On the other hand, when the energy of the heart's contraction is lessened from any cause, the intensity of the sounds will be diminished.

The intensity or the tone of the sounds of the heart is frequently modified by disease; but this applies more particularly to the first than to the second sound: thus, when the walls of the ventricles are hypertrophied, the first sound becomes more obtuse, or duller than natural;—when the cavity of the ventricles is dilated, the first sound is said to become clearer than natural;—when the walls of the ventricles are attenuated, combined with dilatation of their cavities, the first sound somewhat resembles the normal second sound, while the second sound is feeble;—when the muscular tissue of the heart is flabby or softened, both sounds become more feeble and obscure, besides undergoing other changes afterwards to be mentioned. In the advanced stage of fever accompanied by considerable debility, the sounds of the heart sometimes become extremely feeble; during the state of syncope, also, its sounds are feeble and indistinct, and the second sound is often inaudible. In dilatation combined with some hypertrophy of the ventricles, both sounds are loud; when, however, the hypertrophy predominates over the dilatation, and this diseased state has arrived at an extreme degree, both sounds, but particularly the first, become obscure.

Clearness of the first sound of the heart is very generally considered to be an indication of dilatation of the ventricles; this, however, must be taken with some limitation, for this character of the first sound is often observed when the cavity of

* On Diseases of the Chest.

the ventricles is not dilated, as well as in cases where the parietes of the ventricles are considerably hypertrophied. Dr. Olendinning* says he has found the first sound of the heart to be abnormally short, shrill, and clear, approaching the character of the normal second sound in numberless instances, in hearts proved post-mortem to be much and even enormously hypertrophied. According to him, "a short, clear, systolic sound indicates not so much any particular anatomical state of the heart; but rather a defective dynamic condition; that it indicates, in a word, not *attenuation* of the parietes, but merely *debility*." M. Piorry likewise attributes little importance to difference in the clearness of tone of the sounds of the heart, as a sign of hypertrophy or dilatation of the ventricles. He says he has "frequently found the parietes thickened when the sound was clear, and vice versa, and the tone frequently varies in the same person if subject to palpitation." He attributes clearness of tone to the amount of blood contained in the cavity, the sound being clearest when the heart is most empty, yet is contracting with energy, as in palpitation.

Alterations in the duration of the sounds of the Heart.—The sounds of the heart are modified or altered in their duration as well as in their intensity in some diseased states of the organ. Thus, in hypertrophy of the walls of the ventricles, the first sound is more prolonged than natural; in dilatation of the ventricles, the first sound is shorter than natural. "The transition of a thick muscle from slack to tight can never be so complete and sudden, Dr. Williams† observes, as that of a thin one; where there are many fibres, they cloke and muffle each other's vibrations, hence the first sound is dull and prolonged;—a thin ventricle, for the same reason, will give a louder and sharper sound." But if a thick ventricle takes a longer time to contract upon its contents than a thin ventricle, the blood must obviously pass out of the cavity more slowly in the former than in the latter; and as sound appears to be produced during the entire period that the blood is being transmitted from the ventricle, the first sound must necessarily be prolonged in this diseased state.

Alterations in the limits within which the sounds of the heart are audible.—The extent of surface over which the sounds of the heart are audible on auscultation in the healthy and well-formed chest, is not great, being pretty nearly limited to the

præcordial region, and as the stethoscope is moved from this part they become more and more indistinct. There is, however, considerable difference in this respect, in different subjects; in individuals whose chest is covered with much fat, and in whom the impulse is slight, or hardly perceptible, the sounds are more limited; while in thin subjects, in persons with narrow chests, in children and females, in individuals whose chest is contracted and deformed, and in nervous subjects, the sounds are audible over a wider area. The sounds produced at the right side of the heart are more perceptible upon the right side of the præcordial region, while those produced in the left cavities are more distinct at the left side of this region. When the sternum is short the sounds of the heart are often audible likewise in the epigastric region.

In several diseased states of the heart or of the lungs, the sounds become audible over the greater part of the anterior surface of the chest, and sometimes in addition in the lateral and posterior regions upon both sides: while in other morbid states the sounds are very obscure, even in the præcordial region. Whenever the intensity of the sounds is increased, or whenever the heart itself is enlarged, its sounds, as might be expected, are audible beyond their normal limits, provided the circulation through the heart and lungs continues free; hence in dilatation of the ventricles, and in dilatation combined with hypertrophy, the sounds are widely diffused. In palpitation from any cause, particularly in anæmic, nervous, and hysterical individuals, the sounds of the heart will be heard over great part of the anterior surface of the chest, particularly if the subject is emaciated.

The sounds of the heart, however, frequently become audible on auscultation beyond their normal limits, even over the greater portion of the chest, independent of any alteration of the heart. This happens whenever the density of the pulmonary tissue is increased, by which its power of conducting sound is augmented; hence, in solidification of the lungs from hepatisation, from tubercular deposition, or from any other cause, the sounds of the heart will be audible to a considerable distance beyond the natural limits of the præcordial region; and this very extension of the heart's sounds becomes a useful sign in some diseases of the lungs. In displacement of the heart the situation at which the sounds are heard will be of course altered; but, as the seat of the impulse is likewise changed in such cases, it need not be dwelt upon here. A large amount of fluid in the pericardium, or an emphyse-

* Croonian Lectures, Med. Gaz. vol. xxv.

† On Diseases of the Chest.

matous condition of the margin of the lungs, might prevent the sounds of the heart from reaching the ear; but there are other signs by which these diseased states may be more readily diagnosed.

Alterations in the frequency of the sounds of the heart.—The sounds of the heart may be more frequent than natural, or they may become slower than natural, or they may be intermittent or irregular. They are more frequent than natural in all inflammatory affections of the heart or its coverings, as well as in numerous diseases of other parts, in states of debility, in cases of anæmia, or whenever palpitation arises, whatever be its cause. The heart's sounds much less frequently become slower than natural; when they do, the cause lies more frequently in disease of the nervous centres than of the heart. Fatty degeneration has been supposed by some to cause a diminution in the frequency of the heart's action. That it does so appears to me to be doubtful. When slowness of the pulse has accompanied this diseased state it probably depended upon idiosyncrasy, or some other cause. In hypertrophy of the ventricles the heart's action is sometimes slow. It is not, however, an ordinary occurrence, because, although the first sound is more prolonged than natural, the interval of repose is usually shortened in proportion. Dr. Willis* observes that, when considerable contraction of the aortic orifice exists, the pulse becomes much slower than natural: "here the ventricle, engaged in squeezing its charge through a hole little or no larger than a goose-quill, perhaps, has been found contracting with no greater frequency than twenty-five, twenty, and even sixteen times in a minute." It is not easy to understand how contraction of the aortic orifice can render the action of the heart slow, although it may, no doubt, considerably diminish the amount of blood transmitted at each systole of the ventricle.

Alterations in the rhythm of the heart's sounds.—In all the cases which we have been hitherto considering, whether the sounds of the heart were increased or diminished in intensity, whether they were more frequent or slower than natural, the rhythm of the organ was regular; the two sounds succeeded one another, and were followed by an interval of repose, which varied in length according to the duration of the previous systole, and according to the rapidity with which the sounds succeeded each other. It not unfrequently happens, however, that after a certain number of perfectly regular beats, a sudden

pause or silence occurs; the heart's action and sounds appear to be arrested or suspended for a moment, and then go on regularly, as before, when, after a certain number of beats, the same phenomenon is repeated. This constitutes intermittence or intermission of the heart's action; and it may occur after every fourth, eighth, or tenth beat, or at longer or irregular intervals. It is occasionally observed in individuals who are otherwise in perfect health: but it also occurs in diseased states of the valves or orifices of the heart, where either some impediment exists to the direct passage of the blood, or where regurgitation is permitted.

In the ordinary theory of the mechanism of the heart's sounds intermission is with difficulty explained; but if it be admitted that the sounds of the heart have their cause in friction between the blood and the parietes of the orifices of the heart, we can easily understand how, in certain diseased states, so little blood may be expelled from the left ventricle; or it may be propelled with so little force that there is not friction sufficient to generate a sound, or force enough to produce an impulse, or to communicate a pulse to the radial artery. We know that the circulation may go on when the sounds of the heart are scarcely audible, and when no impulse can be felt in the præcordial region, as in syncope; yet, in such cases, the valves must perform their functions, and the muscular walls of the ventricles contract and dilate.

It sometimes happens that every systole of the left ventricle is not sufficiently strong, or the amount of blood transmitted by the left ventricle is not sufficiently great to communicate an impulse to the radial artery, and the pulse intermits: but, on applying the stethoscope to the præcordial region, the sounds are still heard, although more feebly, and there is no real intermission of the heart's action. If this should occur at every second ventricular systole, the pulse will appear to be preternaturally slow: and this probably is the true explanation of some of the cases of slow pulse which have been recorded. The error would have been corrected by applying the stethoscope to the præcordial region.

In certain diseased states of the muscular tissue of the heart, of its valves, or orifices, the rhythm of the heart's sounds undergoes still further alteration: the relative duration of the sounds to the interval of repose is altered, the latter being increased or diminished; while the double sound of the heart is rapidly repeated twice or thrice, followed by two or three slower repetitions of the same sounds, some of the sounds being strong, others weak (particu-

* MEDICAL GAZETTE, VOL. I. 1842-43.

larly the second sound, which is sometimes imperceptible), and accompanied by occasional intermissions. The rhythm of the heart's sounds is then said to be *irregular*. This is observed in cases where the ventricles become overloaded or over-distended with blood, which they are unable to expel; and where, consequently, the circulation through the heart or lungs, or through both, is impeded. It is observed in the advanced stage of several diseases of the heart, as in considerable contraction of the mitral orifice, in softening of the muscular tissue of the ventricles, in pericarditis with copious liquid effusion; or where polypiform concretions are developed in the cavities of the heart previous to death. It is of course a much more unfavourable symptom than simple intermission.

Alterations in the number of the heart's sounds.—Another alteration in the rhythm of the heart's sounds, occasionally observed in cases of disease, is where, instead of the ordinary double sound, we have a triple sound, as if each systolic sound was repeated twice for one diastolic sound, or each diastolic sound was repeated more than once for one systolic sound. In other cases both the systolic and diastolic sounds appear to be doubled, and four sounds are heard for one impulse of the heart. Lastly, it sometimes happens that only one sound is audible, in place of the ordinary double sound, and it is then always the second sound which is absent.

When we hear a triple sound in place of the ordinary double sound of the heart, it is more frequently owing to a reduplication of the systolic than of the diastolic sound. This reduplication of the first sound is compared by M. Bouillaud to the tattoo of the drum: it more nearly resembles what is called in Ireland "the Kentish fire." When the diastolic sound is doubled, the triple sound closely resembles the footsteps of a cantering horse heard at some distance, and to which Dr. Williams compared it.

This reduplication of the heart's sounds is seldom heard except in cases of disease, and, in the majority of instances, some abnormal sound is audible at the same time. Sometimes this phenomenon is only observed when the heart's action is quickened, and the sounds become regular as the action of the organ becomes tranquil. Some physiologists have endeavoured to account for this reduplication of the heart's sounds by supposing a want of synchronism between the movements of the right and left ventricles; but, from the manner in which the muscular fibres of the two ventricles are arranged, it is difficult to understand how their parietes could contract or dilate

alternately. An additional sound is sometimes produced by the apex of the heart impinging against the inferior edge of the fifth left rib during the systole of the ventricle, which, however, is not to be confounded with the phenomenon under consideration.

ADULTERATION OF COFFEE DETECTED BY THE MICROSCOPE.

At a recent meeting of the Botanical Society of London, a paper was read by Dr. Hassall "On the Adulteration of Coffee." By the employment of the microscope the author had ascertained that the substances most frequently used in the adulteration of coffee are chicory, roasted wheat, colouring matter, and occasionally beans and potato-flour. The structure of the coffee-berry, and of the several products just named, was then minutely described, and it was shown that the chicory might at all times be distinguished with the greatest ease by the size and ready separation of the cells, as well as by the presence of bundles of vessels of the dotted or spiral kind. The substance so generally employed to deepen the colour of coffee Dr. Hassall found to consist, in those instances in which he had examined it, of burnt sugar; and he referred to the fact that the rich brown hue of coffee is not peculiar to a decoction of that berry, but that almost all vegetable substances, when charred, yield a somewhat similar colour. The author then proceeded to detail, in a tabular form, the results of 34 examinations of coffee of all prices. From these it appeared that the whole of the coffees, with two exceptions, were adulterated; that chicory was present in 31 instances, roasted wheat in 12, colouring matter in 22, beans and potato-flour in one only; that in 10 cases the adulteration consisted of but a single article, in 12 of two, and in 10 of three substances; that in many instances the quantity of coffee present was very small, and in others not more than a fifth, fourth, third, half, and so on. Contrasting chicory and coffee, it was observed, that while the coffee-berry contains a quantity of essential oil, visible in small drops in the cells, and upon which the fragrance and the active properties mainly depend, not a trace of any similar oil is to be found in the chicory-root. It was advised that the coffee should be ground fine, in order to facilitate the liberation of the essential oil contained in the cells of the berry, and that an infusion, and not a decoction of it should be made.

Original Communications.

BRIEF NOTES ON THE DISEASE,
INDIAN VILLAGE CHOLERA,

AND ITS TREATMENT.

BY ASSISTANT-SURGEON MOORE, B.A.
Gwalior Contingent.

[Concluded from page 187.]

PART V.

Cholera a non-contagious disease: its origin.—The impression abroad is erroneous, that cholera is a visitation of recent date. Mankind, lay and professional, may rest assured that, from the hour the Almighty Creator of the universe breathed into man's nostrils the breath of life, the mucous membrane, and subjacent tissues of his stomach and intestinal canal, were as likely to be attacked with diffused inflammatory action as in this, the year of our Lord 1849-50. The public may rest assured that, so long as this world is peopled with human beings, or with a brute creation possessed of a stomach and an intestinal canal, their structures will continue to secrete serum, mucus, and lymph; and through their sieve-like tissues will percolate the saline ingredients of the blood. This, with its concomitant symptoms, is cholera: whether it occur in man or in beast, the results are nearly the same.

In the Israelitish camp, 1471 years before the commencement of the Christian era, cholera must have made its appearance. In the encampments of the tribes of Israel, thousands were swept away in the space of a few hours by a disease incorrectly translated the "plague." The term is incorrect, and has caused confusion. In the record of the miracles wrought by Moses, the Israelitish law-giver, in the presence of Pharaoh the king of Egypt, the term "plague" has been so far correctly used by the translators of the Old Testament, that at the present time the disease is recognised and described under the same name. There can be no difference of opinion as to the Egyptian "plague" of 1849 being precisely the same form of disease as the plague with which the land of Egypt was scourged in the reign

of Pharaoh. The description of the plague-proper, in the 19th chapter of Exodus, is accurate. The disease is described as "a boil breaking forth with blains upon man and upon beast, throughout all the land of Egypt." The translation of the second form of disease by the term "plague" is erroneous, because there is no form of epidemic which could have broken out suddenly in an encampment of the Israelites, which could have swept them away by thousands, and by tens of thousands, in the course of twenty-four or forty-eight hours, which could have disappeared entirely, or ceased as suddenly and unexpectedly, as it developed itself, save one,—that epidemic was, and is, cholera.

Divines, the ministers and interpreters of the religion of Christ, may not coincide in these views. Whether they assent or dissent, the question still remains one of opinion. It must be confessed, however, that the sudden and unaccountable cessation of the disease or plague in the camp of the Jews, after it had thinned their ranks, is strong presumptive evidence in favour of the identity of the Jewish epidemics in 1471 and 1472 B.C. and our annual epidemics in Indian villages, towns, cities, and cantonments. The sudden change from pestilence and death to a state of health, attributed to the exertions of Aaron, then, as now, may have originated in a shift of the Jewish camp,—in a change of wind,—a heavy fall of rain,—or in a thunder-storm, by which the pestilential state of the atmosphere was dispelled.

The passages in Holy Writ referring to the epidemic translated "the plague," may be found in—

Numbers, chap. xvi., v. 46, et seq.—
Year B.C. 1471; Deaths, 14,700.

46. "And Moses said unto Aaron, Take a censer, and put fire therein from off the altar, and put on incense, and go quickly into the congregation, and make atonement for them; for there is wrath gone out from the Lord: the plague is begun.

47. "And Aaron took as Moses commanded, and ran into the midst of the congregation; and, behold, the plague was begun among the people: and he put on incense, and made an atonement for the people.

48. "And he stood between the dead

and the living; and the plague was stayed.

49. "Now, they that died in the plague were fourteen thousand and seven hundred, beside them that died about the matter of Korah."

Numbers, chap. xxv., v. 8.—Year b.c. 1452; Deaths, 24,000.

8. "And he (Phinehas) went after the man of Israel into the tent, and thrust both them through,—the man of Israel and the woman, through the belly; so the plague was stayed from the children of Israel.

9. "And those that died in the plague were twenty and four thousand."

II. Kings, chap. xiv., v. 35.—Year b.c. 710; Deaths, 185,000.

35. "And it came to pass that night, that the angel of the Lord went out, and smote in the camp of the Assyrians an hundred and fourscore and five thousand: and when they arose, early in the morning, they were all dead corpses.

36. "So Sennacherib, king of Assyria, departed and went, and returned and dwelt at Nineveh."

In the year 1471, before the birth of Christ, Aaron is represented as standing between the living and the dead, and the plague was stayed. The expression is metaphorical. The description of the staying of the plague, in this written record, is oriental in its origin. Those who have witnessed, or have formed part of an Indian encampment, where 80 or 100,000 human beings have been congregated together, will at once perceive, that a separation between the living and the dead could be effected solely by the march of the living from the scene of death. In the march of the Israelitish camp, and consequent shift of ground, the pestilential locality and its victims—the dead corpses, in Scriptural language—were left in the rear.

There is nothing wonderful nor miraculous in an event of this nature. The instinct of self-preservation urged the Jews to a hasty march from an encamping ground, to have remained in which would have been certain death. The strongest, the stoutest, the healthiest, once seized with the epidemic, were smitten down in a few hours by an invisible death-stroke. The same occurs, yearly, in some one, part of the

Indian continent. In Lower Bengal I have resided within a few miles of villages where the cholera has broken out. When a fourth of the inhabitants had been swept away by its ravages, in the course of eight-and-forty hours, the remainder have rushed out in a body, with their wives, and children, and cattle. The villages have been deserted; a living soul did not remain behind in the ill-fated spot.

Time was allowed to elapse, until, in the opinion of their pundits, the wrath of the offended deities had been appeased, and the scourge had ceased. On their return to the villages they felt secure, and, as far as I could ascertain, seldom suffered from a fresh outburst of the cholera in an epidemic form. During their absence the state of the atmosphere, surcharged with noxious pestilential vapours, had undergone a change. Hence the cessation of the cholera, and the freedom from its attacks enjoyed by the villagers, after their return.

Holy writ does not stand in need of facts, such as the recorded epidemics in the Israelitish camp, so obvious and so easily explained, being distorted by the views of divines, and shrouded in the mysteries of miraculous agency. Holy writ does not need to exact from mankind an unconditional belief in the interposition and operation of miraculous agency, as to the origin, progress, and termination of a terrific epidemic scourge, when its sudden outburst and its sudden cessation can on reflection be accounted for in a manner perfectly rational, and consistent with the principles of religion.

The studied endeavours of writers to prove that cholera is a disease of recent development amongst the human race, have induced me to refer to the epidemics in the camp of the Israelites during their journeyings to the land of promise, as reasons for dissenting from their views. The fatal scourge or plague in the camp of the Jews must have been cholera, and no other form of disease.

This leads me to the non-contagious character of cholera.

The experience of the majority of writers and of observers, professional and non-professional, has established the fact that cholera is dependent upon, influenced by, and propagated through the medium of certain atmospheric

changes. Their observations prove that cholera does not spread by contagion, nor by infection—that is, the disease is not communicated from individual to individual by direct contact, but spreads and commits its ravages through the influence of the atmosphere.*

Others there are who entertain a different opinion. This class of theoretical contagionists is not numerous: their theories even are not well grounded: their views of the origin of cholera as an epidemic or as an endemic disease, are not comprehensive nor consistent: the facts upon which they ground their opinions, as to the contagious and infectious properties of cholera, tell equally in favour of the opinions against which they combat. In the list of contagionists figures conspicuously the name of Professor Graves, of Dublin. His reputation has been damaged by the obstinate tenacity with which he adheres to the erroneous views expressed as to the contagious and infectious properties of cholera in his published articles.

The matured opinion of practical

* After the second death from cholera the prisoners were removed from the jail into tents, and marched off to a short distance from cantonments. The jail was closed, whitewashed, and purified. The prisoners suffering at the time from the effects of cholera, or seized with the disease afterwards, were transferred to the Regimental Hospital. If cholera were infectious or contagious,—if a fretted and irritable, a crimsoned efflorescence, and sero-mucous eliminating condition of the mucous membrane from the stomach to the rectum were a disease communicable from one person to another, by contact or close vicinage,—the transfer, being fraught with danger to the sepoys in the hospital, would have been unjustifiable and censurable.

Contagionists would have drawn a quarantine boundary line round the hospital, and would have interdicted all intercourse with persons passing to and from the jail.

The hospital was crowded with patients. In the western verandah, when the centre ward and eastern verandah were full, several sipahis lay on charpoys between prisoners in the last stage of cholera. A single case of cholera did not occur among the sipahis who were in hospital at the time of the transfer. Nor did the disease exhibit itself in a single case admitted at any subsequent period with fevers, or ulcers, or contusions, or cutaneous eruptions. The case of Bahadar Sing, marked No. XLIII. in the Abstract, cannot be regarded as an exception.

The use of tartar emetic in the treatment of intermittent fevers was suspended for a short time, in consequence of vomiting and purging having supervened in a few cases. Tartar emetic, in its effects on the constitution, was more decided and more speedy in its operation than at any time previous to the outbreak of cholera. If such effects can be attributed to the transfer of the cholera patients from the jail to the regimental hospital, why be it so. The inconvenience was temporary, and was easily remedied.

observers in India has set the question at rest, that cholera,—the true type of the Indian village cholera,—originates in and travels from locality to locality, not through the medium of personal contact, but through the medium of an impure, a contaminated, an infected atmosphere: that such is the medium has been incontestably proved. The changes in the atmosphere satisfactorily account for the cessation, as well as for the prevalence of this form of disease during certain months of the year in India. They are sufficient to account for the outbreak of the same form of cholera in any other part of the globe besides India. Farther, the cause assigned for its outbreak, without reference to the theories of contagion and infection, accounts satisfactorily for the peculiarities and eccentricities of cholera, starting into existence in one region of the globe,—travelling from thence by forced marches,—becoming diffused over the earth's surface,—ravaging every country by turn, and sparing neither sex nor age in its progress.

With facts such as these in prominent relief before our eyes, we may reasonably inquire, of what value, of what practical utility, are quarantine laws?

Quarantine laws have already doomed to death healthy and unhealthy alike, crowded together in the close, ill-ventilated holds of vessels. In ignorance of the laws of disease, and through stupidity, when nations were panic-stricken, quarantine laws were framed as safeguards against the invasion of diseases regarded by executive governments as infectious and contagious, and exotic to the soil. With the sacrifice of human life on board of vessels undergoing quarantine, have these laws proved operative in protecting the inhabitants on shore from the outbreak of disease, erroneously supposed to be conveyed from one port to another in the hulls of trading vessels? Have the quarantine laws proved operative in shutting out from Great Britain, or from any port or state on the Continent of Europe, the malignant and erratic type of cholera, when the state of the atmosphere on shore was favourable to its development,—was favourable to its springing into existence without extraneous aid from the pestiferous holds of vessels afloat?

These are simple, straightforward

questions. Reply to them, if you can, advocates of the doctrines of cholera-contagion and of cholera-infection!

Past experience, sad and mournful though it be, has removed every doubt upon the subject, that quarantine laws have proved a gross delusion: they have proved ineffectual; totally inoperative as a safeguard against the invasion of cholera.

Quarantine laws, inoperative as a safeguard against the outbreak on shore of cholera, of typhus, of yellow fever, or of the plague, have inflicted the greatest miseries on the unfortunate passengers and crews, pent up in vessels, obliged to hoist the yellow flag. In the destruction of human life on board, quarantine laws have not proved inoperative.

Common sense has dictated, in the strongest and in the plainest terms, that when cholera or fever, the small-pox or the plague, have broken out on board-ship, the healthy and unhealthy should be separated without delay. Humanity—the laws of nature—point to the necessity of the speedy removal of the sick from the infected atmosphere on board to some healthy locality on shore. The experience of medical men practically acquainted with the essential character of the diseases for the exclusion of which quarantine laws are maintained in full force, has long since decided that, if the atmosphere on shore be infected, the detention of the healthy, the sick, and the dying, in the holds of vessels undergoing quarantine, cannot hasten nor retard the outbreak of epidemic diseases on shore. Experience has also set at rest that, if the atmosphere on shore be not infected—if the atmosphere on shore be not surcharged with noxious, pestilential vapours,—the removal of the sick and healthy from vessels afloat, as well as the discharge of their cargoes, cannot produce the changes in the atmosphere generative of those epidemic diseases against which quarantine laws have been framed.

That cholera owes its origin to, and starts into existence from, certain changes in the atmosphere, scarcely admits of a doubt. With the periodic changes in the seasons, and at those seasons of the year when strong and regular currents of wind do not sweep over the face of the land, cholera may rage as an epidemic or as an endemic disease. In this respect cholera is not

singular. At such time, when the purifying influence inherent in and exercised by strong currents of wind in dispersing accumulated exhalations from the soil is suspended, as well as when the atmosphere is surcharged with the devitalising principles of carbon, of sulphuretted hydrogen, and of other devitalising noxious vapours, generated in and exhaled from the soil, other types of acute inter-tropical diseases,—fever, dysentery, or the plague,—may ravage whole districts with greater destruction to human life than the worst type of cholera.

Cholera, when it rages on ship-board, is subject to the influence of the sea-breeze. Sea-faring, as well as medical men, can testify to the modified character of the disease at sea. In the effects produced by the fresh sea breeze, a transition takes place from sickness and mortality on board to a state of comparative health. I have already stated that this was the case on board the ship *Sophia*, when I proceeded to Mauritius in that vessel, in medical charge of coolies. A few solitary cases may occur in the interval of the three days' sail from the Sand Heads; but the symptoms do not present the same intensity of character, nor the same rapid tendency to a fatal termination, noticed in those attacked off Calcutta.

Another observation is worthy of record. During the prevalence of the south-west monsoon in the Bay of Bengal, cholera is known to disappear from a vessel much sooner than in the north-east monsoon. The south-west monsoon blows fresh up the bay: the north-east monsoon blows down the bay, from the river and land. Hence it is not unreasonable to infer, nor do we travel far beyond the limits of probabilities when we conjecture, that the breeze blowing from the land still retains, and is strongly impregnated with, the poisonous miasmata generated in and exhaled from the decomposed vegetable matter, and from the low swampy ground on either side of the river Hooghly at its entrance.

In mountainous districts, and in districts situated at the base of a hilly range, and lying to leeward of the range, through which strong and regular currents of wind cannot circulate freely, cholera may and has become the endemic scourge of the inhabitants. In towns and villages, and cantonments

In the plains, where a system of thorough drainage has been neglected, or a slovenly system of half measures in drainage is the rule and not the exception, the recurrence of cholera year by year may be looked for as a periodical visitation.

Within the tropics, the rapid and pestiferous exhalations from the soil, combined with certain properties inherent in particular descriptions of soil, appear to be intimately connected with that state of the atmosphere which predisposes to an attack of the mucous membranes, in preference to subjecting to its industries other structures in the body.

Examples illustrative of this marked predisposition to attack the mucous membranes may be found in the forms of disease by which the mortality amongst European troops, encamped in or marching through jungly districts, has been caused. In general, statistical returns furnish data from which no other conclusion can be arrived at than that the mortality among the troops has been caused by diseases directly or indirectly connected with the mucous membranes. The troops have been swept away by diseases which have broken out in the shape of cholera, of dysentery, of gastro-enteric fevers, or in that form of disease closely allied in its symptoms to the Egyptian plague. Inquiries as to the causes of mortality amongst natives living close to those pestilential spots, confirm the conclusions that the diseases prevalent among men, women, and children, are those connected with the mucous membranes.

Why this should be is difficult of explanation. The same difficulty exists in accounting satisfactorily for the development of inflammatory action in the fibro-serous membranes in preference to other structures, under an altered state of the atmosphere. This will be more distinctly understood by bringing forward a practical illustration.

The coolies who had been located on the sugar estates in Berbice and Demerara were embarked in the ship *Louisa Baillie* for the port of Calcutta, without being provided with a supply of warm clothing for the passage. Avarice on their part, and parsimony on the part of the agents of the estates from which they were shipped, left the coolies in a state bordering on nudity, to undergo all the vicissitudes of the weather, from

a calm to a hurricane. The Executive Government of British Guiana did not interfere, as the colonial Treasury could not be saddled with such an expensive item as the supply of clothing to Indian labourers, imported into the colony a few years before at the risk and for the benefit of private speculators.

Thus matters stood. Such is the gist of a subject which afterwards formed part of an inquiry by a parliamentary committee, in connection with the emigration of Indian coolies into the British West Indian Colonies. With the exception of bowel complaints, caused by the intermixture of verdigris with their food, sickness on board was slight, and the deaths from sickness few in number, until we reached the Cape of Good Hope.

The supply of water on board was running short. On the 25th of July, 1843, we sighted the Cape of Good Hope. On the 26th we entered False Bay in $18^{\circ} 45'$ east longitude, and $34^{\circ} 23' 48''$ south latitude. In beating up the bay, we narrowly escaped closing our voyage to Calcutta on the Anvil Rocks. They are invisible, sunken rocks,—a terror to mariners. A ripple, and then a breaker, warned us of our close approach to danger. The Anvil Rocks were close under our lee bow. In the evening we cast anchor opposite Simon's Town.

On the 2d of August, with a fresh supply of water in the ship's hold, but without extra clothing on the backs of the coolies, we weighed anchor, hoisted the sails, and beat out of False Bay in the night, against a strong south-easterly wind. With her head steering towards the southward, the *Louisa Baillie*, freighted with her living cargo of coolies, proceeded on her voyage to Calcutta. It is necessary to be particular as to dates. With each degree of southing made, from the 2d of August to the 27th of the same month, the intensity of the cold increased. During this time hail-storms were frequent; the weather was freezing cold, the decks were covered with sleet; whilst the vessel kept scudding before the wind under a close reefed main-topsail.

The sufferings of the coolies were severe in proportion. But the structures attacked and brought under the influence of disease were not the mucous membranes, nor the parenchymatous tissues: they were the fibro-serous

membranes of the joints, and of the cavities of the abdomen; the chest, and the head. In two cases, which terminated fatally in the course of fifteen hours, the peritoneum, the pericardium, and the pleura, exhibited, in the post-mortem examination, a degree of intense vascularity. Their smooth and polished surfaces were coated with soft semi-gelatinous lymph recently exuded; their cavities contained serous fluid tinged with the red particles of the blood: flakes of lymph floated about in this serous fluid. The mucous membranes were healthy. The solid viscera were free from inflammatory action, with the exception of their fibro-serous envelopes. Whether the cases recovered, or whether they terminated fatally, the force of the disease was directed towards, and seemed to concentrate in, the fibro-serous membranes. Nor did any obvious change take place in the structures attacked until we crossed the line, in longitude 83° East, on the 18th of September, 1843.

There is no difficulty in assigning the reasons for the sufferings of the coolies from the intensity of the cold. They needed warm clothing. The difficulty consists in assigning reasons for the fibro-serous membranes of all other structures in the body having been selected as the seat of acute inflammatory action.

The question remains to be solved, in like manner, why the mucous membranes should be selected for attack, and be subject to specific morbid changes, when the surrounding atmosphere is surcharged with noxious pestilential vapours. It must be confessed that in this respect our knowledge is limited.

In India, medical men are aware that the prevalence of a particular form of disease in a locality can in general be traced to the nature of the surrounding soil. Medical men are aware that in jungly uncultivated districts, in localities where black and loamy soil, or a virgin soil topped with successive layers of decomposed vegetable matter, abound, health may be regarded as the exception, disease the rule. Through this description of soil, black, loamy, and impervious to water, rain does not rapidly filtrate. So long as the moisture of the ground is kept up to the point of thorough saturation, exhalations injurious to health do not appear to be generated.

But, when a heavy fall of rain has been followed by a long-continued drought, the "avant-courier," the forerunner of an epidemic scourge, impure, noxious, and devitalising exhalations are emitted by day and by night, in consequence of the powerful heat of the sun acting on a soil in the body of which so much moisture has been retained. In selecting sites for cantonments, or ground for encampments, military surgeons are aware that such localities ought to be avoided.

Again, in India, the sources from whence the cholera-producing changes proceed are numerous. Numbers of the medical profession, who have interested themselves in ascertaining the actual condition of the poverty-stricken in this country, by strolling through Indian villages, and viewing, for their immediate information, the heaps of nuisances which meet the eye at every corner, can be at no loss to account for the prevalence of disease, and for the mortality which follows in the wake of disease in every village in India.

So far as heaps of manure; so far as cesspools, half filled with stagnant water, and half filled with rotten vegetable garbage; so far as huts, closely crowded together, ill-ventilated, built on swampy undrained soil; so far as the wretched condition of the mass of the people, removed but one degree from actual starvation; so far as the carrion, half devoured by the village scavengers, swine, and pariah dogs, kites, and vultures, can contribute to the production and propagation of disease, in Indian towns and villages, through the medium of a foul, infected, pestiferous, and poisoned atmosphere:—such they fail not to do.

To propose a remedy for these evils would be equivalent to effecting a thorough reformation in the sanitary condition of India. The cleansing of the Augean stables, a work of Herculean labour, would dwindle into insignificance, compared with the cleansing of Indian villages in a single district.

Of all this we possess the strongest proofs: yet we are forced to return to the point from whence we started, without being able to assign a convincing reason for acute inflammatory action being devitalising, in memt

influence of an altered state of the atmosphere.

The admixture of noxious, devitalising vapours with the blood, through the pulmonary tissue by inhalation, and through the cutaneous surface by absorption, whether generated in the earth, or emitted from any other source, approximates so closely to a rational explanation for the outbreak of an epidemic scourge in a particular locality on shore, or in the hold of a vessel afloat, as need be assigned. For practical purposes more is not needed. The subsequent attraction or determination of the vital fluid thus impregnated to the capillaries and surface of the mucous membranes in one instance,—to the capillaries and surface of the fibrous membranes in another instance, may be left to speculative theory and theorists, in so far as mankind will derive any benefit from the discovery.

A CASE OF TWO CALCULI IN THE BLADDER:

ONE REMOVED BY THE OPERATION OF
LITHOTRITY, WHILE THE OTHER RE-
MAINED IMBEDDED.

BY JOHN GRANTHAM, F.R.C.S.E.

I was sent for on the 7th of April, 1847, to visit Mr. A., a farmer, aged 57 years, whom I found suffering very severely from all the symptoms of calculus in the bladder. The urine gave the following analysis: specific gravity (average) between the *urina sanguinis* and the *urina cibi* 1.007, colour pale straw, semi-transparent, acidity normal, urates in excess, with a few triple phosphates, albumen, blood corpuscles, and epithelial scales, denoting what my late esteemed and valued friend Dr. Prout would call "serous urine."

April 8th.—I introduced a sound into the bladder, and struck a calculus. On carefully exploring the stone, I came to the conclusion that it was of such a size as to justify its removal by the operation of lithotripsy, which operation I performed on the 14th of April, 1847. On securing the stone within the blades of the instrument, I ascertained the fact that the calculus measured $\frac{3}{4}$ ths of an inch in one diameter. On the seventh day from the first operation I again examined the

bladder, and broke up the remaining portions of stone: the whole of the fragments passed per urethram. I make this latter statement with great confidence, as this fact will be proved in a subsequent part of this paper. For the space of nine months the patient enjoyed the benefit of the operation with total absence of all the symptoms of calculus in the bladder. After the above period, owing to undue exposure to damp, &c. he received a chill, which was followed by an immense discharge of mucus from the bladder. On examination I found the prostate very large,—at least such was my evidence both by the urethra and the rectum. This attack was succeeded by others, with an interval of two or three months, each becoming more and more severe. During the last five months I considered the bladder quite empty, but thickened in its structure; yet this idea was at variance with the symptoms, from the patient's inability to retain any quantity of water in the bladder: and this circumstance reminds me of the difficulty, during the operation of getting the bladder to retain even one syringe-full of water, and that amount passed out in considerable quantity by the side of the instrument whilst endeavouring to grasp the stone.

June 17th, 1850, Mr. A. died; and on the following day I made a post-mortem examination, when the subjoined facts presented themselves:—Both kidneys were in a state of eccentric hypertrophy, with a granular soft appearance of the cortical portion, which was also studded with tubercular deposits; the pelvis and lining membrane of both kidneys being in a state of extensive ulceration, one ulcer in the *left* kidney nearly through its cortical substance. The bladder presented a corrugated appearance externally; the coats were thickened, but very soft; and, on taking off the fundus, I found a large calculus weighing five ounces and a half, imbedded in the folds of the lining membrane, extending to the under portion of the prostate gland, and not a single fragment could be found besides in the bladder. This immense stone, be it observed, was so firmly imbedded as to require much force for its removal: the upper surface was denuded of mucous membrane, which, I infer, could only have become so during the last five months.

Inferences drawn from the above statement:—

1. The great length of time required to produce so much disease.

2. That the disease of the kidneys preceded that of the calculous formation, as without doubt *both* calculi were formed in the pelvis of the kidney, both ureters being very large in diameter.

3. The complete removal of the first stone by the operation of lithotripsy, and the further establishment of the proposition that a fragment does not form the nucleus of a second stone.

4. The impossibility of saving the life of the patient, by the performance of the operation of either lithotripsy or lithotomy.

In conclusion, this case forms an example of the effects of over-looking, or not having had the opportunity of observing, the early symptoms; and I would remark that those symptoms or causes could not have been discovered by the practitioner unless he had taken

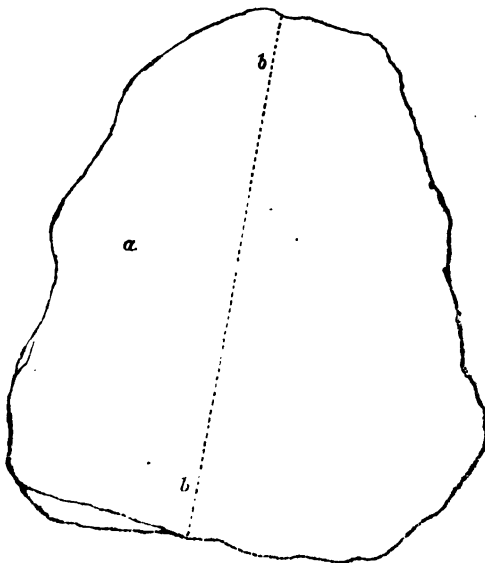
the trouble, and truly possessed the knowledge, of examining, both chemically and microscopically, the secretion of urine at the outset, when the disease might have been arrested. Censure, however, ill becomes any one of us, knowing how liable even the most experienced are to err in their judgment.

Chemical Examination of the Calculus.

By ALFRED S. TAYLOR, M.D., F.R.S.
Lecturer on Chemistry in Guy's Hospital.

The calculus was of an oblong, ovoid form, rather flattened, and wider at one end than the other. The exterior was of a whitish-brown colour, crumbling and soft, with a few irregular shallow depressions, but there was no sharp angle in any part. It measured about three inches in length, two and a half inches in the wider and one inch and a half in the narrower portion.

Outline of form and size.

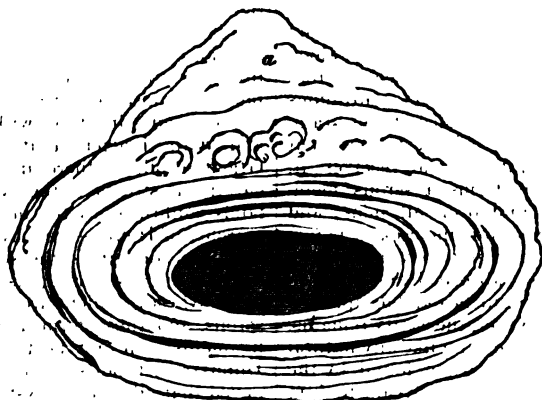


b b, line of section.

Externally it was dry, but internally soft when cut. Its weight was 1920 grains, (about four ounces.)

When sawn through, it presented an irregular oval section, the irregularity being caused by the super-position of a

whitish deposit, of a somewhat conical form, on the flat surface of the oval. The greater diameter of the oval measured $2\frac{3}{4}$ inches, the shorter diameter $1\frac{1}{4}$ inch, and the adventitious deposit (a) half an inch.



The nucleus was of a dark-brown colour, and situated about the centre of the oval. It presented a flattened oval in section, about one inch in length, and half an inch in width. The nucleus presented a somewhat cellular and not a regularly lamellated structure, except at the circumference. With the exception of the nucleus, the whole section of the calculus presented a chalky-white appearance. There was a very regular deposit of laminae immediately around the nucleus, but these became irregular

towards the circumference, especially at the upper part.

By the usual chemical processes it was ascertained that—

1. The *nucleus* consisted of lithic acid, with lithate of ammonia.

2. The *body*, of phosphate of lime alternating with layers of lithate of ammonia.

3. The *crust*, or external portion, of phosphate of lime, with phosphate of ammonia and magnesia.

Chemical Laboratory, Guy's Hospital,
June 25th, 1850.

ON THE ORIGIN, CAUSE, AND NATURE OF CHRONIC OVARIAN TUMORS.

By EDWARD JOHN TILT, M.D.

Physician to the Farringdon General Dispensary,
and to the Paddington Free Dispensary for the
Diseases of Women and Children.

WE have fully detailed in another periodical* all the varieties of the morbid cystic structure to which the ovary is liable, before venturing on any inquiry respecting their origin, or before we attempted to grasp (as far as it is permitted) the chain of their causation and the knowledge of their nature. If we begin by directing our attention to those closed cavities, natural or accidental, which are so numerously developed in the organization, we see that they have been wonderfully ordained to isolate the organs they surround, and to permit their independent action. Has not an important object been attained, by clothing these natural cavities with a serous membrane, lubricated by fluids? The accidental cavities developed in the frame are not less important. Our cor-

poral structure is liable to be invaded by so many and various bodies, that it was thus necessary to provide for the emergency, in order that whatever might be wrongfully introduced into our substance, should, if possible, be assimilated, or, failing this, at least be isolated, until it could be ejected.

That cellular tissue, which resembles serous membranes in its functions as well as in its structure, which separates the organs one from another, and binds them together in one harmonious whole, has, in each of its multitudinous cells, the power of secreting and absorbing the serum by which they are filled.* If, therefore, a ball or any foreign substance be forced into the body, the cellular tissue of the part where it is deposited forms around it an adventitious serous cavity, secreting a serous fluid; and thus the foreign body may remain

* Although the liquids effused in the areolar tissue are, like the blood in our veins, under the influence of gravitation, we cannot admit that these fluids are effused in the areolae of this tissue by mere physical transudation. This explanation can no more account for the presence of serum in the areolar tissue, than it would for the large cysts accidentally developed in the same tissue.

* *Lancet*, 1850.

for years isolated, and therefore harmless. In a similar manner, if, from any given cause (and there are many), blood be forced from those channels wherein it was destined to run its continuous course into the substance of the brain, lungs, or any other of our tissues, that ubiquitous cellular tissue receives it, and from itself forms a serous membrane to surround it on all sides; and as the substance thus inclosed is homologous to the inclosing body, it is sometimes completely absorbed, and the accidental cavity diminishes and disappears. Sometimes, however, the cavity remains, secreting and absorbing like any natural serous cavity; and when found by the anatomist it no longer reveals to him the origin and cause of its existence—the blood clot. In other cases this blood clot is only partially absorbed, and thus reveals the origin of the cyst, by its colour and appearance. We may therefore infer that wherever cellular tissue exists, there an hematic cyst may be developed. From the uncontested properties of the cellular tissue, we may also derive the origin of many other accidental cysts, so frequently met with; for every individual areola of this tissue secretes and absorbs; and, as it holds communion with the neighbouring cells, we find them all alike distended in oedema and anasarca. But certain areolæ are sometimes cut off by inflammation, or some other cause; and thus arises a serous cavity, the increase of which will depend on the nature of the organ wherein it is formed, as well as on a variety of other causes: cysts of the eyelids and of the mammae, the synovial bursæ of the patella, the ganglia, or tumors formed on the sheaths of tendons, may be thus satisfactorily explained.

We have thus deduced two origins for accidental cysts, as depending—1. On the nature of the areolar tissue, which permits one or more of its cells to set up on its own account; 2. On an hematic origin. We may also add, that whereas there is a great uniformity in the liquid produce of natural serous cavities, the serum being transparent, light-coloured, and of a low specific gravity, there is also uniformity in the nature of the fluid contents of all accidental serous cavities, the liquid being more complex in its nature, of a more considerable density, and often mixed with fatty substances and cholesterine. Applying to the subject under consideration these deduc-

tions from the laws of general pathology, we shall have at least the origins of ovarian cysts; for cellular tissue enters into the structure of the ovary, and blood may also be extravasated into its tissue: and admitting, with De Graaf, and with Langer, that, besides the Graafian follicles there are minute vesicles in the stroma of the ovary,—a fact lately confirmed by the microscopic researches of Heale,—we may believe that they are sometimes the seat of intra-ovarian fibro-serous cysts; while the extra-ovarian, which are always fibro-serous, and those formed in the broad ligaments, can only be thus explained. Dupuytren, Chaussier, and others, believed that all ovarian cysts were adventitious; but a slight inquiry into the nature of the ovaries will soon convince us that these are not the only original seats of ovarian cysts, and that we may believe, with Cruveilhier, Pr. Stoltz, and many other writers, that they often originate in the Graafian follicles. The ovaries are cellular in their structure; and it was necessary that they should be so, to render them fit receptacles for the human germ. Every month, during the productive period of a woman's life, is marked by the enlargement of a follicle, which may or may not burst. If it burst, the remaining cavity will be filled with blood, which may, or may not, have been absorbed when a succeeding menstrual effort comes to give increased energy to any healthy or morbid action in which the ovary may be engaged. We have, therefore, in the cystic structure of the ovary, and in the monthly fertility of its soil, additional reasons for the frequency of ovarian cysts; and the possibility of uncontrolled distension, which, from their situation, they necessarily enjoy, is a reason for their increased hydatids.

Very little is known of the origin and causes of hydatid formation in general, and, in particular, of hydatid ovarian cysts. These bodies are formed in fibro-serous cysts; and although the cause of their production is unknown, they may very likely depend upon some unhealthy condition of the blood; but still we know not in what that condition may consist. Thus it has been observed that, in almost every instance of hydatids, the general health has been, for a long time previous, considerably deranged: and it appears that Dr. Jenner produced hydatids and fluke-worms in rabbits, by

feeding them exclusively on succulent food. These experiments acquire additional importance since the publication of Dr. Schleisner's account of the diseases peculiar to Iceland: its corn, esculent roots, and fruit, will not grow in that island; the inhabitants eat little bread, or vegetables, and live upon dried fish, dried meat, butter, tallow, train oil, and blubber. The probable result of this exclusively animal diet is a frequency of hydatid growths unknown in any other country. Hydatids formed one-eighth of the cases enumerated, and one-sixth part of those observed by Dr. Schleisner, who remarks that in one parish he met with two or three sufferers from this disease in every family.—(Review of Dr. Schleisner's book, British and For. Med. Ch. Review, January, 1860.)

Unassisted by an imposing array of facts to account for the origin of these productions, we must have recourse to analogy, and to what we have learned from comparative physiology respecting the generation of our tissues. In explanation of these laws, can we do better than borrow from an illustrious countryman, who has already raised himself a name which must descend to posterity, Professor Owen? who, in his valuable lectures on generation, lately delivered at the Royal College of Surgeons, says,—“The primitive forms of all tissues are free cells, which grow by imbibition, and which develop their like from their nucleus of hyaline. All the animal tissues result from transformations of these cells. It is to such cells that the acephalocyst bears the closest analogies in physical, chemical, and vital properties. When the Infusorial Monads are compared to such cells, and man's frame is said, by a figure of speech to be made up of such monads, the analogy is overstrained, because no mere organic cell has its mouth, its stomach, its pulsatile sac, &c. So also it appears to me that the analogy has been equally overstrained, which makes the acephalocyst a kind of monad, or analogous species of animal. We may, with some truth, say that the human body is primarily composed or built up of acephalocysts; microscopical, indeed, and which, under natural and healthy conditions, are metamorphosed into cartilage, bone, nerve, muscular fibre, &c. When, instead of such change, the organic cells

grow to dimensions which make them recognizable to the naked eye, such development of acephalocysts, as they are then called, is commonly connected in the human subject with an enfeeblement of the controlling plastic force, which, at some of the weaker points of the frame, seems unable to direct the metamorphosis of the primitive cells along the right road to the tissues they were destined to form, but permits them to retain, as it were, their embryo condition, and to grow by the imbibition of the surrounding fluid, and thus become the means of injuriously affecting or destroying the tissues which they should have supported and repaired. I regard the different *Acephalocysts*, therefore, as merely so many forms or species of morbid or dropsical cells.

“The question which remains to be solved is, how the parent hydatid originates; two modes may be conjectured: either it is the result of the development of an organic granule, nucleus, or cell, of the animal infested, according to the hypothesis of fortuitous or spontaneous generation; or it is developed from an impregnated germ-cell detached from a pre-existing acephalocyst, and potentially endowed with the special mode and direction of the plastic force which issues in the repetition of the same organic form as the animal from which it proceeded. Analogy points to the latter as most accordant with actual physiological experience.”

Whatever may be the cause of these singular bodies, when once originated they may for years continue to reproduce similar bodies. The late Dr. Ryan has recorded a case in which hydatids were retained in utero so long as 14 years.

Piliferous cysts.—In some rare instances the impregnated germ falls not into the fallopian infundibulum, but remaining attached to the ovarian birth cell, gradually appropriates to itself the tissue of the ovary, and so distends it as often to attain a considerable size. These fetal cysts may remain stationary for years. Sometimes the fœtus is reduced to a kind of mummy, but at others it undergoes decomposition, the soft parts being converted into various kinds of fatty matter. While portions of skin of the fœtus become attached to the interior of the cyst, reproducing by its bulbs the hair which is so often found in considerable quan-

titles, the more solid parts, such as the bones and teeth, remain to indicate the origin of the cyst.

We are thus led to infer, that piliferous cysts are sometimes only fetal cysts, wherein the greater part of the body has undergone decay. But does this explanation hold good in all cases of piliferous cysts? M. Pigné, who has paid great attention to this subject, does not hesitate to assert that all piliferous cysts are either the result of extra-uterine pregnancy, or of a malformation; that, for instance, when piliferous cysts are found in girls of a tender age, or in young boys, we must believe them to be relics of some other individual contemporary with the one in whose body they are implanted; and that it was in an early stage of their existence that somehow or other one of the two became included in the other. Those who must have an explanation of the fact, and who can never, as Montaigne so quaintly says, "make up their minds to lay quietly their heads on the pillow of doubt," may admit this explanation, but it is surely better to confess our ignorance than to attempt to explain the inexplicable. If we had merely to account for those piliferous cysts growing in the vicinity of the shin, in the scalp, the eyelid, the mamma, and the scrotum, we might, as Carswell suggests, consider the hair they contain as arising from accidental development, as it grows from the anterior portion of these cysts which is lined by the reflected cutaneous tissue; but such an explanation could not account for the hair and teeth found in the ovaries of virgins, in the coats of the stomach and of the bladder of man, in bronchocele as mentioned by Celsus, in the abdomen of a boy by Dupuytren and Mr. G. W. Young, in the lungs by Möhr, in the brain by Morgagni, and in the orbit by Barnes. (Med. Chir. Trans. vol. iv.)

Without pretending to explain the fact, we must observe that in all well-authenticated cases the interior of the cyst has been found to be lined with a substance like the skin, with bulbs producing hair. Cruveilhier, Velpeau, Carswell, and many others, have seen cysts partially lined with skin; in a case recorded by Bricheateau, the cyst was entirely so lined, and this must also have been the case in the instance quoted by Morgagni, from the author of an opusculé, (*de Ovarii tumore pilosâ,*)

Liepsick, 1786), who does not scruple to compare the cavity of the cyst to the scalp. It must be a tissue similar to skin, for it not only secretes fat and sebaceous matter, but hair has been repeatedly seen to grow from its bulbs, horny productions have been known to spring from it, dental sacs and teeth have been found rooted in it: and these are all epidermoid productions. How a fragment of skin could thus be found we cannot explain, but it seems as if the vegetative life adherent to each of the germ-cells which constitute our frame, were able sometimes to produce the "horn layer," (mucous layer), or that portion of the germinal layer which covers the embryo, and is the foundation of the non-vascular and nervous tegumentary appendages. Here, again, we willingly quote from Professor Owen's second lecture on the generation and development of invertebrated animals, in order to bring the light of comparative physiology to bear on the more abstruse points of pathology.

In many classes of the invertebrate animals the retained spermatic force operates with such energy as to form a new individual from a secondary, tertiary, or quaternary derivative germ mass. How this is done we know not; suffice it to say that it is done. The completion of an embryonic or lowest form by the development of an ovarian germ-cell, as in the aphidæ, without the immediate reception of fresh spermatic force, has never been known to occur in any vertebrate animal; in the vertebrate animal, the whole of the spermatic force, originally diffused amongst the cells or nuclei of the germ mass, is exhausted in the development of the tissues and organs of the individual, in the mysterious renovation of the spermatic power in the male, by a special organ; and in the development of ova or cells prepared for its reception in the female. But it now and then happens, even in the highest of the vertebrata—the human species—that the ovarian cell sets up the process of embryonic development, but without sufficient of the spermatic and plerotic power to complete even a lowest form: some crude materials of the embryo are the only result,—teeth, it may be, or hair, with irregular amorphous ossifications, such as are met with occasionally in ovarian cysts.

Whatever may be the origin of such epidermoid appendages, they are more frequently found in the ovary than in any other organ, and Meckel asserts that they are more frequently found on the right side. They are met with at all ages: in 49 cases collected by M. Pigne, 5 were not 12 years old, the age of 6 varied from 6 months to 2 years, 4 were full-grown fetuses, and piliferous cysts were also found in the ovaries of 2 fetuses which had been cast off before the end of the ninth month.

These cysts may communicate with the bladder, as in the case given by Suetin, (An. de Med. Belge, 1838), where a tooth was found forming the nucleus of a stone in the bladder. Hair has likewise been passed in the urine by a communication between the bladder and a piliferous cyst, but Magendie has published cases of gravel, wherein hairs were passed mingled with the usual saline conditions. Where did these hairs come from? Were they secreted by the kidneys?

It is well to know that such productions are not peculiar to the human species, and that the hair of the cow and the wool of the sheep have been found in the ovaries of these animals.

Penada found a fatty bag containing two bunches of feathers, 33 in number, in the breast of a young cock.

6, York Street, Portman Square.

FATAL CONGESTION OF THE BRAIN,

OCCURRING IN AN INFANT WHILE TAKING
COD-LIVER OIL.

By W. B. KESTIVEN, Esq. M.R.C.S.

THE death now recorded is not related as being the undoubted effects of cod-liver oil: it is put on record as worthy to be known, whilst the properties of this oil are undergoing an extensive examination by the profession. It is meant as a contribution to that examination, and may serve to teach caution in the use of cod-liver oil. At the same time the writer distinctly avoids the application of the *post ergo propter* line of argument in this case.

H. O—, aged one year and nine months, was attacked on the 17th June with symptoms of congestion of the brain, which were attributed by me to dentition. The means employed were

attended with success, and the child was quite well on the 24th of June. On the 27th the child was again seized with more grave symptoms of cerebral congestion: it was convulsed, its head was hot, its bowels disturbed, and it had frequent vomiting of bile. Treatment, this time, was of no avail. The child died in the course of two days. A post-mortem examination was refused.

The history of the case here follows. This child had been, according to its parents' account, a "delicate child" from its birth. About fourteen months since I was consulted with reference to a curvature of the spine which it presented in the dorsal region;—the result of bad nursing. Strict adherence to the horizontal posture, nourishing diet, and plenty of fresh air, were advised. The child's health improved, and the back had very much improved. Some kind friend, who could not let well alone, urged the parents to give cod-liver oil. This was done, and its use in full doses continued for several months previously to the first attack of cerebral congestion. The parents had asked my opinion of the oil (after having given it for some time). I did not advise its discontinuance, partly for the sake of not seeming to be captious, and partly because I looked upon it only as so much food, of which I hoped it might not get too much. Its use was discontinued while under my care for the head symptoms; but, directly health was restored, so also was the cod-liver oil (by the parents). The rest of the case has been told above.

It may be repeated, that the child's death is not positively attributed to the cod-liver oil; but the writer thinks it highly probable, that if it had not been administered—at least, to the extent, and at the time that it was—the child might not have died of congestion of the brain.

Holloway, July 1860.

SLOUGHING ULCERS.

No case of fatal hospital gangrene has occurred in the Pennsylvania Hospital wards during the last four years or more. The disease, as an epidemic or endemic, is unknown there. Where a sloughing tendency was perceived, the applications which appeared to the writer of most service, were the carrot poultice, and the wash of nitric acid, used daily, *gtt. 1.* in Oj.—Dr. Hartshorne, in *American Journal of Med. Sciences*, 1850.

MEDICAL GAZETTE.

FRIDAY, AUGUST 9, 1850.

In reference to the remarks made in a recent number* on the enforcement of privileges by English and Scotch Corporations, it has been suggested to us by an Edinburgh physician, on whose opinion we are inclined to place great reliance, that the charge of illiberality cannot be fairly made against the Scotch Colleges. The writer informs us that English members and licentiates are not persecuted in Scotland. The Edinburgh College of Surgeons, with ample privileges, has not prosecuted any one for practising within its bounds for half a century; and on that occasion the prosecution was directed against a person who held no license whatever—a mere quack: therefore, so far as the liberty of practising in Scotland is concerned, the charge is unfounded.

The Colleges of Surgeons of Edinburgh and London pursue, however, a widely different course with respect to the members and licentiates of each. The English College of Surgeons has the following among its regulations:—

“II.—Members or Licentiates of any legally constituted *College of Surgeons* in the United Kingdom, and Graduates in Surgery of any University requiring residence to obtain degrees, *will be admitted for examination* on producing their *diploma*, license, or degree, together with proof of being twenty-one years of age, and of having been occupied at least four years in the acquirement of professional knowledge.”

There is here no inquiry into the minutiae of curricula or courses of education,—a subject upon which no two licensing corporations ever have agreed,

never will agree,—but a free and liberal recognition of the right of the Scotch member to examination. The practice of the Scotch College is set forth in the letter which led to our remarks.* The writer says—

“I applied to the College of Surgeons of Edinburgh to admit me to examination on producing my diploma of the College of Surgeons of England. I was refused to be admitted to examination unless I possessed certificates of lectures on all the classes required by the Edinburgh College. Surely this is hardly an act of courtesy, when the London College admits licentiates of the Scotch College to examination solely on production of the Scotch license.”

We believe this statement to be strictly correct; and so long as this system is adopted by the Edinburgh College, it must create great dissatisfaction among members of the English College. A comparison of curricula is assuredly not needed, when the course of study and examination enforced by the London Colleges qualifies a man to practise as a surgeon throughout England and Wales, and makes him eligible to hold any surgical appointment in the kingdom. By rejecting the English diploma as a sufficient testimonial for admission to examination, the Scotch College passes a severe censure upon the English College of Surgeons, and upon all who are connected with it. One great step to an amalgamation of practice in the United Kingdom, would consist in the different corporations recognising their respective curricula in medicine and surgery as sufficient at least for the purposes of *examination*: and on this point we regret that the English College should have adhered so strictly to the *letter* of its regulations as to refuse examination to a licentiate of the Glasgow Faculty of Physicians and Sur-

* See our number for July 12th, p. 67.

* See MEDICAL GAZETTE, July 12, p. 67.

geons.* We presume, however, as the Glasgow license is for physic as well as surgery, there was some legal difficulty in the way of making the concession in this instance.

The curriculum of the English College has produced, and still produces, many excellent surgeons: and there is nothing to justify that distrust of the extent of study, which is shown by the Scotch College in refusing to admit to their examination those who possess its diploma.

THE necessity for some legislative measure to prevent the frequent destruction of life which occurs by explosions in coal-mines, has been long apparent. A catastrophe of this kind, in which from fifty to one hundred persons lose their lives, occasionally leads to inquiry, but unfortunately no practical measure to arrest these calamities is based on the successive inquiries. An accident has only recently occurred at Glasgow, in which nineteen persons out of twenty lost their lives by the explosion of fire-damp in a coal-mine. In general all are destroyed, and the cause of the accident becomes a matter of pure speculation. In this instance, one person escaped, and we learn from his statement that "the men had not Davy lamps." We believe that these calamitous explosions are generally due to the want of the lamps or to the improper use of them; and if the Government desire to arrest the wholesale destruction of life, the simple remedy will be to appoint inspectors of Mines, who shall be empowered to see that the men are provided with properly constructed and well-secured lamps. A short Act of Parliament would, we believe, suffice for securing the miners, while it would not injure the rights of proprietors.

* See our number for July 26th, page 170.

THERE appears to be a considerable difference of opinion regarding the nature of the questions which can be legally put to medical witnesses in civil and criminal cases affecting the insane. On the occasion of the trial and acquittal of M'Naughten for shooting Mr. Drummond in the year 1843, a series of questions was submitted to the judges by the House of Lords; and one of these (Question V.) bears directly upon the limitations to be put on medical opinions regarding the insanity of individuals. The question was as follows:

Question V.—Can a medical man, conversant with the disease of insanity, who never saw the prisoner previously to the trial, but who was present during the whole trial and the examination of all the witnesses, be asked his *opinion as to the state of the prisoner's mind*, at the time of the commission of the alleged crime; or his opinion whether the prisoner was conscious at the time of doing the act, that he was acting contrary to law, or whether he was labouring under any, and what delusion at the time?

Answer. The question could not be put in the precise form stated above, for by doing so it would be assumed that the facts had been proved. When the facts (indicative of insanity?) were proved and admitted, then the question, as one of science, would be generally put to a witness under the circumstances stated in the interrogatory. [Mr. Justice Maule dissented from his learned brethren. In his opinion such question might be at once put to medical men without reference to the facts proved.]

The right of putting such a question to a medical witness was, however, recently denied by Mr. Baron Alderson, in a trial which took place at the Old Bailey; but, instead of preventing the question from being put, the judge, on this occasion, rebuked the witness for returning an

answer! The question again arose in an important will-cause—*BAINBRIDGE versus BAINBRIDGE*—lately tried at the Stafford Assizes, before Lord Campbell, in which it was alleged that the testator was insane, and that the will which had given rise to the suit was therefore invalid.

A large mass of evidence was adduced, to show that the testator was not conscious when the will was executed; and that for some time previously, his habits and conduct were such as could only be explained on the supposition that he was insane. We are then informed that

towards the conclusion of the plaintiff's case, Dr. Monro, Dr. Conolly, and Dr. Forbes Winslow, were put into the witness-box to give evidence on the question of insanity, and were allowed to give general scientific evidence on the causes and symptoms of insanity, but *not to express an opinion as to the result of the evidence they had heard with reference to the sanity or insanity of the testator*, his Lordship saying peremptorily that he would not allow a physician to be substituted for the jury. The Solicitor-General hereupon proposed, in form, the following question to the first witness, Dr. Monro, for the purpose, if it should be necessary, of having its propriety determined in the court above: *'Having heard the evidence in this case, are you of opinion that this gentleman was or was not of sound mind?'*

Mr. KEATING objected to the question.

LORD CAMPBELL.—I have not the smallest hesitation in overruling it.

His Lordship, at Mr. Cockburn's request, took a note of the point.

Dr. Monro was subsequently proceeding to state, in answer to a general hypothetical question from the defendant's counsel, an opinion upon the facts proved in the cause, when

LORD CAMPBELL interposed, and *peremptorily requested him not to express any opinion on those facts, but to confine himself to general scientific principles.*

We are at a loss to reconcile the proceeding of Lord Campbell with the answer of the fifteen judges. The "facts," on which the plaintiff relied to make out evidence of insanity, were "proved

and admitted:" and we do not see the any injustice would have been caused to the case of the defendant by allowing the three medical witnesses to express an opinion on these facts. The jury might or might not have been guided by that opinion, which, it may be remarked, could only have had force in proportion as it was in accordance with the evidence, and with common sense.

The peremptory direction of the judge, that they should confine themselves to *general scientific principles*, is tantamount to affirming that medical evidence is of no value in such cases. In a case of death alleged to have been caused by a blow on the head, it would be considered rather an eccentric ruling on the part of a judge, that a medical man should not be permitted to express an opinion of the cause of death, from the "facts" that the deceased had fallen dead after having sustained a severe blow, which had fractured the skull and ruptured the lateral sinus. The ruling in the Stafford case, if correct, would, however, lead to this:—a medical man so situated, would not be permitted to express an opinion on those "facts," or to say, in answer to a question, that death was caused by the blow proved to have been inflicted by the prisoner; for this would be substituting him for the jury. The witness would have to confine himself to "general scientific principles,"—i. e. to blows on the skull in general, and to the effects of a lacerated lateral sinus in the abstract.

If a physician affirm that a person is insane when the facts fail to bear out the opinion, the jury correct this error by refusing to act upon his judgment, just as in the assumed case of manslaughter they would not be guided by the opinion of the surgeon, unless it were borne out by the circumstances proved. We are at a loss, therefore, to know why this peremptory direction to

keep to anything medical, except that which had immediate reference to the case under trial, should have been laid down as the law by Lord Campbell, when it is in open violation of the answer returned by the judges on this very point.

We must admit, however, that the answer somewhat resembles the responses of the Delphic oracle: it may be read in two or three different ways. We here take it in its plain meaning: certain facts are proved and admitted. Do those facts lead to the conclusion that the person affected by them is insane? The fifteen judges say this is a question which may be fairly put to a medical witness. Mr. Baron Alderson and Lord Campbell hold that a question of this kind is for the jury only, and not for a medical witness: but, unless the jury be well versed in the subject of insanity, their verdict will, in many cases, be only a random conjecture.

NEW METHOD OF RELIEVING RETENTION OF URINE WITHOUT THE USE OF THE CATHETER. BY M. J. J. CAZENAVE.

WHEN called to a patient having retention, complete or incomplete, M. Cazenave in the first place directs the large intestines to be cleared out by an enema. When this has returned a second is administered, but consisting solely of a quart of cold water. Absolute rest on the bed is enjoined; while cloths dipped in cold water, or, better still, bladders of ice-cold water, or pounded ice, are applied to the anus, perineum, thighs, and hypogastrium. If the patient do not void his urine in the course of half an hour, or void it only very scantily, he is placed at the edge of the bed, which is properly guarded, and a stream of cold water is poured on the region of the bladder during from twenty to twenty-five minutes. After the lapse of this time another enema of cold water, and small smooth fragments of ice, are introduced into the rectum, the cold applications to the external parts being at the same time continued. The cases in which this mode of treatment is found applicable are those in which the retention proceeds from acute inflammation or spasm.

—*L'Union Médicale*.

X

Reviews.

Pathological and Surgical Observations on the Diseases of the Joints. By SIR BENJAMIN C. BRODIE, Bart., V.P.R.S., Serjeant Surgeon to the Queen, Surgeon to H.R.H. Prince Albert; formerly Surgeon to St. George's Hospital, &c. &c. Fifth edition, with alterations and additions. 8vo. pp. 399. London: Longman and Co. 1850.

THE publishers of the new edition of this well-known work have appended to its author's name the highest titles of honour to which the greatest surgical talents can lead their possessor in this country. The fame of Brodie, however, will always be more closely connected throughout the world with the work itself, than with these merely honorary dignities. The first edition of this work showed an author of close reasoning powers, endowed with a more than common share of those qualities which contribute to form the skilful surgeon. The fifth edition finds the opinion then formed to be correct, and its author elevated by the conjoint approbation of his profession and of the public, to the most exalted point of professional success; the enjoyment of the highest honours and emoluments that industry and ability are capable of commanding. Of Sir Benjamin Brodie it may be said with truth, even at the present time, "*Actis ævum implet, non segnibus annis;*" and we would add, long may he live to reap their just reward.

We cannot better introduce the fifth edition of this invaluable work to our readers than in the words of the author himself:—

"The fifth edition of my treatise on the diseases of the joints, which is now offered to the public, differs, in many respects, from those by which it has been preceded. I have found no reason to alter the general arrangement of these diseases which my early investigations had led me to adopt. But many new cases and observations have been introduced, illustrating the pathological changes which were described formerly; and some new chapters have been added, relating to other changes, which my earlier experience had led me to notice only in a brief and cursory manner. Those parts, which relate to the diagnosis and treatment

of diseases have been considerably extended, and I hope that the volume will thus be rendered more useful to the practical surgeon, whose principal object must always be to obtain the means of cure, and to whom scientific pathology will be valuable in proportion as it leads to this ulterior result.

"Although these researches have occupied more or less of my time during the greater part of my professional life, I am aware that they are still imperfect. When I first turned my attention to the subject, I found that I was engaged in a new and extensive field of inquiry, such as it was impossible for one individual, however diligent, and however great his opportunities, thoroughly to explore. Those who follow me will, I doubt not, find much both to add and to correct; but I trust that what I have been able to accomplish will assist them in their labours, and will, in the meanwhile, in some degree supply what was formerly a great deficiency in the literature of scientific surgery. In the earlier editions I published a series of cases illustrative of the history and progress of the various diseases of the joints, as they are exhibited in the living person, and the treatment which they require. I was led to do so, as the subject was at that time, in a great degree, new to my readers, as it had been to myself. Under present circumstances it has appeared to me that another course was to be preferred, and I have accordingly endeavoured to supply the information which it was thus intended to convey, in the form of a more complete analysis of the observations which I have made in the course of my practice, omitting the details of individual cases, with the exception of those relating to pathology."

The subjects treated in this volume are nearly the same as those contained in the preceding editions of the work. We shall enumerate its contents, in order that our readers may compare this edition with its predecessors. At the same time we must remark, that a close study alone of the work itself will show the extent of the additions.

"Inflammation of the synovial membranes of joints. Ulceration of the synovial membrane. Cases in which the synovial membrane has undergone a morbid alteration of structure. Scrofulous disease of the joints having its origin in the cancellous structure of the bones. Ulceration of the articular cartilages. Necrosis of the joints. Chronic disease of the joints connected with gout and rheumatic gout. Loose cartilages and excrescences in the cavities of the joints. Malignant diseases and other morbid growths connected with

the joints. Neuralgia of the joints. Chronic abscess in the articular extremity of the tibia. Caries of the spine. Some diseases of the joints not included under the foregoing heads. Inflammation of the synovial bursa."

The following observations, which occur in the first chapter, may well be borne in mind at a time when a manifest tendency exists to the too frequent reliance on topical remedies for what is often erroneously regarded as purely local disease. We have marked one sentence by italics, to direct especial attention thereto.

"Inflammation of a synovial membrane may arise as a local affection, the consequence of a sprain, a contusion, or other mechanical injury. In other cases, various joints being affected, either simultaneously or in succession, it is manifestly the effect of a disordered state of the general system; and even in those instances in which the inflammation is confined to a single joint, a careful inquiry will generally satisfy the surgeon that it has had a similar origin. *Indeed, I must confess that, in proportion as I have acquired a more extended experience in my profession, I have found more and more reason to believe that local diseases, in the strict sense of the term, are extremely rare.* Local causes may operate so as to render one organ more liable to disease than another; but every thing tends to prove, that in the great majority of cases there is a morbid condition, either of the circulating fluid, or of the nervous system, antecedent to the manifestation of disease in any particular structure." (p. 25.)

We would direct attention to Sir B. Brodie's opinion with reference to the opening of abscesses in the joints—a complication, often, of some general morbid condition, as scarlatina, puerperal fever, &c., constituting a symptom of very grave import, and, from the concurrent effects of disease, one extremely difficult to cope with.

"As a general rule, I believe that it is advisable to open an abscess connected with a diseased joint, rather than to allow it to burst spontaneously; but the opening should be deferred until the skin is become thin over it, except in a few cases in which it is burrowing among the muscles of the limb under a thick fascia. There are few questions in surgery of greater practical importance than that which relates to the management of an abscess connected with a diseased joint. There are great objections to the opening the abscess by a small puncture. Even if the

orifice should not (as frequently happens) become obliterated by portions of solid lymph impacted in it, neither at the time of its being made, nor afterwards, does such a puncture afford a free passage for its contents. A certain quantity of pus accumulates in the cavity of the abscess, the overplus escaping through the artificial orifice as the overplus of water escapes through the waste-pipe of a cistern. Under these circumstances the purulent discharge is invariably profuse; for a lodgment of pus operates like a pea in an issue, by stimulating the secreting surface, and augmenting the secretion.

"The practice which has appeared to me to be, on the whole, the best, is the following:—An opening having been made with an abscess-lancet, the limb may be wrapped up in a flannel wrung out of hot water; and this may be continued until the first flow of matter has ceased, a poultice, or warm water dressing, being applied afterwards. In some instances, after a short time the discharge ceases, the orifice heals, and the puncture may then be repeated some time afterwards. But where the puncture has not become closed, I have never found any ill consequences to arise from its remaining open. On the contrary, I have no doubt that it is desirable that the wound should not be closed until the abscess has contracted, granulated, and healed from the bottom; and this is one reason for making, not a small puncture, but a free opening with an abscess-lancet. Another reason is, that the matter will escape readily without squeezing or pressure. *All rough manipulation is to be carefully avoided.* It produces hæmorrhage into the cavity of the abscess, the ill consequences of which I have already pointed out; and, independently of this, it may excite inflammation of the cyst, attended, where the surface is extensive, with so much constitutional disturbance as to endanger the life of the patient at the time, and materially lessen the chance of his recovery afterwards." (p. 133–137.)

The following remarks on a subject of much diversity of opinion deserve notice, as showing harmony between the results of practical experience and scientific research.

"If a section be made of the articular cartilages in a growing child, canals or sinuses may be distinctly perceived in them, containing red blood. These are not constructed with the distinct tunics of ordinary blood-vessels, nor is there any appearance of minute ramifications of vessels pervading the cartilage generally. Still it would be unreasonable to doubt that they are intended to convey blood into these

structures for the purposes of nutrition and growth.

"But after the period of growth is concluded, no blood-vessels can be detected in healthy cartilages, even on microscopical examination; and as modern researches in anatomy have shown, that not only nutrition, but absorption, and various changes of structure, may take place in the living body, independently of any distinct vascular apparatus, we are justified in the conclusion, that under the circumstances which have been just mentioned, no such apparatus exists."

After allusions to the observations of Messrs. Mayo and Liston, Sir Benjamin proceeds to remark:—

"Interesting as these observations may be, as to the vascularity or non-vascularity of cartilages, and the mode in which the changes which they undergo in health and disease are produced, and important as the researches in minute anatomy, conducted by means of the microscope, may ultimately prove to be, it must be acknowledged that the time has not yet arrived at which the results can be applied, with much advantage, to the advancement of practical surgery. In whatever way it may be accomplished, whether by the development and destruction of cells, or by means of capillary vessels, or partly by one and partly by the other, there is no doubt that cartilages are nourished and grow, and undergo changes of organization, and that they are absorbed and die, and exfoliate, very much in the same way as parts which are distinctly vascular. And after all that can be said on the subject, the difference in these respects between cartilages and the other animal structures is probably more apparent than real. There can be no more doubt that the former derive new materials in some manner from the vessels of the bone to which they are attached, and that their old materials pass into the general circulation through the same channel, than that the fetus somehow receives its nourishment from the maternal portion of the placenta. Nor should we lose sight of this consideration, that even in those organs which are endowed with the highest degree of vascularity, it cannot be supposed that every change that takes place is by the distinct agency of blood-vessels. The latter supply the new materials, but they cannot occupy every point of space, and there must be intervals between them in which it is probable that molecular changes are taking place, corresponding to those which occur in what are called extra-vascular organs." (p. 155.)

It is almost superfluous to remark

that these observations correspond with the results of the latest microscopical investigations into diseases of the cartilages.

One subject with which Sir Benjamin Brodie's continuous labour, and later observations, have enriched surgical science, is the pathology of certain obscure cases of disease about the knee-joint, in which, doubtless, amputation may have needlessly been had recourse to, but which Sir B. Brodie has shown to be curable by the use of the trephine alone: this disease is chronic abscess in the head of the tibia. We shall quote a portion of the author's remarks on this disease, which requires great nicety and judgment in its detection and treatment.

"Occasionally, chronic inflammation of the articular extremity of the tibia terminates in the formation of an abscess in the centre of the bone, but contiguous to the joint. An abscess of this kind is attended with an extraordinary degree of suffering, such as not only would justify amputation, if there were no other means of obtaining relief, but would induce the patient cheerfully to submit to the operation. Fortunately, a less formidable mode of cure is within our reach." (p. 288.)

"Chronic abscess of the extremity of the tibia may exist during a very long period before it interferes with the neighbouring joint. In one case the patient had laboured under the symptoms of the disease for as many as eighteen years before I was consulted. The symptoms by which the disease is indicated in the first instance, is pain in the affected part, which is more or less of an intermitting character. The pain gradually becomes more severe, but still it is intermitting. For some time the patient may suffer so little from it that he is not prevented from attending to his usual occupations; then, without any manifest reason, a paroxysm occurs, in which the pain is intense: he is utterly disabled, and even unable to quit his bed. This gradually subsides, and he has another interval of ease. As the disease goes on, the bone becomes increased in size, the general health becomes affected, and the mind probably is rendered miserable and irritable by long-continued suffering. In one case, whenever the patient began to use the limb, the knee itself became inflamed, and there was an effusion of fluid into the cavity of the synovial membrane." (p. 286.)

"The most important point in the operation is that of ascertaining the exact part at which the trephine should be applied. I have always found that there was one spot to which the pain was more especially re-

ferred, and which was more especially sensible to pressure; and, when this has been satisfactorily determined, I have concluded that this was the part at which the perforation should be made. The trephine which I have generally used is a little more than half an inch in diameter, and is without any projecting rim, so that there is nothing to prevent it penetrating to any depth that may be required. Sometimes, when I have taken out a portion of the bone by means of the trephine, I have found it expedient to make use of a common elevator to complete the operation, by breaking down the immediate boundary of the abscess; but I have never yet had occasion to make a second application of the trephine. However, such an occasion may occur. A very experienced hospital surgeon applied the trephine for a supposed abscess in the head of the tibia. No abscess, however, was discovered; and, in consequence, the limb was amputated. On the parts being examined afterwards, the abscess was discovered at a small distance from the perforation made in the operation; and it was plain that the removal of a small portion more of the bone would have preserved the patient's limb." (p. 298.)

The work we have here brought before our readers needs no commendation from the pen of a reviewer. Its position in surgical literature is fixed. We would merely impress upon all those whose library-shelves do not possess a copy of this treatise, that whether physicians or surgeons, whether "pure" or "general practitioners," their libraries are deficient in one of the latest and best models of what medical researches should be. The senior members of our profession will find herein practical reminiscences of their own experience; while the junior will find an example which they may wisely follow, whatever may be the subject of their studies and researches. May they pursue them, like Sir Benjamin Brodie—*fortiter, fideliter, felicitèr*.

The Sumbul: a new Asiatic remedy, of great power, against Nervous Disorders, Spasms of the Stomach, Cramp, Hysterical Affections, Paralysis of the Limbs, and Epilepsy: with an account of its physical, chemical, and medicinal characters, and specific property of checking Collapse-cholera, as first ascertained in Russia.
By A. B. GRANVILLE, M.D. F.R.S.,

of the Royal College of Physicians, London, &c. &c. Pamphlet, 12mo. pp. 40. London: Churchill, 1850.

"SUMBUL is the Asiatic name of a vegetable drug, hitherto nearly altogether unknown to the medical practitioners of Europe. It is only within the last two years that in some parts of the Continent it has acquired considerable celebrity, in consequence of its ascertained value in the worst stages of CHOLERA."

Dr. Granville thus introduces the subject of his pamphlet. He informs us also, that "previously to such application of the drug, the Sumbul had been employed in Russia, as a stimulant and rouser of animal energy in the advanced stages of malignant fevers, (Typhus); as well as in dysentery of an asthenic or debilitating character, not less than in chronic diarrhoea, with unquestionable success."

The information which the author has been able to obtain on the *habitat* and character of this plant is very imperfect. But there is every reason to believe that the plant belongs to the umbelliferous family, and that it is probably an aquatic plant. Dr. Granville mentions two very striking physical characters, its "perfume like that of the purest musk, and the powerful aroma it exhales in the mouth when masticated," which is very peculiar, somewhat resembling that of *Angelica* root.

Chemical analysis gives as its constituents; water, an ethereal oil, two balsamic resins, wax, aromatic spirit, and a bitter substance.

The classes of diseases for which it has been employed are indicated on the title-page, which we have copied above.

The author adds "A Few Words on the question of Cholera;" in the collapse of which Dr. Granville asserts that Sumbul has been found pre-eminently efficacious. We rejoice to know that at present we have no opportunity of trying this new remedy for the last-named disease. In other forms of disease its virtues will doubtless soon be ascertained, if they are not extravagantly lauded after the fashion of "certain cures" for all cases of "consumption, asthma, coughs, colds, shortness of breathing," &c. &c.

Proceedings of Societies.

ACADEMY OF MEDICINE, PARIS.

July 30, 1850.

New Speculum for the Mouth.

M. MATHIEU submitted an instrument which has the advantage of being placed in the mouth without the necessity of its being previously opened wide for its admission; and which does not obscure any portion of its cavity, nor impede operative proceedings.

Gutta-percha Instruments.

M. ROBERT read a report on a memoir presented by M. Gabrol, which spoke in favourable terms of the employment of gutta-percha for the manufacture of many surgical instruments. M. Ricord agreed with the report, with the exception that he did not concur in the opinion that the use of gutta-percha sounds disposed, less than others, to the formation of calculous deposits. These, M. Ricord observed, are dependent upon individual peculiarities, and not upon the introduction of instruments. M. Ricord considered sounds and bougies of gutta-percha to be more durable, and superior to those of caoutchouc or wax.

M. Velpeau had found gutta-percha bougies to yield when warmed by contact with the walls of the urethra, and from their want of elasticity lose their shape. The same inconvenience, M. Velpeau stated, had attended the use of gutta-percha pessaries.

M. Segalas had found these sounds and bougies an improvement upon those formerly in use.

SURGICAL SOCIETY OF PARIS.

July 31, 1850.

M. HUGUIER stated that the microscopical examination of the cutaneous tumors submitted on a former occasion had confirmed the opinion of M. Giraldes, who considered them to be simple hypertrophy of the skin. The polypus of the rectum, which also was exhibited on a previous occasion, had been examined microscopically, and found to consist of thickened mucous membrane and fibrous coat of the intestine.

Fatal Vesical Hemorrhage.

M. VIDAL read a report on a work by M. Desormaux, in which a case was related of a man who, in otherwise good health, died

from hemorrhage into the bladder. That organ was found filled with coagulated blood, in the midst of which was a small calculus. No disease of the bladder, prostate, or urethra, could be found to account for the hemorrhage; but, as M. Vidal observes, the kidneys and ureters not having been examined, who knows whether the kidney might not have been its source?

ACADEMY OF SCIENCES, PARIS.

July 29, 1849.

Antidote for the Bites of Serpents, &c.

M. JOMARD submitted, in behalf of M. HERRAN, *Chargé d'affaires* in France, of the Republic of Costa Rica, a number of grains yielded by a tree which is called in that country *Cedron*, and grows on the Cordillera of the Andes. These grains are said to be antidotes to the bites of venomous serpents, &c. They are used in doses of five or six grains, given internally in brandy, and a small portion of their powder is at the same time applied to the wound. M. HERRAN, in desiring that the properties of these grains should be investigated, stated that he had also employed them with success in cases of ague that had resisted quinine.

Veterinary Surgery, &c.

M. BAYEN described a new method of removing the ovaries of calves by an incision through the superior part of the vagina, which had been found successful, and was not followed by peritonitis.

M. LUTERIE (of Rambervillere), informed the Academy that he had observed epidemics of influenza attacking horses, with, in every respect, the same symptoms as occur in man.

DURHAM COUNTY MEDICAL ASSOCIATION.

AN influential meeting of the members of the medical profession belonging to the city of Durham and the surrounding neighbourhood was held last week at Durham. W. Green, Esq., presided on the occasion: and it was unanimously determined that an Association should be formed, to be called "The Durham County Medical Association for the prosecution of unqualified Practitioners, and for the suppression of Quackery in all its forms, whether in the Profession or out of it." Resolutions respecting the qualifications for membership and other matters were passed; and after Mr. J. Patrick had been appointed the solicitor and secretary of the Association, the meeting was adjourned until Monday, the 5th of August.

Hospital and Infirmary Reports.

FELLOWS' PRIZE REPORTS
OF
CASES OCCURRING IN UNIVERSITY COLLEGE
HOSPITAL.

Summer Session 1845.

By C. H. F. BOUTH, M.D. Lond.

[Continued from p. 189.]

Autopsy, twelve hours after death.—Temperature 70° Fahr.

The body was greatly emaciated. Skin very pale. Abdomen prominent; integuments over it somewhat discoloured. Lips and cheeks very pale also. There was a slight abrasion of the skin over the left knee-joint; no oedema or rigor mortis. Muscles pale and flabby.

The brain was not examined.

On opening the trunk, the lungs were found collapsed. The liver seemed to extend about one inch below the margin of the ribs. The abdominal contents found to be closely agglutinated together by recent lymph, containing about $\frac{3}{4}$ viij. of serum. About three or four inches above the margin of the pubis a perforation of the small intestine was visible. About one or two inches above this, two other spots, where the peritoneal covering alone was entire. Around the complete perforation before noticed, some fecal matter was found to have escaped in the cavity of the abdomen. These appearances, however, will be more closely described immediately.

The left pleura contained about $\frac{3}{4}$ j. of fluid. The left lung itself was soft and pale, studded with tubercles of recent formation, soft, plump, and pale in colour. There were several small cavities at the apex, about the size of small beans, containing broken-up yellow tubercle. In the lower lobe several small clustered tubercles existed, with congestion around them.

The apex of the right lung was firmly adherent. There was a large cavity, about the size of a pigeon's egg, and two or three smaller openings into it, containing a little grey softened tubercle, but no large bronchi entering into it. The apex to the feel was consolidated, and the substance of the lung at this part, except the innermost portion, appeared to be infiltrated with solid matter. The inner surface of the apex was of a dark grey colour, with scarcely any tubercle. The middle lobe contained also a large cavity: the lower lobe was thinner,

and also contained soft tubercle. Weight 3iv½.

Some of the bronchial glands were tuberculous, others not, and several containing carbonaceous matter, with a little cretaceous grit.

Heart was small; left ventricle somewhat contracted. The right auricle contained a polypus. Otherwise the structure of the heart was healthy. Weight 3viii.

On examining the *abdomen* more carefully, the intestines were found to be all agglutinated, of a greyish yellow colour; but the adhesions appeared recent, and could be readily torn through. A quantity of fine granular tuberculous points were found studded over the peritoneal surface, varying from the finest points to the size of large pins' heads, or small peas; over the peritoneal surface covering the right kidney they were larger. In the right iliac fossa there were 3ij. to 3iij. of fetid muddy-looking serum, on removing which four patches of ulceration were seen in the ilium, two of which had opened in the peritoneal sac, and allowed the contents of the gut to escape. Situated opposite, about the middle of the transverse ramus of the pubis, there was an adhesion, which, on being severed, exhibited an ulceration of the intestine at this part, which had proceeded to perforation. The upper surface of the liver was adherent to the diaphragm; the intestines generally so soft as to tear with the slightest force.

The *liver* contained a good deal of bile. The under surface was of a dark slate colour; the upper surface was studded with fine granular tubercles, but which appeared to be confined to the capsule, and not in its substance, as they came away easily, and could be separated with it. The pale spots of the liver were of larger size than usual, and the structure generally coarse, and otherwise unhealthy; the upper surface generally softer than the under. On cutting through the substance of the liver there were here and there some white spots, which at first resembled tubercle, but on closer inspection appeared to be only branches of the portal vein, cut through, thickened and tinged with bile by the biliary duct. The gall-bladder was full of bile. Weight 3lbs. 3xiv.

Kidneys.—Left supra-renal capsule much larger than usual. The upper surface of the left kidney had a little the appearance of coarse mottling, and here and there slight points of cacoplastic deposits in a separate form. The structure of the kidney was generally coarse. Weight 3iv.

The peritoneal surface of the right kidney was covered with tubercular deposits, varying from the smallest size to that of a hemp-seed. The upper layer of the capsule

was not adherent; the under was quite so, and could not be removed. The texture throughout was coarse. Weight 3vss.

The *spleen* was quite healthy, only a little larger than usual.

The stomach.—The organ appeared somewhat opaque, and the surface a little softened. The isolated pyloric glands were more conspicuous than usual, but there were no signs of inflammatory redness or ulceration about the mucous membrane.

Small intestines.—The ulcerations began in the duodenum, and there was an immense mass of hardened mesenteric glands filled with tubercular matter attached behind.

At the lower end of the ileum the peritoneal covering was found to contain in its substance several tubercular masses, which could be turned out, and were cheesy-looking in appearance. In addition, there were many opaque patches, with recent and tuberculous granular matter. These patches, in by far the greater number, corresponded to ulcerations within the gut, and *portions could be traced along the vessels*.

The internal mucous membrane presented the following appearances:—

Beginning in the duodenum there were several separate ulcers, varying from the size of a bean to two inches in length, and one broad. These ulcers were situated at the portion where the mesentery is attached, and spread transversely to the axis of the intestine. In no parts of the ileum or jejunum was there three inches of the intestinal tract free from them. The edges of some of these were quite pale; others, again, were more vascular. Some of the edges, again, were quite hard, and appeared to consist of tuberculous matter still remaining. In most of these, the mucous, submucous, and muscular coats, seemed to be completely eaten through by the ulceration, the peritoneal only remaining entire. Where perforation had occurred, the edges of the peritoneal surface corresponding were ragged. In some of the ulcers, especially the smaller, the edges of the tubercular mass were seen softening, the tubercle existing in largest quantity in the neighbourhood of the colon.

Large intestines.—The ilio-colic valve was remarkably ulcerated, and of a dark colour. At the commencement of the ascending colon there were three patches of ulceration—one about two inches by one, just above the valve; another four inches lower down, of the same size; and the third about the size of a bean, in position intermediate between the two others; *the long diameter of all the ulcerations was placed transversely to the axis of the intestines*. The rest of the colon was remarkably free

from ulcerations or cicatrices; indeed, quite healthy.

The clusters of mesenteric glands attached were all in a tuberculous state, varying from the size of half a hen's egg to that of a pea. Some of these were quite of a cartilaginous hardness.

The bladder was full of urine, high-coloured, sp. gr. 1018; no albumen. The upper portion of the ascending aorta contained a fibrinous clot of a quantity of dark black fluid blood. There were no eacoplastic deposits in any part of it, or in the iliac arteries.

Microscopical Appearances of the Tubercles. Examined by Dr. C. J. B. WILLIAMS. (400 diameters.)

No. 1.—Cut from the yellowest part of a cluster of miliary tubercles taken from the left lung.—It contained clustered granules of irregular shape and size, with a few long waving fibres here and there to be traced amongst them.

No. 2.—Squeezed from the softest part of

a solid yellow tubercle in the left lung.—Granules clustered, as in No. 1, but without any trace of fibre, and a few more oil globules, breaking down readily with water.

No. 3.—Squeezed from the centre of a yellow mass in the mesentery.—Same appearances as No. 2.

No. 4.—Cut from a semi-opaque granulation in the peritoneum over the right kidney.—Granules closely impacted in a few fibres.

No. 5.—Cut from an opaque granular deposit under the peritoneal surface covering the small intestine near an ulcer.—The same as No. 4, but with less trace of fibres.

There was no appearance of pus globules in any of the five specimens examined.

I had also an opportunity of examining under the microscope several portions of the tubercles, and they agreed in every respect with Dr. Williams's account. I only observed, however, the fibres very distinctly in one specimen, and that taken from the lung.

State of Urine during Life.

Date.	Reaction.	Lithates, Sp. gr.	Albu- men.	Sp. gr.	Ounces passed in 24 hours	Diet.	Microscopical characters	Quantity of solid matter passed in 24 hours. Grains
June 25	Very acid.	Some.	None.	1090	—	Low.	—	—
" 26	Do.	Do.	Do.	1029	—	—	—	—
" 27	Acid.	None.	Do.	1027	15	—	—	471
" 28	Do.	Do.	—	1027	12	Middle.	—	376
" 30	Do.	Do.	—	1026	12 saved.	Chop daily.	—	—
July 1	Do.	Do.	—	1016	6 saved.	—	—	—
" 3	Very acid.	—	—	{ 1024 1081	{ 8—A.M. 6—P.M.	—	Large quantity of epithelium scales; hairs.	447
" 4	—	—	—	{ 1028 1027	{ 28—A.M. 8—P.M.	—	—	844
" 5	Neutral.	—	—	1021	8 saved.	—	—	—
" 7	Acid.	—	—	1031	24	—	—	858
" 9	Do.	—	—	1031	26	—	—	929
" 10	—	Some.	—	{ 1031 1035	8 saved.	—	Lithates; hairs.	—
" 11	Very acid.	None.	—	{ 1030 1033	{ 18—A.M. 1—P.M.	—	—	659
" 12	Do.	↓	—	1030	12	—	—	412
" 14	—	None.	—	1027	4 saved.	—	—	—
" 15	Acid.	—	—	1026	8—A.M.	—	—	552
" 17	Very acid.	—	—	1025	12—P.M.	—	—	—
" 18	Do.	Some.	—	1023	12	Fish.	—	322
" 18	Do.	—	—	1021	18—P.M.	—	Triple phosphate; uric acid; tabs.	—
" 20	Do.	—	—	—	—	—	—	—
" 21	Do.	—	—	1027	12—P.M.	—	Uric acid.	—

REMARKS.—The above is a most interesting case of perverted or mal-nutrition, well illustrating the influence of such a state in determining the formation of aplasia and tuberculous matter, where, under ordinary circumstances, lymph would have been effused.

Diagnosis.—The principal complaints made by this patient on his admission had reference to his stomach. He experienced here a feeling of uneasiness, sometimes amounting to severe pain, after eating. There was tenderness at the epigastrium, nausea, loss of appetite, flatulence. These symptoms might be referred to three conditions of the stomach—1st, simple gastrodynia, or gastralgia; 2d, chronic gastritis; 3d, organic disease.

Now, the present symptoms were more those of gastritis, in the early history of the case perhaps it was only gastrodynia, the pain being relieved by warm drinks. Now, however, the reverse was the case: warm drinks increased, but cold relieved the pain. These are symptoms of gastritis. Moreover, the tongue was dry, red, and lobulated: in gastrodynia it is usually pale and furrowed, and may be moist. It has moreover an oedematous kind of look, not here remarked. Moreover, the nature of his occupation was such as might give rise to chronic gastritis. His gums had a blue line upon them, which an excess of lead in the constitution produces. Now one of the properties of lead is that of an irritant poison. There was, moreover, very much thirst. There was therefore very clearly gastritis present; yet it could not be considerable. The pulse was not quickened; the skin was cool and moist; and there was no actual vomiting.

But there was also certainly something more than mere gastritis. Here was a patient very much emaciated, with the abdomen very prominent and large. Moreover, there was habitual costiveness. By reference to the previous history, moreover, there had probably been melæna. Here were marked symptoms of organic disease. Was it, however, tuberculous or cancerous? First, in regard to the latter supposition: no tumor or ulceration could be detected in the stomach; and, although his face was sallow, yet it did not bear the marked sallowness of cancer. It is true, in tabes, the stools are not usually bloody; but where ulceration co-exists in the intestine, even in tabes this result would necessarily be observed. Moreover, the thoracic symptoms were rather those of tubercle than cancer. There were some signs of consolidation in the upper lobe of the right lung. It was duller on percussion, and the expiration was too loud. The pain on respiration or dyspnoea was trifling. Now cancer of the

lung is a very rare disease; and in cancer, moreover, the dyspnoea is usually more marked, and the pain greater. Then his hereditary predisposition might be considered as phthisical. His father died from bursting a bloodvessel (phthisis?). In addition, he had been exposed to causes favourable to the production of tuberculous growths.

On the other hand, his father's death might have resulted from hæmatemesis. He himself denied his family were consumptive or scrofulous. His pulse was natural as regarded frequency; he had never had hæmoptysis; nor was he liable to coughs.

Yet, while it could not be positively stated he had not cancer of the lung or stomach, the weight of evidence seemed to be on the side of tubercle.

II. There was also some hepatic congestion, as indicated by the increased dullness on percussion over this region.

Treatment.—The indications were threefold—1st, to regulate the bowels, the continued constipation of which might have had a considerable share in the production of the gastric symptoms; 2d, to palliate, and, if possible, to remove the gastric affection, by direct means; 3d, the extreme weakness and emaciation of the patient implied the necessity of supporting him, as well as of improving the general tone of his habit. This, however, could not be attempted until the second indication was fulfilled, and the irritability of the stomach in some measure subdued.

To fulfil the first indication, he was purged with calomel and colocynth, and castor-oil at different times. To fulfil the second, prussic acid and carbonate of soda given. This mixture experience has shown to be peculiarly serviceable in cases of gastralgia, or chronic gastritis, and not only to lull the pain, but also the general sensibility, and thus to give time for the use of measures more calculated to produce a permanent impression upon the system. To fulfil the third, he was first put upon low diet, which, as the gradual improvement in the stomach symptoms took place, was superseded by middle diet, and finally a chop daily.

But the complication of organic disease could not be overcome; and thus, although the treatment employed was so successful that all pain at the stomach ceased, the sensation of fulness after eating disappeared, the liver resumed its natural size, the night sweats were less copious, and he was even enabled to get up,—still the organic disease progressed with fearful strides both in the lungs and stomach, and very soon all hopes of his recovery were given up.

First the castor-oil seemed to disagree with him. This is by no means an unusual occurrence. With some persons it always produces these results. The pain in the stomach returned, though perhaps it was not quite so bad as on his admission.

The symptoms on the 5th were peculiarly interesting. It was observed the *vocal fremitus* was transmitted through the abdominal parietes. This would necessarily imply a direct solid communication between the lung and the abdomen, through which the sound could be continuously conveyed. The most probable explanation was the presence of tubercle in the lungs; and tabes mesenterica. The enlarged glands, however, could not be then detected, owing perhaps in a great measure to the tympanitic state of the abdomen; nor did the examination of the back, by making the patient lie on his belly, throw any more light on the diagnosis. The examination made on the 8th, however, did, and was conclusive as to the increase of the tubercular disease in the lung. At first, as has been seen, the right side had been duller than the left; then, however, the expiration was only heard. Now, however, both in front and behind, it was tubular. The resonance, it was true, was still considerable, but his extreme emaciation explained this. As the pain at the stomach was no better, a blister was applied, and the dose of the hydrocyanic acid increased. It is interesting to note, that notwithstanding the serious complications that occurred subsequently in the course of this patient's disease, yet that as a result of this treatment no pain was again felt after eating, and the only inconvenience was occasionally a little fullness. As the case progressed, however, other more unfavourable symptoms presented themselves. One of these was sleeplessness, to remove which, first hyoscyamus, subsequently morphia, were given, but with little benefit. His debility and emaciation increased. Aphthae were formed on his tongue and inside his cheeks, depending, no doubt, on the general atony and cachexia present. Bark was therefore given.

On the 15th his countenance began to assume a very peculiar expression; thoroughly deprived of the slightest anxiety, and expressive of the greatest indifference. This character of the countenance we have noticed on several occasions where most extensive organic disease was present. Symptoms of tubercular peritonitis accordingly soon presented themselves. Ascites without pain occurred. On the 16th those of ulceration of the intestines were superadded: a large quantity of blood was passed by stool. From its appearance, most of it being dark and pitchy, it was inferred the greater part came from the upper portion of

the intestinal tract. The presence, however, of clots of a more florid colour also seemed to indicate that some of it might also come from the lower portion of the small intestine, or upper part of the lung. The question again presented itself—was it carcinomatous or tubercular ulceration? In support of the first there was only the foetid odour which was supposed at the time to resemble that of cancer. What, however, has been said before, applies equally well here again. The weight of evidence was in favour of tubercle. In tabes mesenterica, moreover, the stools are very offensive.

On the 17th the hemorrhage again recurred, and for the first time, owing to the less tympanitic state of abdomen, something like a tumor was detected in the region of the pylorus. Now here was a puzzling symptom, supposing the diagnosis above given to be correct. Could this not have been cancer of the pylorus? But the negative even here seemed to be the more probable opinion. Cancer of the stomach usually produces sickness and vomiting; pain, moreover, is at this advanced stage almost invariably present. 2. This tumor might simply have been enlarged mesenteric glands, which M. Guersant believes is a disease equally common in the adult as in the child. Moreover, the same objections before made applied with equal force at present.

The blood evacuated on the 17th was less pitchy. As, however, it contained but very little fecal matter, it was thought advisable to give him a gentle purgative. This put him to severe pain. On the 21st diarrhoea set in, with copious loss of blood. At one time, moreover, most acute pain was felt at one spot, accompanied with great faintness, which lasted two hours. Under the influence of wine, but particularly of opium, he was recovered and relieved for the time. Viewing this symptom in connexion with his previous sufferings, it appeared probable that perforation of the intestine had occurred; and it is interesting to note here the beneficial effect of the opium, given even in small doses, in relieving this pain for the time. The same night, however, a second perforation seems to have occurred, in which the collapse was more marked, and which proved fatal. Throughout the patient's illness there appeared to be no disease of the kidney. The urine was sufficient in quantity; the excreted solid matter not defective, and bearing a fair proportion to the ingesta taken. Purpurine, so often detected in phthisis, was absent. The urine, indeed, gave with nitric acid some abnormal blue reactions; but when heated it was unaffected by hydrochloric acid. Uric acid crystals were present; but dyspepsia is a

common cause of their occurrence, and their presence, therefore, in no way facilitated the diagnosis.

The post-mortem appearances were full of interest.

1. *Tabes mesenterica* was found, and advanced even to the state of ulceration. The insidious nature of the attack is curious. During the first weeks of his residence in the hospital the symptoms were almost entirely confined to the stomach. After death this organ appeared least affected; there might indeed have been a slight amount of inflammation of the mucous membrane, as indicated by the somewhat opaque rugae and softening; but the inflammation in the intestines was probably then also equally and even more advanced, and yet they were remarkably free from pain, and all diarrhoea was absent. Either, therefore, we must suppose that the ulceration and inflammation of the intestines only occurred at a late period, and progressed with great rapidity, to account for the absence of pain at first; or that the disease was in some respects masked by the presence of lead in the system, producing constipation, and thus accounting for the absence of diarrhoea.

2. In relation to the phthisis present. If we except the emaciation, and hectic, and the night sweats, all of which might have been produced by the *tabes*, the symptoms of phthisis were certainly less marked during life than we should have been led to expect from the post-mortem appearances. There was no cough or dyspnoea. There had never been any hæmoptysis. The pulse was remarkably slow and regular. Moreover, the dulness was never very great, though sufficient to denote disease in the upper lobes of both lung. This is explained by the degree of emaciation. Two cavities were found in the right lung. The respiration had never been cavernous or gurgling during life; simply bronchial, and that only towards the last days of his illness. The rapidity with which the disease progressed may explain this in some measure. Perhaps, had he been examined on the day of his death, it had been cavernous; but apart from this suggestion, there can be no doubt Laennec laid too much stress on cavernous respiration as being always present where a cavity existed. All the signs of a cavity may be present, and yet there may be no cavity, and vice versa. A simple bronchus abscess, surrounded by consolidated lung, will give rise to cavernous respiration, especially if a large bronchus, and in the neighbourhood of the trachea, from which indeed the sound may be occasionally transmitted. Moreover, if no large bronchus open in a cavity, and more especially if the cavity be not very large, no ca-

vernous respiration will be heard. This was the case here.

3. We have already noticed more than once the transmission of the vocal fremitus through the abdominal walls. The post-mortem appearances explain satisfactorily this phenomenon. The posterior part of the lungs was consolidated, along the spine, continuously with the enlarged mesenteric glands, which, by their projection anteriorly, and connexion with the liver, afforded a continuity of solid matter to the abdominal parietes for the transmission of sound.

4. The extreme rapidity with which the disease progressed, though already more than once incidentally alluded to, may be especially insisted upon as most remarkable in this case. No case could more beautifully illustrate the occurrence of disease from perverted nutrition, and the insidious nature of such affections generally, with so few prominent symptoms, so that scarcely a portion of the vital organs shall remain unaffected, and yet the patient, till but within a very few days of his death, shall not complain; so little do material sensations or mental feeling appear to be connected with it.

Causes.—I. *Predisposing.* Of the gastric affection; a. The previous attack of dyspepsia. This, by debilitating an organ previously healthy, would render it more obnoxious to a second attack.

b. *Imperfect nourishment.* While a confectioner he had eaten at very irregular intervals, and chiefly articles of confectionary. Lately, since his misfortunes, his food appeared to have been bad in kind.

c. His occupation as a painter. Lead is also an irritant. Moreover, painters generally are liable to disorders of the nutritive functions.

d. His residence damp and close.

e. *Mental anxiety*: a very powerful debilitating cause.

f. *Hereditary predisposition.*—His father probably died of phthisis. All these causes, by affecting his strength, and weakening his constitution generally, would equally act as predisposing causes to the occurrence of the tubercular affections in the lungs and abdomen.

Exciting cause.—The general opinion at the present day of the occurrence of *tabes mesenterica* seems to be, that the glands only become affected after the mucous membrane has been so. This man certainly appears to have suffered first from gastritis, probably also from the inflammation of the mucous membrane of the intestines. This primary affection may therefore be considered as the exciting cause of the *tabes*.

In accordance with Louis' remark, the abdomen being affected with *tabes*, the

lungs were also found diseased. How far the one might be considered as the effect of the other, does not appear. No distinct exciting cause, except the combined influence of the predisposing, could be traced.

Proximate causes and pathology.—This was a disease developed in its purest form. It was albuminous granular matter, destitute of cells and fibres, which form the true matter of organization. The plasma of the blood was too degraded to form part of living structure, therefore injuring those parts on which it was effused, and causing obstruction.

2. The situation explained further the rapidity of its progress. The deposit took place chiefly in the intestinal canal and mesentery; thus, by attacking and impeding the functions of the principal organs of nutrition, causing starvation of the body.

3. This defective nutrition will in some measure account for the pain and inflammation being so disproportionate to the amount of disease present. The distressing and harassing symptoms of many cases of phthisis do not depend on tubercle alone, but chiefly on congestion and inflammation of the surrounding parts. Withdraw this inflammation or congestion, as by derivants, counter-irritation, local depletion, the symptoms are at once alleviated. Here, however, the blood was so poor in quality, and so deficient in plastic material, that the inflammation was therefore but very limited. Although in his case the most formidable lesion that can occur was present, perforation, its symptoms were by no means well marked, perhaps from the same reason. The inflammation (if indeed it might not rather be considered a non-inflammatory softening) that gave rise to the original ulceration, was trifling and very limited, probably from the same cause.

Prognosis.—The suspicion which existed from the first as to the presence of organic disease was unfavourable: as, however, he at first appeared so completely to improve, it was hoped he might have recovered at least from his present attack. This hope, as the case progressed, was soon abandoned. The fearful rapidity with which both the abdominal and thoracic complications advanced, justified the most unfavourable apprehensions. The occurrence of the hæmorrhage assured a fatal termination.

THE CHOLERA AT MALTA.

LETTERS from Malta of the 28th ult. state that the cholera is somewhat on the decline there. Eight died of the troops sent to Fort Chambray, Gozo, and the same number are ill. The 44th Regiment has lost 65 men, 10 women, and 9 children. The 69th lost only one, a sergeant; the Artillery only three men.

Medical Trials and Inquests.

KILKENNY SUMMER ASSIZES.

July 1850.

Trial for murder by poisoning with corrosive sublimate—Local and general effects of the poison—Death in fifteen days—Solubility of corrosive sublimate in spirit—Mercury not detected in the body.—Conviction in the absence of chemical evidence.

[THE subjoined case is of great interest in medical jurisprudence. The mode in which the medical evidence was given is highly creditable to the gentlemen engaged as witnesses for the prosecution.]

Michael Walsh was charged with the murder of Simon Power, a bailiff holding a distress on his premises on the 30th August, 1849, by administering to him a deadly mercurial salt in whiskey. The prosecution was conducted by Messrs. Scott and Sansae; and the prisoner was defended by Messrs. Harris and Armstrong.

The general evidence for the prosecution established that the prisoner asked the deceased and a witness, Murphy, to take some whiskey. Murphy felt a pain in his stomach after swallowing about a tea-cupful, which was poured out for him by prisoner. In about ten minutes afterwards, prisoner called the deceased to him, and according to the evidence, the deceased must have taken a cup of whiskey, while alone with the prisoner.

In a few minutes, he became sick, and although able to walk home with Murphy, he was very ill. Murphy deposed that he did not perceive anything remarkable in the taste of the whiskey which he swallowed. The condition of the deceased (Power) is thus described by his daughter.

Anastasia Power examined—I am daughter of Simon Power; I remember his coming home from being keeping at the prisoner's; he came at six in the morning with James Murphy; he was vomiting; he went to bed; he continued very bad during the whole of that day; the vomiting continued until he died, in fifteen days afterwards; Dr. Boyd attended him a week after he was ill; he was very often sick and vomiting during that week; he was not able for that week to go about his business: every bit of him was sore; his mouth was sore and scalding, and the flesh inside his mouth would come away when he was spitting; I could pull out the flesh and it was black; his teeth were also black; the odour of his breath was most

offensive; the water from his mouth was green and thick; it was often coloured bloody; he complained of pains cutting him in the bowels, and of pains in his head; before he died lumps of lard like came from his stomach; the day he came home his cheeks were yellow; after some time his head and cheeks swelled up and became red; the middle of his tongue was white, and the borders red; he would start up when asleep and bawl as if in a fright; the first week his feet were warm and sore, and they got cold the last week. *Cross-examined*—

My father ate his supper on the night of Wednesday before he got sick; he ate peas for supper: he never went about his business after that Thursday. *Re-examined*—The supper of peas was a usual one.

To the Jury—My father had never had all those symptoms before; (he also had purging mixed with blood).

To the Court—During the first week he never went out; he never could have gone to Walsh's ground to keep.

Surgeon Boyd examined—I attended Simon Power; I saw him on the 6th of September: I prescribed for him on the 3d for a sore mouth; I found him lying in bed on the ground with a profuse flow of saliva from his mouth, with a most disagreeable odour; his tongue very much swollen, and partly protruding, indented at the sides and thickly coated; the gums were ulcerated, mouth generally swelled, with several whitish stains on it, with ulceration of the palate behind the teeth; the face was swollen and red; he complained of great inability to swallow; he could not eat, and spoke with difficulty; his pulse was excited; I got him taken out of bed to examine his body to see if he had been rubbing his body with ointment; I could detect nothing; I asked him if he had been taking medicine; he said not, but that that day week he got something from the prisoner in whiskey which was not right—that immediately after getting the whiskey he became sick—that he was not able to remain that day; that off and on he was in that day; that on swallowing the whiskey it had a very queer burning taste, and felt a burning in his mouth and throat: that about the third day his mouth became salivated and continued so; that he had a pain in his stomach; the chief pain he complained of was about the mouth and head, and he had never used any mercurial preparation, except four years before when he was in fever; that he had frequently drunk whiskey without any ill effects from it, and was in rude health before he took the whiskey; I saw him again on the 8th, 10th and 13th; the salivation continued, but the vomiting not so frequent: he was weaker on the 13th; there was a flow of blood

from his mouth on the 13th; from the symptoms I perceived I think he died from the effects of mercury; corrosive sublimate is a sort of mercury; it could have been administered by an ointment rubbed to the body, or by its being given in a drink; I consider it must have been administered to him in drink from what he said. *Baron Pennefather*—You are at liberty to say from the appearances or non-appearances whether you think it was administered externally or internally. *Witness*—I believe it was administered internally: corrosive sublimate is soluble in whiskey, and would not colour the drink, or have any smell; it would have a dry caustic taste, like as if a man got a taste of lime when in a kiln; from the symptoms, and from what the man said, I believe he came by his death from corrosive sublimate taken inwardly; it is a deadly poison; I made a post-mortem examination in order to send the stomach and its contents up to Dublin to be analyzed; a portion of the mouth was also sent up; the liver, kidneys, and spleen were sent to him on a second occasion; I gave the first parts to Sub-Inspector Rogers, and the second to Constable Coghlan; they were sealed up in a jar; on opening the belly the peritoneum was healthy, but the mesentery was red; the inspection of the body gave me no additional evidence, but my object was rather to remove the contents to have them analyzed; there was a dull slaty appearance in the stomach, the consequence of inflammation; the longer the person lives the less appearances of the poison after death. *To Baron Pennefather*—I consider the man to have died from weakness, the corrosive sublimate interfering with the digestive organs, and also from hemorrhage. *Cross-examined*—Cholera and dysentery were very prevalent through the country at the time; he was a stout, strong, well-built man, not likely to be affected by dysentery; I attended him for the mercurial salivation; the first effects of corrosive sublimate when given in large quantities are to produce a burning taste in the mouth, and vomiting; the deceased described as accurately as any medical man the effects which would follow the taking of this poison, and I was confirmed in my belief of what he had taken by finding an ignorant man describing day by day with the utmost accuracy what symptoms should follow the administration of corrosive sublimate; all the appearances could not have followed from the administration of bad whiskey, in which there was inserted bluestone and verdigris; the sublimate is very soluble; it is soluble in thrice its weight of spirit, and would leave no sediment if pure; verdigris has no mercury in it; three grains of sublimate have been known to cause

death, but a drachm will kill several, and eight drachms may be dissolved in one glass of whiskey; one may take as much of the poison and escape from death that would kill three others; some are more susceptible than others of the influence of poison. *To Baron Pennefather*—If the sublimate was in powder still I would not expect it to have been so dissolved in ten minutes that the man would not see some appearances in the whiskey.

The information of Simon Power was then read, in which the deceased swore that he got the whiskey from the prisoner: that the whiskey was rough in his mouth, and had a shocking stinking smell.

Inspector Rogers and Constable Coghlan proved delivering the contents of the stomach, &c. to Dr. Geoghegan.

Dr. Geoghegan, Fellow and Profr. R. O. Surgeons, Ireland, examined by Mr. Sauss—The medical history of the present case, as given in evidence, unequivocally establishes that the death of deceased resulted from poisoning by a soluble salt of mercury, and, by inference, that the compound taken was corrosive sublimate; the *non-detection of poison* in the various organs of deceased is not alone compatible with the fact of death from that cause, but in conformity with the laws which govern the elimination of poisons from the body. In the present instance the illness was of fifteen days' duration, a period quite sufficient for the escape of the offending matter by the organs of secretion, particularly the kidneys and salivary glands. Witness did not expect to discover mercury in the alimentary canal, but thought that traces might possibly be found in the organs which had appropriated it subsequently to absorption; the salivation produced by the soluble salts of mercury may be of a two-fold character, arising either from their local influence on the mouth in the act of being swallowed, or from their absorption into the economy; the former is a spurious salivation, which may be immediate, and is occasionally accompanied by factor of the breath; the latter is the true mercurial influence, which is usually manifested after the lapse of a couple of days. *Cross-examined* by Mr. Harris—Corrosive sublimate is incapable of communicating any peculiar or fetid odour to whiskey; a wine-glass full of the latter fluid may in the course of a few minutes dissolve as much of the poison as would suffice to destroy life: on this point only, witness is unable to concur in the very intelligent evidence of Dr. Boyd. *To Mr. Harris*—I did not make any experiment on that point to reduce my opinion to a certainty.

Mr. Harris addressed the jury, and submitted that there was not such a case as

would warrant the jury in arriving at the conclusion that the prisoner was guilty of the offence with which he stood charged. Counsel adverted in detail to the evidence, and to the doubts which must arise as to the cause of death.

Mary Walsh, and another sister of the prisoner, and two men in the prisoner's employment, were examined in detail, and their evidence was to the effect that the whiskey was given freely to the deceased and Murphy at the same instant, and that they did not leave the house for some time afterwards. The two men, Dwyer and another, also swore that the deceased vomited the day before he got the whiskey, and was on the ground for days after taking the whiskey. On cross-examination their evidence varied in many particulars.

Mr. Phayre, an apothecary, deposed to having made experiments as to the solubility of corrosive sublimate; and his evidence went to show that it could not dissolve in ten minutes, or for a period much longer; but on cross-examination, said that in a few minutes the whiskey might take up a large quantity.

Mr. Scott wished to examine Dr. Geoghegan who had, since being on the table, made an experiment at the request of the Counsel for the Crown.

Mr. Harris objected, but this being part of the Crown's original case, Baron Pennefather concurred in it.

Baron Pennefather charged the jury, who returned a verdict of Guilty.

Baron Pennefather was about to pass sentence, but after consideration directed that the prisoner should be removed and brought up next morning, (Friday).

The trial was one of a very interesting nature as to its medical details, and the learned Judge in the course of his charge, after complimenting the Counsel for the manner in which they discharged their duties, also adverted in high terms of commendation to the clearness and ability with which Dr. Geoghegan, of Dublin, and Dr. Boyd, of New Ross, gave their testimony as to the cause of death.

* * One question of especial interest in this case was, whether a tea-cupful of whiskey, poured on corrosive sublimate, would in *ten minutes* dissolve as much as would suffice to destroy life. Admitting the corrosive sublimate to have been in powder, the act of pouring would so agitate this as undoubtedly to cause the whiskey to take up a fatal dose; and if in lumps, the lumps would have been seen and have excited the suspicion of the deceased. Mr. Phayre, an apothecary who appeared for the prisoner, swore in his examination in chief, that the poison could not be dissolved by

the whiskey in ten minutes, or even in a longer period; and although he professed that this very decided opinion was based on experiments, he did not hesitate to swear in his cross-examination that in a few minutes the whiskey might take up a large quantity! But for this open recantation, the fairness of Mr. Phayre's opinion might have been readily tested by Counsel requesting him to drink some whiskey which had been poured on powdered corrosive sublimate and allowed to remain ten minutes!

It appears to have been a refined piece of cunning on the part of the prisoner to give the whiskey about the same time to two persons, but to take care that the cup should be only fully poisoned to one. This, with the non-detection of mercury in the body, might have overthrown the case, but for the convincing and judicious evidence of Dr. Geoghegan and Mr. Boyd.

Correspondence.

RELATIVE FREQUENCY OF THE PULSE AT DIFFERENT ELEVATIONS, AND AT DIFFERENT PERIODS OF THE DAY.

SIR,—If not trespassing too much on your columns, might I ask you kindly to permit me to make a few remarks on Dr. Bellingham's lecture, published in your columns of the 1st of March, with respect to the relative frequency of the pulse at different elevations and at different periods of the day. My opportunities have been pretty extensive, and my attention particularly directed to the pulse for the last 15 years at least.

1st. The pulse of a soldier in health, before breakfast, say 7 A.M. (in Hospital), in the recumbent posture, and free from nervous excitement, may be reckoned, according to my experience, at 60 or 64, and in the evening a few beats more, the difference depending upon the degree of debility. It is least frequent in the morning, however, when first convalescent from remitting fever; 54, or even lower, is then by no means uncommon. No doubt, as you have said, numerous exceptions will occur, depending chiefly upon collateral circumstances, as before or after breakfast, before or after dinner, fresh or fatigued, after a restless night or refreshing sleep; or it may be only these that make the difference; for if hungry, or fatigued, with the skin soft and perspiring, it is certainly lower in the evening.

2. I have never noticed the least difference of the pulse from elevation, whether

at Madras or Bombay, on the level of the sea: Poona and Belgaum, 2000 feet above it: Candahar 4000: Kelat or Cabul, 6000; or Ghuznee, or along the range of the Suliman Mountains, 8000 or 9000. Along the latter too, we several times changed our elevation from 1,000 to 2,000 feet in a day; so I cannot but think that the presumed difference has been originally based upon theory, and continued from generation to generation without further inquiry.

Very truly yours,
B. H. A. HUNTER,
Staff Surgeon.

We are all too apt to fancy, perhaps, that nothing is worthy of publication except it be extraordinary, or at all events something out of the way, and so it has occurred to me to test this, by throwing together a few common-place practical remarks, just as they present themselves, though if approved of we may extend them over all the more ordinary fatal diseases of the East and West Indies. The value of such remarks, however, being in some degree proportionate to the opportunities enjoyed, it will be necessary to premise with the usual "*experientia haud non paucorum annorum*," or, as that might be deemed too indefinite in the present instance, with a simple detail of the facts.

Our personal experience may be pretty nearly summed up in 28 years' service as an Army Medical Officer, (regimental chiefly*) in a variety of climates and localities; in Ceylon, to wit, 1½ years, Madras 1 year, Doab and Deccan 5 years, Bombay and Colaba 3½ years, Ghuzerat or Marwar 2 years, Scinde and Afghanistan, from the mouth of the Indus to Cabul, 1½ years, in the West Indies, (Barbadoes and St. Vincent), 1½ years; about three years at sea, mostly with troops, and the remainder in England.

We shall commence with cholera, for this was the most ordinary fatal disease during a great part of the period embraced, and of this we may say, cholera has in our experience been decidedly contagious, that it is not communicated by contact, as some would ingeniously limit the term, but by some subtle self-multiplying principle, like that of scarlet fever, typhus, or plague, though, if pressed for the proof, we could adduce abundance either ways. We could trace repeatedly its introduction and subsequent progress from individual to individual, and from place to place; or shew, on the other hand, that ourselves as

* One advantage a regimental medical officer possesses, is that if his patients are only scratched, they return again and again upon him till they die, or are invalided incurable.

well as many others have been in the midst of it for days, and have even, in the height of our zeal, sat by the bedside watching every movement or change in the temperature and pulse, almost from its commencement till its termination in death, without ever being affected with either cholera, colic, or diarrhoea. Nay, further, we might leave the domain of legitimate induction, and, mounted upon the wings of imagination, penetrate the dark portentous cloud, and there exhibit the cause of cholera in myriads of insects, like a dense flight of locusts darkening the air, hanging over the camp, or pursuing the tract of some devoted column. We might point to it amid the flowing Stercoraceæ, or exhibit it in the uredo fungi; or give it an aerial form, and trace its deadly course in the blast of the simoon. In short, could we collect a little of the marvels of its most marvellous origin and progress on various occasions, Ceylon* might stand abashed, whilst "Joe Miller"† we proclaimed *yet a man of straw*, and burnt him in the fire.

But, be the cause of cholera what it may, or its mode of propagation, nothing in our experience has so certainly predisposed to it as diarrhoea and dysentery; so much so, indeed, that on one occasion we lost every one of the class in Hospital at the period of its introduction, whether recently admitted or convalescent, in the short space of 36 hours, (of these, no less than 4 or 5 went off with the first funeral party, within twenty-four hours); and on another occasion, in the same regiment, meeting a detachment of 50 sick and wounded returning, it cut off 16 of the 19 with "chronic bowel complaint," in three days. Diarrhoea and colic too, that is, vomiting and purging without cramps or collapse, we have frequently noticed precede and also accompany cholera, and have even by the character of these been enabled to predict confidently its approach. Nevertheless, we have seen hundreds at a time, not less than one third of the regiment, suffering from serous diarrhoea, (both officers and men) several of them in a most violent degree, and yet not a case of cholera, nor yet a death in the corps from any disease even for months. I more especially allude to Tatta, on the Indus, in January, 1839, but more or less throughout Scinde and Afghanistan. It is true we were then ahead of the

cholera,—that is, where cholera had never been,—for hitherto the sword of the Beloochi and knife of the Affghan had to that proved an impenetrable barrier. No sooner were these removed, however, than it followed up in the rear, and the same year, as far as Kwetiah above the Bolan, was extremely destructive, as it was some years subsequently at Caubul, causing the city for some time to be almost deserted.* It was no doubt these interminable deserts, with their thinly scattered ferocious inhabitants, that so long kept the cholera within the bounds of the Indus; and even when it did get to Persia it was only by outflanking these, taking the sea route direct from Bombay. It certainly did not pass through Afghanistan or Beloochistan.

It has been too hastily inferred, we think, on various occasions, during the prevalence of this epidemic, that where diarrhoea has been checked or cured, cholera has been prevented. Not one in ten, perhaps, of such cases would ever have terminated in cholera, whilst, on the other hand, we have seen cases repeatedly on the line of march, from their first falling out of the ranks, with a sense of faintness, squeamishness, diarrhoea or vomiting, which in spite of every remedy have run rapidly into collapse, and proved fatal almost before the day's march was completed. The same malignancy we have noticed in quarters, and have often puzzled ourselves for some new method of treatment, even while the temperature was still good and the pulse tolerable; for we fancy there is an expression and gait indicative of mortal disease from the beginning. Indeed, it would not be difficult to show, statistically, at least, that cholera has a certain ratio of mortality whatever be the treatment, and whether this be in the East Indies, in England, or America. The West Indies, at least the British portion, it has not yet reached, though there is reason to believe, (from an examination of the records on the spot) that sometimes the first cases on an outbreak of yellow fever have been "returned" as such.

Treatment.—On this we have nothing new to offer. We may say, however, we have tried with and without venesection again and again, and certainly do think, if it can be practised with moderate effect, that it affords a much better chance of reaction, even though vomiting be induced, and the pulse sink for a time. Next to that in importance, we reckon Calomel gr. xx. c. Opii, gr. ij., notwithstanding the same

* "Ceylon," proverbial in the Army for its marvels.

† "Man of Straw."—Dr. Kennedy, in the introduction to his "Campaign in Afghanistan," says Joe Miller is no "man of straw," as he once was led to suppose, for he saw the veritable gentleman himself at the Cape of Good Hope.

* This is the usual way with the Natives of the East on any severe outbreak, and seems to be the natural way of dissipating the contagion. It is well to keep this in view.

has in this country been declared inefficacious, or in the more delicate Pil. Hyd. gr. x. c. Opil, gr. j. repeating the one or other once, twice, or oftener, till the vomiting and purging be allayed, and afterwards in smaller doses alternately with some such mixture as the following:—Mist. Camphor. Liq. Ammon. Acet. ss. ʒij.; Spirit. Ether. Nitrici ʒss.; next a strong blister to the epigastrium, so that, if possible, it may be coming into play about the time reaction is most likely to commence. For this the mustard poultice is too evanescent, and lastly stimulants. Of these we have tried a great variety, scarcely missing a new suggestion, but have found none in military practice superior to cold brandy and water. Frictions relieve the cramps of the calves of the legs, but, where these are abdominal, (as we observed in the English cholera of August, 1826) venesection, with the warm bath, and the large doses of calomel* and opium, already mentioned, are best. In such cases we augur less danger. In the Asiatic variety they more usually occur when the epidemic is about to subside. There is yet another variety of cholera, which we might call spurious, constantly occurring in regiments in the East Indies. It appears to be caused by poisoning, supposed often to be from stale sausages or pork, which have been kept in copper vessels, hawked about clandestinely by the natives. We have also seen it occasioned by arsenic, or something so very like cholera, where cholera is so common, that it was never suspected even, till the arsenic was found in the stomach. The former, however, are very seldom fatal.

We have said nothing of secondary fever, because we do not look upon that as essential. It seems rather to depend upon the protracted local congestions, or inability of the system to restore the circulation in these parts. It is less frequent in the east, where the course of the disease is more rapid than in England.

Prophylaxis.—As this paper is meant to be strictly practical, it will only be necessary here to notice such precautionary measures as we have ourselves known adopted, and the results. In August, 1833, cholera broke out in the hospital of the wing of the 2d, or Queen's Royal Regiment, in the town barracks of Bombay. The men were much crowded at the time, and with only a temporary hospital, separated off from one extremity of the rooms by a partition of straw matting eight or

ten feet high. The first affected was a native "ward-boy," who had been out in the native town. This was in the forenoon. At 5 P.M. the first of the patients in hospital was seized; and whilst I was with this patient, about 7 or 8 P.M., two others in the same ward; and, by midnight, one or two more. At daylight 16 palanquins were engaged conveying away the other sick to the Regimental Hospital on the island of Colaba, about two miles distant. Next morning those sufficiently recovered, were sent off in a similar manner, and again that evening and following day, to the General European Hospital in the Fort, which was near at hand. By the second evening there was a marked change in the character of the disease, and by the 5th it had altogether subsided. Its progress was from the extreme of collapse, almost without cramps, to strong spasms of the legs and abdomen. In all there were 54 cases of cholera, and about an equal number of choleraic diarrhoea and colic; and, of the 54, 8 or 10 proved fatal. These were nearly all of the first cases. The regiment lost in all 29 by cholera that year, besides women and children; but it was three times epidemic—to wit, in May, in August, and in December.

Again, in September 1842, cholera, which for months previously had been approaching from the southward, and from village to village, with immense mortality, reached Deesa on the border of Marwar, where the 2d or Queen's Royal were then stationed. We had scarcely heard that a village fourteen miles on the Ahmedabad road was deserted, than one morning at four o'clock, a child, already in the stage of collapse, was brought to the hospital by its mother. The latter appeared to be in good health; but, fearing the effects of her assiduity, I desired her to lay it on the bed. At 9 A.M. she too was seized; and in the evening both mother and child were laid in the same grave. Next morning another child was brought at 4 A.M., in the stage of collapse, by its mother. The same thing was repeated in every respect, and again in the evening both were laid in the same grave. The disease was found to be in the regimental bazaar, where the married women particularly went for their daily supplies. The bazaar guard was immediately withdrawn, and placed in a tent between that and the barracks, and all direct communication cut off; whilst the people of the bazaar were set to turn that almost literally inside out. The disease continued to prevail for some time in the married men's lines, called the Patcherry, but particularly in the hospitals, both male and female (these were in the same enclosure, and only a few paces apart); but

* The large doses of calomel there is reason to believe act as a local application, but the blue pill and opium are very effectual, particularly in diarrhoea. A stock was always kept ready on the lie of march.

not a case was admitted from the barrack-rooms or pendalls, though these extended from within 100 yards of the bazaar to within 50 of the patchery. The apothecary, who resided within the hospital enclosure, died, and 10 men, besides women and children: but this was a very deadly cholera,—scarcely any recovered; and even two men and one woman, supposed to have been out of all danger for a day or two, relapsed, or rather, we might say, had a fresh attack, and died.

Again, in April 1840, in H.M.'s 17th Regiment, of which I was then in temporary medical charge, we had lost two men by cholera (sporadic cases) on the island of Colaba, Bombay, and three by *coup de soleil* on the 29th,—the day we crossed over to Panwell with the right wing, *en route* to Poona. At Panwell we halted three days, waiting for carriage from Bombay; but there was no report of cholera in the neighbourhood—(it was, however, in Bombay). At the end of the second march,—for we made a long march from Choke to Khandalla,—we met with a native regiment coming in, having this disease among them; and, in consequence, we went a mile further. Here we had a few mild cases, and one died, I think. Next day we encamped between their two former encampments half-way, and had none; but the next day at Waugaum, on the same ground. There was some hesitation about this; but there was no other place at all convenient, and the native regiment had been quite free when here. The same evening cholera broke out with all its usual virulence, and turned the rest of our route towards Poona more like an orderly retreat, with the wounded perpetually crying "Water, water!" It was proposed by the officer commanding to make a push for Poona, but I dissuaded him from this, and so we continued our ordinary rate and distances, using only the extra precaution of keeping the walls of the tents open. Several were carried into hospital at Poona with secondary fever of a bad type, but there were no fresh cases after the first march. In all, ten men died on this occasion, but no officers, women, or children. A similar course was pursued on this march with the 2d, or Queen's Royal, in December 1833, and with a similar result; and also by the main body of the Bombay column of the army of the Indus, on their return, crossing the desert between Baug and Shikarpoor. But a more decided step was taken with the left wing of H.M. 57th Regiment at Arcott, near Madras, in 1845. Here they left the cantonment, and kept circling round in short marches, at the distance of ten or twelve miles: 29 men died, but I believe there were no fresh cases after they

left the cantonment. The same object may be attained nearly by a more ready method (the same as we adopted at Kalat when crowded with wounded, and the position, in a military point of view, did not admit of alteration)—to wit, by pitching the tents one day a few paces to the rear, another to the front or side, and again upon the old ground alternately.

Bristol, June 28, 1830.

ON THE COLORATION OF WATER BY ALGÆ.
—CAUSE OF THE COLOUR OF THE RED SEA.

SIR,—I beg to be permitted to offer a few observations on the subject of the coloration of the water of the Red Sea, of which, in the last number of the *MEDICAL GAZETTE*, you expressed a doubt.

The coloration of the Red Sea is neither a constant nor a general phenomenon, but, like that of the Serpentine, periodical and partial. It is therefore not surprising that some travellers on visiting that sea should report that its waters are not, as has been supposed, of a red colour.

Nevertheless, the fact of the coloration of the water of the Red Sea rests upon authority which would appear to be conclusive—viz., the testimony of several observers, and amongst the number Ehrenberg, who was the first to notice the phenomenon, and in honour of whom the plant occasioning it has been named by the celebrated cryptogamic botanist, Dr. Montagne, *Trichodesmium Ehrenbergii*.

In my History of the Fresh-water Algae a long account is given of the coloration of water by means of algae, from which I extract the accompanying particulars in reference to the Red Sea, and which may possibly prove interesting to some of your readers.

I have the honour to remain,

Your obedient servant,

ARTHUR HILL HASSALL.

66, Park Street, Grosvenor Square,
July 15th, 1830.

"During the year 1823 (writes Dr. Ehrenberg) I made a stay of many months at Tor, upon the borders of the Red Sea, close to Mount Sinai. On the 10th of December I there saw the surprising phenomenon of the blood-red coloration of all the bay which forms the port of that city. The high sea, without the boundary of the coast, preserves its ordinary colour. The short waves of a tranquil sea bring upon the banks during the heat of the day amucilaginous matter of a blood-red colour, and deposit it upon the sand in such a manner as that, in the course of a good half-hour, all the bay with the reed-

ing tide is surrounded with a red border of many feet in depth. I removed from the water some specimens with glasses (?), carried them to a tent which I had near the sea. It was easy to perceive that the coloration was due to little tufts, scarcely visible, often greenish, and sometimes of an intense green, but for the most part of a deep red. The water upon which they floated was always colourless. This very interesting phenomenon, sufficient to afford a reason for the etymology of the name which this sea has received (an etymology up to the present time always buried in complete obscurity) attracted all my attention; and I examined it at leisure, with all the care of which I was capable.

"The phenomenon of the Red Sea was not permanent, but periodical. I observed it three other times—the 25th and 30th of December, 1823, and the 5th of January, 1824."

The same phenomenon of the coloration of the Red Sea occurred at a later period, and on a scale infinitely more extensive, to M. E. Dupont, a distinguished advocate of the Isle of Mauritius. The following is the very circumstantial and satisfactory account of that gentleman, as contained in a letter addressed to his friend M. Isidore Geoffroy Saint Hilaire:—

MY DEAR FRIEND,—You demand of me certain details in reference to the circumstances in which I gathered the *cryptogamic* plant which I sent you from the Red Sea, and which you told me appeared a new species. They are as follow:—

"The 8th of July last (1843) I entered into the Red Sea, by the Strait of Babel-mandel, upon the steam-boat the *Atalanta*, belonging to the Indian Company. I demanded of the captain and the officers, who for a long time navigated in these latitudes, what was the origin of this ancient name of the Red Sea,—if it was owing, as some have pretended, to sands of that colour, or, according to others, to rocks. None of these gentlemen could reply to me; they never, they said, remarked anything to justify this denomination. I observed then for myself, as we advanced, whether the ship approached by turns the Arabian coast or the African coast, the red was in no part apparent; the horrid mountainous barriers which border the two banks were uniformly of a blackish-brown, except where in some places the appearance of an extinct volcano had left long white streams. The sands were white; the reefs of coral were white also; the sea of the most beautiful cerulean blue. I had given up the hope of discovering my etymology.

"On the 15th of July, the burning sun of Arabia awoke me suddenly by shining all

at once from the horizon without spot, and in all its splendour. I turned myself mechanically towards the window of the poop, to seek a remnant of the fresh air of the night before the ardour of the day had devoured it. What was my surprise to behold the sea tinted with red as far as the eye could reach! Behind the ship, and on all sides, I saw the same phenomenon.

"I interrogated the officers anew. The doctor pretended that he had already observed this fact, which was, according to him, produced by the fry of fish floating on the surface; the others said that they did not recollect having seen it before. All seemed surprised that I should attach such interest to it.

"If it be necessary to describe the appearance of the sea, I should say that its surface was covered with a compact stratum of but little thickness, but of a fine texture, of a brick-red, slightly tinged with rouge: sawdust of this colour, of mahogany, for example, would produce very nearly the same effect. It seemed to me, and I said at the time that it was, a marine plant. No one seemed of my opinion; so, with a pail tied at the end of a rope, I was able to procure a certain quantity of the substance. This, with a spoon, I introduced into a white glass bottle, thinking that it would be the better preserved. The next day the substance had become of a deep violet, and the water had taken a pretty pink tinge. Fearing that the immersion would hasten the decomposition, instead of preventing it, I emptied the contents of the bottle upon a piece of cotton (the same which I remitted to you). The water passed through it, and the substance adhered to the tissue. In drying it became green, as you actually saw it. I ought to add, that on the 15th of July we were by the side of the town of Cosseir; that the sea was red the whole day; that the next, the 16th, it was the same, until near mid-day, the hour at which we found ourselves before Tor, a little Arabian village, the palms of which we perceived in an oasis on the border of the sea, below the chain of mountains which descends from Sinai, even to the sandy shore. A little after mid-day, the 16th, the red disappeared, and the surface of the sea became blue, as before. The 17th we cast anchor at Suez. The red had consequently shown itself from the 15th of July, towards five o'clock in the morning, up to the 16th, nearly an hour after mid-day,—that is to say, during thirty-two hours. During this interval, the steam-boat, making eight knots an hour, had traversed a space of 256 miles, or 85 leagues and a third.

"In the different works relative to Egypt and the Red Sea which I have had occasion

to read, I do not recollect to have found mention made of a similar fact. It appears to me, nevertheless, but little probable that it has not been observed by others. I reproached myself for not having questioned the Arabian pilot whom we had on board, and who for twenty years had traversed that sea. This idea unhappily presented itself too late.

► "If it should be in your opinion worth the pains, I would demand new observations of the surgeon or officers of the *Atalanta*, for it would be easy for me to write to them by way of Alexandria.—Believe me,

"My dear Geoffroy, &c.

"EVENON DUPONT."

Medical Intelligence.

FAT AND UGLY ANIMALCULA IN LONDON WATER.

THE recently published Report of the General Board of Health on the supply of water to the metropolis contains a curious piece of evidence by a Dr. Angus Smith, from which we learn, for the first time, that the bodily condition of animalcula forms an item in estimating the quality of the Thames water. It appears that specimens taken at Hungerford Market, and on the London side between Blackfriars and Southwark Bridges, "contained animalcules larger, fatter, and uglier than any preceding. One creature was observed about a thirtieth of an inch in size. When the deposit of mud was removed, and the water seemed clean, these specimens were allowed to stand some time. In a short time the flocculent matter spoken of was formed, brown like iron-rust, and the covering of one side of the vessel by the brown animalcules took place also on the side next the light."—*Report*, p. 41.

Unlike Dr. A. Smith, we are inclined to draw a favourable augury from the larger size and fatness of river animalcula. We say nothing of their personal beauty, for they have always appeared to us to be microscopic monstrosities. Large and fat animalcules must consume a larger portion of organic matter than the lean and consumptive-looking infusoria. They thus tend to depurate the water, and to render it more fit for the use of man. We have no evidence that living animalcula, fat or lean, are more unwholesome when swallowed in water than oysters, shrimps, or lobsters, and knowing the nutritious properties of the first-named mollusca, we decline to adopt the assumption that they are injurious to health.

In this report issued by the Board of Health, the term *animalcula* is invariably spelt *animalcules*!

TESTIMONIAL TO DR. CONOLLY.

A MEETING was held Saturday, August 3, at 12, Old Burlington Street, Lord Ashley in the chair, to take the necessary steps to present John Conolly, M.D., of Hanwell, with a public testimonial, commemorative of his invaluable and most successful labours in ameliorating the treatment of the insane; and a committee was appointed to carry the resolutions of the meeting into effect. It was resolved that the form of the testimonial should be a portrait, to be presented to Dr. Conolly's family, and an engraving from the same to be presented to the subscribers. The subscriptions are to be limited to five guineas; subscribers of two guineas and upwards are to be entitled to proof impressions of the engraving; and subscribers of one guinea to prints. Sir James Clark and Dr. Little were appointed treasurers, and Dr. Forbes and Mr. Frankum secretaries. It is not doubted that a very extensive list of subscriptions will be obtained, Dr. Conolly being one of the few men whose services in the cause of humanity have made a marked impression on society in his own day. Although not the originator or first advocate of the rational or non-restraint system of treating the insane, Dr. Conolly has been the great means of its introduction into general use, both in public and private asylums, by his strenuous advocacy of the system on all occasions, and by his own example at Hanwell. It is a fact that, since the first appointment of Dr. Conolly to the great asylum at Hanwell, now upwards of ten years, not a single patient (out of a thousand constantly there) has worn a strait waistcoat, or sustained any other form of restraint, except occasional seclusion for a brief space in the patient's own room. Such is at present the general treatment in most of our asylums, the mild and rational system being now almost the only one followed. A striking fact was mentioned at this meeting by Lord Ashley,—*vis.* that under this new system the proportion of suicides has decreased immensely; and all those who have been accustomed to see the insane are well aware that the very character of the disease, as it now shows itself in our asylums, has been extremely changed by the modern treatment. Dr. Conolly, as we have said, having been the great agent in bringing about this state of things, it is impossible to overrate the good he has done to his fellow creatures; and there can be no sort of doubt that the manner in which Dr. Conolly's professional brethren and the public at large will respond to the appeal of the committee, will prove how deeply the sense of his great services is impressed on the minds of his countrymen.—*Observer*.

A SPECIMEN OF MEDICAL EVIDENCE.

IN a case of manslaughter lately tried at the Bodmin Assizes, in which deceased died in a few minutes after having received two blows on the head with the fist, the surgeon is reported to have delivered himself of the following curious piece of evidence:—

"The surgeon said, he considered the deceased died from apoplexy, which might have been occasioned by the blow. He did not make a *post-mortem* examination, because the coroner's jury did not wish it. The witness, upon being pressed in cross-examination, went into all kinds of possibilities. The deceased might have been attacked with apoplexy, even without a blow. He was a stout subject, and therefore liable to apoplexy; and from the surgeon's evidence it would seem that, not having had a *post-mortem* examination, he could not tell much about it, and he could not say anything, in fact, that was of any value upon the subject."

On such evidence there could be no conviction for manslaughter. The prisoner, although he had undoubtedly caused death, could only be found guilty of a common assault.

THE BRITISH MEDICAL FUND.

AT a recent meeting of this Society, Dr. Forbes in the chair, it was stated, that subscriptions to the amount of £220. 8s. 9d., had already been received, which the committee thought was highly satisfactory, remembering that only a few weeks had elapsed since they were authorised to call for payments in respect of membership; and they had great pleasure in stating that the promises of support they had received, from all parts of the kingdom, had been most numerous, and the approval of the plan of the Society, by the profession, as far as they could learn, had been almost unanimous.

THE LONDON HOSPITAL.

THE Duke of Cambridge has become a Life Governor of this institution, and has consented to allow himself to be proposed for the office of President, at the next General Court of Governors.

ROYAL COLLEGE OF SURGEONS.

GENTLEMEN admitted members on the 2nd inst.:—Messrs. H. C. Warren—J. B. Allen—R. Leach—G. P. Rugg—M. Ingle—J. Hutchinson—T. Ball—E. S. Willett—T. P. Johnson—W. Draper—W. G. J. Ayre—M. C. Dukes—W. H. Walker—G. C. Attfield.

Admitted on the 6th inst.:—Messrs. E. Hunt—J. Chambers—J. N. Jakins—A. Thornton—W. W. Tinsley—J. H. Thomas—T. G. Fitzgerald—J. Rigby—T. Lancaster—T. Howell—J. Stephenson—Henry Nelson.

APOTHECARIES' HALL.

NAMES of gentlemen who passed their examination in the science and practice of medicine, and received certificates to practise, on Thursday, 25th July, 1850:—Charles Young, Lewes, Sussex—Henry William Bromley, Rippingale, Lincolnshire—John Hunt, Cirencester, Gloucester—Francis Salter, Wrexham, Denbigh—John Mathew Charlton, Durham—William Jackson, Bolton-le-Sands, Lancaster.

OBITUARY.

On the 22nd of May, at Rio de Janeiro, Christopher Fryer Curtis, Esq., M.D., in the 29th year of his age.

On the 28th ult., at Fareham, Hants, James Ainge, Esq., surgeon, aged 63.

On the 30th ult., at Aberystwith, in his 44th year, William Thompson, Esq., Surgeon, Kingston, Herefordshire, sincerely regretted and lamented.

On the 5th inst. at Cheltenham, James M'Cabe, M.D. aged 59. *R. I. P.*

Selections from Journals.

TREATMENT OF ANGINA PECTORIS.

DURING a paroxysm, after all evident exciting causes have been removed, the treatment will be active in proportion to the strength of the patient and the violence of the symptoms. If there is reason to suspect an effusion of blood into the medulla oblongata, active depletion would be indicated, as recommended by Parry and Burns, even in plethoric individuals, by venesection, leeches, or cupping over the spine. Whatever the supposed cause, opiates, antispasmodics, and carminatives, are usually given, with ether, ammonia, or other stimulants, if there be coldness and faintness. To these would be added counter-irritation by blisters, but especially by dry cupping over the spine. Besides these, and others, which would occur to every one, we would advise the inhalation of oxygen, as recommended by Dr. Robert Reid, in the *Dublin Transactions* for 1817. We have seen that the principal danger is from insufficient aëration of the blood; so that, by such inhalation, the immediate symptoms of suffocation might be relieved, and sufficient time gained for the removal of the exciting cause.

During the intervals, in addition to the numerous precautions and remedies detailed in the books, we would suggest the preventive and curative treatment of neuralgia and rheumatism, whenever these may seem to have any connection with the past history of the symptoms.

We think, however, that the great remedy must be *electricity*, which, according to Matteucci (quoted by Dr. W. F. Channing, *Medical Application of Electricity*, p. 10), differs from all other nervous stimulants in that it excites sensation at one time and contraction at another, according to the direction in which it traverses a nerve: which alone has the power of increasing or diminishing the excitability of a nerve, according to the direction in which it is made to flow; which has the power of awakening the excitability of a nerve, after all other stimulants have ceased to act. The form to be preferred depends on whether we wish merely to stimulate the filaments of the par vagum in the organs to which they are distributed, or to produce structural change in the medulla oblongata. In the former, electro-magnetism would be indicated; in the latter, the uninterrupted current of the battery. If electro-magnetism should fail in the first, the battery should be tried, as this has been found beneficial in paralysis without structural change, where the first has failed. Without careful diagnosis, therefore, harm instead of good might ensue. By stimulating the paralysed spinal accessory, not only is the vitiated air expelled from the lungs, but oxygen is admitted, which might also be increased by inhalation of this gas; so that these two remedies might be advantageously combined. A current in the direction of the nervous ramifications would stimulate directly the motor filaments; in the opposite direction, the sensitive filaments: but in the mingled filaments of the par vagum, reflex action will often cause similar effects, in whichever direction the current is passed. However, in cases of spinal irritation or inflammation, it would be improper to send a current towards the medulla.—*Dr. Kneeland, in American Journal of Medical Sciences.*

CHEMICAL ANALYSIS OF CHOLERA EVACUATIONS.

M. MIALHE has established, with M. Andral, that these contain neither albumen nor fibrin; but, on the other hand, that choleraic liquids yield an abundant precipitate with various chemical reagents, this precipitate being of an albuminous nature, the product of final digestion of albuminous aliment.

M. Masselet, from his experiments, observes—The presence of albumen depends upon the character of the evacuations in cholera; the rice-water stools do not contain albumen, but it may be found in considerable quantities (as much as four or five per cent.) in the yellowish viscid motions streaked with blood, and which deposit portions of epithelium and pus globules.

M. Masselet has also made the observation, that from the same patient the evacuations during life may yield but mere traces of albumen, while after death albumen may be found more copiously in the contents of the intestines. He also noticed, that the stools passed at the later period of the disease contain more albumen than the earlier evacuations.—*L'Union Médicale.* x

TRAUMATIC TETANUS CURED BY FRICTIONS WITH TINCTURE OF BELLADONNA.

M. BRESSE, surgeon at the Military Hospital of Rennes, proposed, in 1848, the treatment of traumatic tetanus by the application of tincture of belladonna, and reported a case, in which it had been successfully employed, in the *Gazette Médicale de Paris* of Sept. 30, 1848. M. Bresse has now placed on record another case which has come under his notice. The patient, one of the *Garde Mobile*, received a wound on the 20th of March; tetanic symptoms appeared on the 5th of April. Frictions of belladonna were commenced on the 6th, and by the 12th the patient was out of danger. Imprudently exposing himself to cold, the tetanic symptoms returned in the muscles of the back, but were quickly removed by again having recourse to the frictions.

The tincture employed was composed of five parts of extract to eleven of alcohol, and was applied all over the body, and more particularly over the rigid parts.

M. Bresse adds, that another practitioner has arrested trismus, which he feared would proceed to general tetanus, by the same means.—*Gazette Médicale.* x

ON THE ACTION OF STRYCHNIA IN CHOREA.

MM. TROUSSEAU and LASSGUE have been led to regard chorea as a partial paralysis; and they have found strychnia of great service in the treatment of this disease.

The form in which they have administered this preparation is that of the sulphate, which they have preferred, on account of its solubility. Their mode of exhibition of the salt has been by the solution of five centigrammes, = 77 English grains of the sulphate, in one hundred grammes, = 1500 English grains, of simple syrup. The dose for infants, of from six to twelve months old, has been 30 grammes, = 45 English grains of the syrup, every six hours; for children fifty grammes, = 75 English grains, and for adults sixty grammes, = 90 English grains. It may be observed that seventy grammes, = 108 English grains of syrup contain one centigramme, = 154 English grains, of the salt of strychnia.

The beneficial effects are seen in proportion as muscular rigidity occurs. If spasm occurs the dose requires to be diminished.

The dose in any case is to be very slowly and cautiously increased. When the cure appears to be effected it is still advised to continue the use of the remedy for a time, decreasing the dose very gradually—*L'Union Médicale*.

BOOKS & PERIODICALS RECEIVED

FOR REVIEW,

DURING THE LAST TWO WEEKS.

The Principles of Surgery. By James Miller, F.R.S.E. 2d Edit. Edinburgh.

On Bengal Dysentery, and its Statistics. By John Macpherson, M.D.

Medical Relief. Mr. B. Tatham's Case against the Huddersfield Board of Guardians.

Annual Report of the Royal Edinburgh Asylum for the Insane. 1849.

Description of a New Method of converting Gas and other Elastic Fluids into Fuel for Fire. By D. O. Edwards, M.R.C.S.

Annual Report of the Progress of Chemistry. By Justus Liebig and H. Kopp. Parts 4 and 5.

The Edinburgh Monthly Journal of Medicine. August.

The Dublin Quarterly Journal of Medical Science. August.

London Journal of Medicine. August.

Pharmaceutical Journal. August.

Annuaire de Chimie. Par E. Millon et J. Reiset. 1850.

Journal de Chimie Médicale. Juillet.

Annales d'Hygiène et de Médecine Légale. Juillet.

Comptes Rendus. Nos. 1, 2, 3, 4, 1 à 22 Juillet 1850.

AMERICAN PUBLICATIONS.

The Diagnosis, Pathology, and Diseases of the Chest. By W. W. Gerhard, M.D. Philadelphia.

A Treatise on Baths. By John Bell, M.D. Philadelphia.

A Lecture introductory to a Course of Surgery. By H. J. Bigelow, M.D. Boston.

Proceedings of the Medical Society of Pennsylvania. April 1850.

American Journal of the Medical Sciences, July 1850.

The New York Medical Gazette. Nos. 1 and 2, July 6 and 13, 1850.

The New York Journal of Medicine. July 1850.

Boston Medical and Surgical Journal. July 1850.

BIRTHS & DEATHS IN THE METROPOLIS

During the Week ending Saturday, Aug. 3.

BIRTHS.		DEATHS.	
Males.....	772	Males....	462
Females..	712	Females..	455
1484		917	

CAUSES OF DEATH.

ALL CAUSES	917
SPECIFIED CAUSES	917
1. Zymotic (or Epidemic, Endemic, Contagious) Diseases.....	277
Sporadic Diseases, viz.—	
1. Dropsy, Cancer, &c.	40
2. Brain, Spinal Marrow, Nerves, and Senses	99
4. Heart and Bloodvessels.....	38
5. Lungs and organs of Respiration	70
6. Stomach, Liver, &c.	61
7. Diseases of the Kidneys, &c.	10
8. Childbirth, Diseases of Uterus, &c.	7
9. Rheumatism, Diseases of Bones, Joints, &c.	11
10. Skin.....	3
11. Old Age	30
12. Sudden Deaths.....	2
13. Violence, Privation, Cold, &c....	26

The following is a selection of the numbers of Deaths from the most important special causes:

Small-pox.....	13	Convulsions.....	26
Measles.....	11	Bronchitis.....	22
Scarlatina.....	20	Pneumonia.....	28
Whooping-cough.....	18	Phthisis.....	123
Diarrhoea.....	136	Lungs.....	8
Cholera.....	11	Teething.....	7
Typhus.....	32	Stomach.....	6
Dropsy.....	17	Liver.....	13
Hydrocephalus.....	30	Childbirth.....	7
Apoplexy.....	17	Uterus.....	8
Paralysis.....	19		

REMARKS.—The total number of deaths was 103 below the average mortality of the 31st week of ten previous years. Of the 136 fatal cases of diarrhoea, 124 were among infants. Of the 11 cases of cholera, 6 were among infants. There is no appearance of the malignant cholera in the metropolis.

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer	29.85
" " " Thermometer ^a	61.3
Self-registering do. ^b	Max. 63.8 Min. 42.4
^a From 12 observations daily. ^b Sun.	

RAIN, in inches, 0.69.—Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was equal to the mean of the month.

NOTICES TO CORRESPONDENTS.

Communications have been received from Mr. Galloway, Dr. W. Addison, Mr. Lonsdale, and Dr. Day.

Mr. J. H. Tucker.—The circular did not reach us, or it would have been inserted.

Mr. T. H. Johnston.—We can enter no further into the matter. We wished at the time to publish the names of all the parties concerned, but could not obtain permission. That the facts were as published, is beyond all doubt. We saw the powders, and satisfied ourselves of the correctness of the analysis.

Lectures.

LECTURES

ON THE

MEDICAL JURISPRUDENCE OF
INSANITY.

*Delivered in the Medical School of King's
College, Aberdeen.*

BY ROBERT JAMIESON, M.D.

Lecturer on Medical Jurisprudence in the
University.

LECTURE II.

Medico-legal authority in insanity—Civil consequences of the state—Legal subdivisions; idiocy, lunacy, unsoundness of mind.—Legal doctrines.—Medical Classification; (a) general insanity; 1 amentia, 2 mania, 3 dementia; (b) partial insanity; 1 monomania, 2 moral insanity relation of the foregoing states to each other; doubtful and variable cases—General description of the insane state: the relation of country, age, sex, station, temperament, and social condition, to disease of the mind; (a) moral condition of lunatics—Premonitory moral disorder—The emotional features of insanity, selfishness, &c.

THE members of the medical profession have very important duties laid upon them in the detection and discrimination of insanity. They are certainly not more peculiarly qualified than any other equally intelligent class in the community to determine upon the evidence of mental inefficiency, or to come to sound conclusions on the important question of civil competency, and the more difficult subject of criminal responsibility; and, on this ground, their authority in cases of mental unsoundness has sometimes been querulously objected to, and their judgment undervalued. Leaving it to jurymen and judges to solve these questions authoritatively and judicially, there is no class better entitled, by education and general acquirements, to undertake the investigation of mental conditions, and none so well qualified to pronounce upon the point of mental health. It would, however, be to the interest of the public, and the credit of the order, that these particular medico-legal functions were discharged by the specially experienced section of the profession. The medical examiner alone has aids in his knowledge to guide him to a correct opinion as to the healthy or unhealthy state of mental organs, and the soundness or unsoundness of the functions

of the brain. He has had practice opportunities of learning how to distinguish mental from delirious, lethargic, hysterical, and other allied conditions, and can discover evidence by detecting symptoms which others would overlook, and appreciating indications which the non-professional would misconstrue. The physician is a jurist in cases of insanity, not on psychological, but pathological merits. He knows the phenomena of mental aberrations, and, with no greater metaphysical acumen than his neighbours, is better able to distinguish the results of disease. Insanity is the phenomenon of ignorance or vice as the effect of disease.

Insanity, when substantiated, annuls agreements entered into in that condition, deeds executed, and testimony given; and it is the duty of the medical jurist to examine persons suspected to be labouring under diseased mind, and to give evidence fitted to guide juries to a right decision on the question of the competency or responsibility of such individuals. He has to investigate the foundation for the pleading of insanity, when urged in criminal cases as a reason against trial, judgment, or punishment; and to testify as to the mental condition of parties whose wills, contracts, marriages, declarations, or confessions, are attempted to be set aside upon the same plea. It is upon the written certificates of medical practitioners that a legal warrant is obtained for the confinement of a person as lunatic in any hospital or asylum, by which his personal liberty is infringed, and, it may be, his civil privileges curtailed, and his responsibility to the law for a time suspended. After an individual has been so far recognized as insane, it is necessary, before curators or others can intrude with, or administer his estate, that his incapacity be shown to the satisfaction of a jury, as in a court of commission in lunacy, which is the mode in England, or in the Sheriff's court, which is the plan followed in Scotland. Medical testimony is a necessary preliminary to such proceedings, and in their course the professional evidence is the most important.

Before we turn our attention to the forms and varieties of mental derangement recognized by medical writers who have studied the subject, let me first introduce you to the divisions and doctrines which the authority of the law has established.

Lawyers speak of three forms of insanity—idiocy, lunacy, and unsoundness of mind—and attach to these terms the following meanings: *idiocy* is congenital insanity, and therefore always defect of mind; but the deficiency of intellect must amount to an extreme of imbecility; *lunacy* is an acquired insanity, not of necessity permanent, but

capable of lucid intermission, or complete cure; and *unsoundness of mind* implies such a degree of mental disease, or decay, as renders the subject of it unfit for the management of his affairs.

All who are idiotic, lunatic, or unsound of mind, are looked upon as incompetent in civil cases; but only the first of these classes are of necessity irresponsible for criminal actions. Lunatics, and others of diseased mind, are accountable for any crime not committed during such a paroxysm of their disorder as may be held to have subverted the power of distinguishing right from wrong.

It will appear to you, then, that we have, in law, three classes of the insane, and, as it were, two degrees of insanity—the lower degree vitiating civil acts; the higher absolving also from criminal liability. Now, the species of evidence admissible for the establishment of the former, or lower degree, will not suffice to substantiate the latter. In respect to a civil contract, proof of incompetency may be derived from any circumstances indicative of diseased mind, even from the character of the act itself; but in regard to a criminal offence, the evidence of irresponsibility cannot be derived from the action, but must be deduced from such other circumstances as manifest the individual's incapacity to distinguish lawful from unlawful.

A civil act, flowing from an insane delusion, will, on that very score, be invalidated, and deprived of efficacy; indeed, the rule is, that the civil deeds of a lunatic are void, even when they are not the results of insanity, though to this principle there are recorded exceptions. But a lunatic who has compassed a criminal action is not of necessity absolved from legal consequences, even should the offence have been a natural result of his insane delusion.

Such is a brief statement of legal doctrines; and they have given rise to much and intemperate criticism. They are the results of an imperfect knowledge of the disease, with a clear estimate of its practical consequences. The divisions are not nosological, but judicial: you have not mania, melancholia, and so forth, but only incapacity to learn the law—incapacity for civil freedom—incapacity to obey the law—and incapacity for personal freedom—the exponents of a diseased state conventionally divided into idiocy and lunacy, and, in order that the classification may be perfectly comprehensive, supplemented by a vague condition, called *unsoundness of mind*. A greater anxiety seems manifested to guard the lunatic's pocket, and the interest of his heirs, than to protect his life. He shall not by his harmless folly place either himself or his friends at disadvantage,

but a criminal action shall not interfere with his privilege of being condemned. Harmless acts may be evidence of his lunacy, but illegal ones shall not; for it is assumed that he may be blind to what is merely foolish, and yet at the same time able to distinguish and avoid what is sinful. The law has a ready eye for insanity in a civil deed, but will not even look for it in one that is criminal. Freedom of will is held to be quite compatible with diseased judgment; and the mind which is enlaved by a delusion is still presumed to have the power of disobeying it. The plea of civil incompetency can be based upon any action of the diseased mind; but that of criminal incompetency must be laid on a more restricted foundation. Disqualifications for the rational management of affairs where it exists, is in general readily seen in the confusion which it has created, and is always possible of legal proof; but non-responsibility for crime is frequently first suspected, in consequence of the act for which the individual is called in question, and is held to require a species of evidence that cannot be put to a satisfactory test. Under legal expositions of such doctrines, lunatics may be carried from the asylum to the jail, and, almost idiots, swing on the gallows. Not only may such things be, but such things have been, and within recent memory. No degree of imbecility, unless a supreme degree, will annul guilt and avert punishment; for if there be any glimmering of reason, the imbecile is not an idiot in the legal acceptation, but capable of distinguishing good from evil, and thereby accountable to the law. The best evidence of insanity may be rejected in conformity with the above dicta, for doubtless, in some instances, the fullest proof of an insane mind at the time will lie in that inadmissible particular, the character of the crime itself. To adduce such testimony would be deemed equivalent to taking for granted that which it is an object of the trial to determine; but, if it is also an aim of the inquiry to come to a just decision, there are cases in which the aspect of the criminal circumstance must be permitted to have influence.

In medical opinion, insanity results from the action of certain pathological agents, so limiting the faculties of the mind as to render, in some matters, the influence of judgment unseen, and right conduct impracticable by independent personal effort. Incompetency in the affairs of life is one manifestation of the condition, loss of voluntary control another; but perhaps neither feature is a complete portrait of any one case. It is the accident of external circumstance that educes either characteristic the more prominently, and leads to the mistake of accepting a merely comparative

soundness of the other for perfect integrity. For full government of the conduct, all the faculties of the mind are required; and when it is clear that the judgment has suffered to the extent of producing civil incompetency, we must admit that one important guide of the will is enfeebled, and a presumption of irresponsibility already obtained. We, who have daily opportunities for knowing that no vital organ of the animal body suffers alone, may be prepared to give more than due weight to seeming indications of a corresponding analogy in the psychical economy; but when we consider how judgment hangs on memory, memory on attention, and attention on a power of will, we cannot but in the very fact of received delusion recognize a defect of the faculty which gives personality to our actions.

Those members of the legal profession whom official position has afforded opportunities of becoming acquainted with the nature of insanity, will in general be found to have formed views on the subject more in accordance with medical opinion than those which are ordinarily expressed by their brethren. Were those lawyers to whose duty it may fall to address juries in such cases, to devote a share of their attention to the study of mental disease, either by reading, attending lectures, or, best of all, by visiting the insane themselves, there would be less opportunity to remark a difference between medical and legal opinions.

The most generally recognized division of insanity is into general and partial.

General insanity is unsoundness of the whole mind,—the powers of intellect, emotion, and will, being all involved in a greater or less degree; so that the individual may neither think, feel, nor act, as an intelligent and responsible being in regard to any matters. Such a state may be the result of three different conditions of the mind, and its phenomena will vary with its origin. It may be the natural consequence of the *non-development* of mental faculties to the healthy human standard, in which case thought will be feeble, the expression of emotion imperfect, and conduct foolish. It may result, in a mind of natural capacity, from *disorder* of the mental faculties, occasioned by a loss of voluntary control over the succession of thoughts; and then, thinking will be delirium; language, incoherence; and action, extravagance. Lastly, general insanity may be the accidental consequence of *obliteration* of certain faculties by disease, in which case the phenomena will be a mixture of those of non-development and disorder. There are, accordingly, three distinct forms of general insanity; and to these the terms *amentia*, *mania*, and *dementia*, have been applied.

Amentia is congenital general defect of mind, intellectual, emotional, and impulsive. Under this term we comprehend idiocy, and all the degrees of natural imbecility that may be included in the general term insanity. In its highest degree the mental feebleness is found to be associated with imperfect physical development; and the whole phenomena are, by Feuchtersleben, referred to a depressed cerebral, spinal, and even ganglionic vitality.

Mania is that form of insanity characterized by general perversion of the ordinary mental powers of the individual, so that no aspect of his mind is natural. There is loss of voluntary control over thought, emotion, and action, manifested in delirium, extravagance, and motor excitement. Its phenomena have been referred to cerebro-spinal exaltation. *Mania* has been divided into acute, chronic, remittent, intermittent, epileptic, hysterical, and puerperal, according to certain features in its history. The phrase "moral mania," which you will sometimes meet with, does not imply any variety of mania, being employed to indicate, not a species of general, but of partial, insanity.

Dementia is general deficiency of mental power, intellectual, emotional, and impulsive; not congenital, but the consequence of disease or natural decay. It includes all forms of acquired imbecility, and has the features, but not the history, of *amentia*. Its symptoms are those of impaired voluntary control over thought, feeling, and action, manifested in incoherence of language, irrational excitement, and a tendency to spasmodic motions and paralysis—phenomena which may be referred to cerebro-spinal injury. It occurs as a primary form of insanity in persons of uncultivated or weak mind, but much more usually is a secondary disease, and the result of some other form of the disorder. On the table written up before you, you will find the terms acute, primary, chronic, secondary, and senile, employed to designate its varieties.

Partial insanity is a term which would evidently imply unsoundness of certain of the mental faculties, with a natural condition of the rest; and indeed you will find it so defined by most writers; but it is much nearer the truth to say that it is a more limited exhibition of unsoundness, or a less degree of the same state as general insanity. The mind is so far diseased, that there are one or more intellectual or emotional ideas with which it cannot deal as the minds of other men would do. The judgment is constantly at fault on some point, and the will unfree; but though the points are limited in number, the confusion which they occasion in the mind is great.

general, not partial. Partial differs from general insanity, rather in the number of manifested aberrations, than in the number of faculties involved. It also, I believe, may be the result of congenital defect, and accidental obliteration of faculties, as well as a mere manifestation of derangement of powers naturally complete. It may have the character of imbecility or defect, as well as that of confusion or delirium. Two kinds of partial insanity are recognized—viz., monomania and moral insanity, which I shall now briefly define.

Monomania is that form of insanity in which the judgment is evidently incapacitated on certain subjects, yet seems to be sufficiently capable of acting in regard to others. A fixed delusion fetters the attention, overruling the influence of all opposed perceptions, and directing the conduct. The phenomena of the state are referable to a persistent morbid cerebral action. Monomania has been subdivided into a multitude of forms, to which, as you see from the table, various pedantic designations have been applied, intended to be descriptive of the nature of the most prominent diseased manifestations. The best practical division would be, into monomania with depression, in which the delusions are productive of despondency; monomania with exaltation, in which they are such as to elevate the self-esteem of the individual; monomania with alternation, in which the morbid idea sometimes occasions excitement, sometimes depression; and monomania with indifference, in which the craze can neither be said to be of a high nor of a low cast, being of a kind that negatively affects self-estimation.

Moral insanity is that form of the disease which is manifested, not by incapacity of judgment and intellectual aberrations, but by morbid emotions, leading to loss of control and blind or irresistible obedience to disordered impulses. In all cases, though the intellectual power is below the average standard, it is the actions of the individual that are irrational, and not the words; there may be feebleness in thinking, but delirium only in the conduct. The phenomena might be accounted for in the supposition of a depressed cerebral vitality in combination with a state of cerebro-spinal irritation. The varieties of the disorder are many; and it is probable that some of them are congenital defects. I would distinguish two kinds of moral insanity—viz., diseased passion, and insane impulse. Of the former, erotomania and nostalgia may be taken as examples, and the latter would include the propensities to suicide, homicide, theft, incendiarism, drunkenness, and unnatural crimes, where such can be characterized as diseased. Feuchtersleben

seems to hold that insanity of impulse is just a variety of mania (*mania sine delirio*), in which obscure feelings in the nerves of vegetative life—the ganglionic system—acquire such an intensity as to infringe on the motor nervous sphere, and thereby occasion delirious involuntary action.

You will find mention in medical writers of such forms of insanity as fatuity, incoherence, melancholy, brutality, &c.: these are not additional varieties, but merely terms used in place of the foregoing, better employed to characterize symptoms than to construct a classification. There are just five leading forms of insanity—*amentia*, *mania*, *dementia*, *monomania*, and *moral insanity*; these form distinct natural groups, though neither absolutely separated nor harshly outlined. From what I stated to you of the relation of partial to general insanity, you will judge that cases of a composite order may be of occasional occurrence. The acquired forms, by which you are to understand all, with the exception of *amentia*, not unfrequently merge into each other; and instances from time to time appear, of a description so intermediate as to be with difficulty classified. There is sometimes an alternation of one form with another; a case of *monomania*, for example, assuming, during a casual paroxysm of excitement, the features of *mania* or *dementia*. The progress of the disease not unfrequently exhibits, in a single individual, every description of disordered mind. The patient is at first morally insane; then he adopts some particular delusion, and is evidently a *monomaniac*; delusions multiply, excitement supervenes, the mind acts sanely on no occasion, and his whole aspect is maniacal; finally, this violent cerebral disturbance produces irrecoverable mischief in the brain; the excitement abates, or assumes a chronic type, but the powers of the intellect are as much or more in abeyance than ever, and he falls into the state which is termed *dementia*—the natural termination of every state of disordered mind which resists the influence of remedies. In the most ordinary mode in which insanity is developed, moral disorder is the first indication of the disease. The patient labours under emotional perversion—is, as it were, morally insane before he manifests intellectual derangement; and the disease not unfrequently disappears by retracing the same steps. The patient has not completely recovered, however unencumbered the imagination and unfettered the judgment, so long as the affections, desires, and sentiments, have not regained their former character and tendencies. Until the cure is thus completed, though the individual should be reckoned sane enough to be competent to his own management, and be libe-

rated accordingly, no one who has had experience of lunatics will regard him as sound enough to be warranted and trusted. Such an one will either acquire additional mental firmness, or be certain to relapse into his former state of disorder, upon the slightest exciting cause, or without the influence of any apparent agency whatever. He will not remain stationary, an example of a man with a new moral character—a second edition, but not improved.

I shall not weary your attention with a separate description of each of the above forms of lunacy. I have already given you certain specific distinctions, and shall now portray the generic features of insanity,—the features which distinguish it from other conditions, and which will be most conveniently taken up, with certain other particulars that require notice, in a *general description of the insane state*.

Diseases of the mind are most common in highly civilized communities, and in countries which have acquired the highest degree of political and religious freedom.

They are also, upon the whole, more common in the female than in the male sex,—a fact which is thought, however, to be in some degree affected by station in society. In the higher ranks, according to some observations which have been made both in Britain and in France, lunacy prevails more among males than amongst females. The more sensitive character of the nervous system in females, and their less perfect mental and bodily training, would lead us to expect a great preponderance of them amongst the insane, as there no doubt would be, were it not that the greater prevalence of intemperance and vice amongst men serves to bring up the proportion on the other side nearly to an equality.

As to *age*: insanity is not a disease of early life, nor is it so liable to occur in declining years as when both mind and body are at maturity. The most common period of life for the appearance of insanity is between 30 and 40; rather earlier in the female than in the male sex. About three-fourths of all cases occur between the ages of 20 and 50. Below puberty, insanity is almost unknown, unless in the forms of congenital and accidental imbecility; by which last term I mean imbecility of mind, the consequence of convulsions, fever, or accidental injuries of the head.

The physical *temperament* which shows the greatest tendency to mental derangement is the nervous; the temperament which is least liable is the sanguine. As far as observation has led me to any conclusion on this point, I would say that the greater the admixture of the nervous and

the lymphatic temperaments, the greater is the tendency to madness.

All writers concur in representing the married state as more exempted from the danger of lunacy than the state of *celibacy* is,—a statement verified by numerical returns, and still farther strengthened when it is considered that the mean age at which insanity commences is reckoned at 34.* If, then, we were to imitate the phraseology of certain statisticians who speak of “the average man,” we should say that the average lunatic is a female about 35 years of age, and of a lymphatic-nervous temperament.

The moral condition of lunatics.—Most of the premonitory symptoms of insanity are evidentiary of a change in the moral constitution of the individual. The best warning is given by a metamorphosis of the natural character, of a permanent and remarkable description. The subject of this alteration has, as it were, turned his back upon himself, and becomes the opposite of what he formerly was. A diffident and retiring man will be found making some extraordinary exhibition of himself at a public meeting, to the amazement of all his acquaintances; or, perchance, he takes to writing and publishing productions in which he makes manifest to the whole world the symptoms of approaching disorder. I have known the *cacothetes scribendi* to be a premonitory symptom of madness in many instances; private matters and domestic occurrences being laid before the public in newspaper advertisements, placards, and hand-bills; nor is it an unusual discovery to find an insanely-poetical manuscript in the repositories of one who has just become lunatic. A man whose natural disposition it was to be very timid and cautious in the prosecution of his business affairs, will, in the moral disorder, which gives the first warning of diseased understanding, be found plunging into all manner of rash speculations and hazardous stocks, or even turning projector of the most bold and startling schemes. Changes of profession, pursuits, and religious persuasion, are other modes in which the alteration of disposition and temper manifests itself. A sudden passion

* In upwards of 1000 cases of insanity occurring in this quarter of the country,

{ 48 per cent were males.			
{ 53	"	"	females.
{ 75	"	"	between 20 and 50 years of age.
{ 51	"	"	had the nervous temperament dominant.
{ 27	"	"	bilious " "
{ 5	"	"	sanguine " "
{ 17	"	"	temerarious " "
{ 29	"	"	—

for intoxicating liquors, contrary to all former habit, is also not unfrequent evidence of threatening insanity. In short, the sober man often becomes intemperate; the prudent, rash and extravagant; and the sedate and reserved, noisy and wild,—dispositions being acquired which seem opposed to the natural character. Changes, even in a desirable direction, if sudden, unaccountable, and extreme, are equally suspicious. I have known in such circumstances the intemperate to become, not merely temperate, but abstinent; the dull and heavy to seem active and intelligent; the careless, pious; and the timid, courageous; each of these spurious amendments foreshadowing the mental eclipse. Such reversals as described, occurring in those hereditarily predisposed, you may consider as indicative of explosion; and if you act opportunely as a physician, you will lessen the probability of your being required as a medical jurist.

Though a change of this description is the usual precedent of the intellectual disorder, still the moral aspect of the unsound mind is not always of the antipodean character. When insanity does occur without this sort of forewarning, the emotional features of the disease are then as likely to be a caricature of the natural outline of the patient's disposition. Passions which had stamped the mind in the state of health become dominant in a state of disease in which the voluntary power over their exhibition is impaired or suspended,—constantly diminished. The naturally ambitious man, who in his heart worshipped wealth and station, stands forth confessed and bare; he becomes a king, or at least the possessor of enormous wealth. The vain female, whose chief objects in life were dress and adulation, becomes a queen, and is alchemist enough to convert any paltry convocation of rags and straw into a crown. When insanity is marked by the predominance of some one passion, it is more broadly displayed than it ever is in a sane being; and the natural language of the emotion is such as no caricaturist could exaggerate. Such moods are, however, not generic, but special phenomena, and are seen in certain varieties of monomania. Our business at present is rather with those feelings which affect the moral portrait of every lunatic, and of these one of the best marked is selfishness.

Selfishness is one of the most prominent features of the unsound mind. A great many moral defects are comprehended in this term. It implies the decline or destruction of the best part of our social nature, the loss of the sympathetic and benevolent emotions, and, in consequence, a less control over much that is ugly and hateful in humanity. Such feelings seem

to be a finer part of the moral constitution, for they are ordinarily the first to suffer from the approaching malady, and the last to be perfectly re-established. "In many persons thus situated," says Dr. Thomas Mayo, "in whom, at the time at which the inquiry is made, the reasoning powers are in a clear and sound state, and whose emotions are flowing easily and quietly, we are enabled to detect disease by observing that the patient is not going along or sympathizing with us, or, indeed, with any one extrinsic to himself. Perhaps his conversation can only be characterized as morbid by its remarkable want of relation to all that is passing in the minds of bystanders. In this point the insane are remarkably contrasted with the inebriated, whom in many points they greatly resemble. Persons in the latter state overflow with sympathy, and carry it to a ridiculous extent."

We usually find that alteration or disappearance of the natural affections has been one of the earliest symptoms that attracted attention. In a lunatic you will generally observe that the ties of consanguinity are snapped, and that no interest is manifested in the welfare of friends; the mother may have forgotten her child, and the wife her husband. It is well if the loss of affection have assumed no other than a passive character, for it very frequently happens that they to whom they most owe affection are those whom the insane visit with their hatred and their violence. To them their fancied ills are attributed; but even when their relatives are not mixed up with their delusions, they are usually treated with aversion or with apathy. Not only are the natural affections lost in the overwhelming tyranny of selfishness in the insane mind, but so also are those social feelings which unite men to each other. Kindness, compassion, friendship, and sympathy, are not amongst the virtues of the insane. In an asylum you will not find much association among the inmates. It is no uncommon thing to see in a confined airing-yard twenty or thirty people walking about, and no two keeping company together; and in a day-room of lunatics there is very little conversation. Each one is willing enough to speak of his own delusion, or to discuss the particular injustice of his own imprisonment, but he has neither ear nor heart for the complaints of his fellows. They are under circumstances which might be expected to bind men strongly to each other, but they are not so bound. They are under circumstances which the multitude of them deem to be the consequences of unheard-of injustice and oppression; and with this powerful incentive to social action they are directed, controlled, and superintended by a very few, without ever attempting to re-

gain the freedom which they all desire so much by any effort of co-operation. They do not combine or conspire together, not from want of intellect, but from the decay of all social and sympathetic feelings. In many hospitals the walls are so low that, though they prevent an unaided person from escaping, they are quite capable of being scaled, if the person making the attempt had only the aid of one sympathizing individual; yet the insane never make their escape in this manner. The lack of the benevolent affections miserably curtails the happiness of life; and since so many of the insane have, from this moral deficiency, no active sensation of happiness, we cannot wonder that a propensity to self-destruction is so common amongst them.

CONCLUSIONS RESPECTING THE SEAT AND NATURE OF ANGINA PECTORIS. BY DR. KNEELAND, U.S.

DR. KNEELAND draws the following conclusions from his researches into the nature and causes of angina pectoris:—

1. From the symptoms and morbid appearances, angina pectoris is not a disease of the lungs, heart and its vessels, or stomach, but an affection of the nerves supplying these organs.
2. Anatomy, physiology, and pathology would lead us to place the seat of angina pectoris in the par vagum, and not in the sympathetic system of nerves.
3. Like other nerves, the par vagum may be affected with neuralgia and rheumatism, with inflammation; it may be compressed by morbid growths; its spinal origin may be compromised by hemorrhage, accidental wounds, and various irritations—all of which may cause the symptoms of angina pectoris.
4. Angina pectoris and asthma are intimately related: the former being an affection more especially of the sensitive filaments of the par vagum; and the latter an affection of its motor filaments. Both are generally more or less combined in the same case.
5. Angina pectoris is a disease not necessarily fatal, especially in young persons, if accurately diagnosed, and properly treated.
6. In addition to the remedies of the books, special attention should be given to the inhalation of oxygen, and to the use of electricity.
7. In cases of angina pectoris, attention should be directed to the examination of the par vagum, from its origin to its terminations, which, doubtless, on careful examination, will exhibit lesions sufficient to account for a fatal result.—*American Journal of Medical Sciences*, Jan. 1850.

Original Communications.

ON INFLAMMATION AND ABSCESS OF THE UTERINE APPENDAGES.

BY MATTHEW JENNETTE, M.R.C.S.

Surgeon to Birkenhead Hospital.

HAVING met with, in the course of my practice, several cases of pelvic abscess in the female, more particularly after parturition, it may not be useless to direct the attention of the profession to the subject. I am the more induced so to do, as I find that in our British works on midwifery and post-partum diseases it is generally lost sight of, or else very slightly alluded to. It is one, however, of great importance, calculated to give the practitioner much chagrin and uneasiness, requiring nice tact in the diagnosis, and great attention as regards its treatment. Although our systematic writers have been comparatively silent on the subject, several very valuable papers have of late years been published upon it in the medical journals; as, for instance, by Dr. Doherty in the 22nd vol. of the "Dublin Journal;" Dr. Churchill in the 24th vol. of the same Journal; Dr. Lever in "Guy's Hospital Reports" for 1844; Bell, "MEDICAL GAZETTE," 1846; Battersby in the "Dublin Quarterly," 1847; and Dr. H. Bennet in the "Lancet," 1848; besides several cases scattered through the medical literature of the day: nevertheless, it is a subject which I fear is still very much overlooked, and one that is not as generally known in this country as its importance deserves.

While the disease in question has been comparatively so neglected or unknown to us, it is evident from the writings of Paulus Ægineta and others that pelvic inflammation and abscess in the female, its symptoms and sequelæ, as well as its treatment, were known to the ancients. French pathologists in the 17th and 18th centuries also directed their attention very much to the subject; the earlier writers among them, however, considering the purulent abscesses as of the womb, and more recently, from Puzos to Petit, adopting the fanciful theory of metastatic depôts of milk to account for their existence: within the last 30 years, however, more correct

views have been adopted by our Gallic neighbours, and much and valuable have been the amount of their writings and research on the subject,—among others by Dupuytren, Velpeau, Grisolle, and Marchal de Calvi. For more particulars as regards the historical development of our knowledge on the subject, by the French, I refer to the valuable papers of Drs. Battersby and Bennet, referred to above.

Notwithstanding all that has been done to elucidate the subject even by the French, there is still much field for investigation, which Dr. Bennet attributes with much truth to the fact of its being almost altogether studied with reference to the puerperal state, although pelvic phlegmonous inflammation is far from being uncommon apart from that condition; yet as it is generally the case that in the former it appears in its severer form, the milder type of the latter state is overlooked. We know, for example, that puerperal metritis is of a type much more intense than inflammation of the womb unconnected with pregnancy—a fact explained by Marchal thus. The process of pregnancy induces (so to speak) a fibrinous diathesis, the fœtus requires a constant supply of fibrine; and the female system in consequence of this demand acquires a singular tendency to the secretion of this formative element; but it is unfortunately at the same time the essential material of inflammatory action, and thus it is, that the very condition that gives the maternal system the power of supporting the life of the young being, is the prolific source of many ills and sufferings; thus, although the *pus en nature* is never existent in the blood, and therefore can never be transported by metastasis to any local seat of inflammatory action, the elements of the fœtus are present in it during the progress of foetal development. Keeping this idea in view, we may be inclined to regard with more favour than is fashionable now-a-days, the old theories about “milky deposits,” “metastatic collections,” &c. Another evil consequence has resulted from the overlooking the non-puerperal form of the disease; extreme cases only being in general recorded, which has among others misled the otherwise accurate Marchal de Calvi, in giving it as his conviction that the non-puerperal abscess is generally fatal, and that it as often points by the abdominal walls as by the bowel

or vagina, which is decidedly erroneous.

To understand the seat of the disease properly, it is necessary to recollect how the peritoneum, in passing from the posterior wall of the bladder on to the uterus, and thence to the rectum, forms two large folds (the lateral ligaments) containing within them the Fallopian tubes, ovaries, and round ligaments, as well as a large quantity of areolar tissue connected, although in a great measure distinct from the extra-peritoneal cellular tissue of the pelvis. Thus in the cavity of the pelvis immediately in contact with the uterus, between the peritoneal folds, but external to them, and connected with the bladder and rectum, we have an abundant supply of that tissue which of all others is most prone to inflammation; together with other important organs, more or less disposed thereto. Hence the pathological understanding of pelvic inflammation of this part is so easily acquired: it is strange the subject was not long since fully developed by our systematic writers, particularly of the midwifery class. The inflammation may of course attack any one of the uterine appendages in the first instance, but generally in all probability the loose cellular tissue in which they are lodged; it then may extend to the others in succession; the peritoneum, however, in non-puerperal inflammation, seldom is affected, whilst in the post-partum variety that membrane is particularly liable to inflammation.

Causes.—In the non-puerperal state the disease may be produced by anything calculated to increase the vascularity of the part, or arrest the functions of the uterine system; chronic inflammation of the uterus, or direct injuries (well illustrated by a case of Dr. Bennet in the “Lancet” for 1848) but more especially by the arrest of menstruation—a good example of which is a case by Mr. Bell, in Vol. II of the “Medical Gazette,” New Series. The menses being arrested by exposure to cold, an abscess was formed, which ultimately discharged through the vagina. The case was, previously to Mr. Bell seeing it, attended by three medical men, who successively treated it for strangulated hernia, continued fever, and inflammation of the bowels. These mistakes, I suspect, are far from being uncommon, and hence many cases of non-puerperal phlegmon of the uterine appendages are treated for,

or considered to be something else,—as neuralgia, hysteria, uterine irritation, or rheumatism, and many examples from respectable sources confirmatory of this suspicion could be deduced; as a case from Mr. Taylor, on rheumatism of the uterus, which I abridge. Mrs. H.—aged 24, had a miscarriage during her second month: eight days after she exposed herself to a privy: next day she had great bearing-down pains, frequent desire to pass water, which she did with excessive pain, and very little at a time; great pain above the symphysis pubis, the pressure of the hand insupportable, fever, no discharge from vagina, uterus hot and tender, could not lie but on the back, &c. The treatment was antiphlogistic, and she gradually recovered; a profuse *fluor albus* appearing at the same time. Two of the cases recorded by Churchill are non-puerperal, as well as one of the eight narrated by Mr. Bell; in the sequel I will give examples of the same variety. Various circumstances connected with parturition may predispose the parts concerned to become affected by this disease; as impatient interference, resulting perhaps from want of sufficient confidence in the efforts of nature, premature or too long delayed mechanical aid, turning, removing with the hand in the uterus the placenta, exposure to cold, falls, blows, &c.: it may follow an attack of acute inflammation, or be a sequent of puerperal fever, as is illustrated by one of Lever's cases. Dr. Copland, in his admirable article on puerperal diseases, in his Dictionary (in which, by the way, the disease I am treating of is all but unnoticed) introduces some remarks relative to the modern use of anæsthetic agents in midwifery, which I think is equally applicable here; he considers their use as not altogether innocent; immediate fatal results may be prevented, but can we so prevent other consequences? Anæsthetic agents have been demonstratively shown not only to produce insensibility, but also congestion of the lungs, and subsequent death in a short period; witness the case of the late Dr. Anderson of this town (vide "Lancet," Vol. ii. p. 99, 1848). In nine out of ten, or nineteen out of twenty cases, no unpleasant result may take place, but the tenth or twentieth may be a serious puerperal disease; it may be the one we are writing about.

Symptoms.—I will describe those of the puerperal variety, the non-puerperal

being (child-bed apart) of the same nature, but generally milder in intensity, more chronic in character, and the abscess seldom opening through the abdominal parietes, but generally through the vagina or rectum; hence it is so often overlooked, the discharge through the vagina being looked upon as a copious discharge of whites, and that with the fæces being still less liable to be observed. The symptoms may commence two or three days after delivery, or not for several weeks, generally with a rigor, or a succession of shiverings; there is more or less fever, occasionally remitting; pulse varying from 90 to 110; pain in the abdomen; the patient generally feels better in the morning, with increase of fever in the evening; the uneasiness and pain increase as the disease advances; sometimes there are pains resembling sciatica in one or both legs; painful micturition, and tenesmus occasionally of a most distressing character; the secretion of milk generally is suppressed, the lochia sometimes; tongue is coated; the patient becomes emaciated, is subject to hysterical paroxysms, frequently vomits, and sweats profusely on falling to sleep. These symptoms, inasmuch as the formation of pus in phlegmon of the iliac fossa is remarkably slow, may continue for a considerable time if overlooked, or not relieved, until the matter is evacuated either by a natural or artificial opening. On examining the abdomen, it will be found, that although the patient complains of pain all over it, pressure over the pelvic portion aggravates it, while elsewhere it relieves it; showing that the general pain is neuralgic, whilst the local proceeds from inflammation. In one or other iliac fossa will generally be detected by the touch a tumor with a brawny hardness, but not always, as in one of my cases which is hereinafter described: but in every case it may be detected, and the true nature of the disease known, by *vaginal* or *rectal* digital examination. To examine by the former outlet, it is advisable that the patient lie on her back, with her legs drawn up, when, on carefully introducing the forefinger of the right hand, while with the left hand you press over the iliac fossæ, you can easily detect the tumor, and even, if supuration has not yet taken place, the roof of the vagina will be found, as Dr. Doherty so well expresses it, "as firm and inelastic as a deal board"—a very

important evidence, as, when we know the true nature of the case, we may hope by appropriate remedies to cause resolution, which several recorded cases show to be quite possible; as, for instance, the case recorded by Doherty himself, and others in Dr. Churchill's paper. If further evidence is required, examination by the rectum in a similar way, the patient however being placed on her side, will afford it. It has been stated by some writers, Lever for example, that the disease is more frequent on the left side than on the right; but, as far as I can find by reference to published cases, I think it is equally liable to attack either: thus, out of forty-one cases, nineteen were on the right, nineteen on the left, and three on both sides.

The terminations of the disease may be, 1st, resolution, which is of course the most favourable, and one which we may reasonably expect to induce, provided we ascertain the nature of the disease before it becomes too far advanced: hence, examination with the finger in the vagina or rectum is so important, and also the great error of attributing too hastily and without careful examination, painful affections of the iliac fossa or abdomen after parturition, to neuralgia, irritable uterus, &c. Puzos, Marchal, Churchill, and others, relate several cases where this happy result took place. 2dly, suppuration. When either through neglect or the inefficiency of the remedies employed, resolution does not occur, an abscess is formed, the purulent matter of which is said sometimes to be absorbed, but more generally it finds its way either externally through the abdominal walls covering the tumor, into the peritoneum, after giving rise to fatal peritonitis, into the vagina, bladder, rectum, or uterus, and sometimes through the femoral ring or thyroid foramen. Among 37 cases in which the matter escaped, I find 11 opened through the vagina, 9 by the abdominal walls, 5 by the bladder, 3 by the inguinal ring, 3 by the rectum, 3 into the peritoneum, 1 through both vagina and rectum, 1 into the uterus, and 1 through the thyroid foramen. These proportions cannot be considered conclusive, inasmuch as the number of cases are too few to decide. 3rdly, the intensity of the disease, or the secondary affections induced by it, may prove fatal after an indefinite time.

Diagnosis.—The vaginal examination is in itself sufficient to distinguish this affection from any other: however, inasmuch as this will not always be permitted, and the disease has been from time to time mistaken for others, it is right to study its peculiarities; particularly as, when it occurs independently of child-birth, or a considerable time after, it may not be so reasonably expected. Abscess of the abdominal parietes may be distinguished from it, by causing the patient to lean on her knees and elbows, when the tumor will be found to move with the parietes, while in iliac abscess you can move them over it. The pressure of the tumor on the nerves may cause pains resembling sciatica, but a careful examination, together with the history of the case, ought to be sufficient to show the nature of it. In like manner may be distinguished lumbar abscess, pregnancy, &c. From peritonitis it is easily distinguished by the neuralgic pain and several of the symptoms of peritonitis being absent: however, peritonitis may occur as a consequence of the disease in its advanced stage; either on account of the abscess bursting into the peritoneal cavity, or in consequence of the extension of the inflammation from the uterine peritoneum. But this only occurs when the disease has existed for some time, and is easily known by the aggravation of the pains and fever. I have known it to be mistaken for hernia, and considerable pressure used for the reduction of the supposed rupture with apparent effect, owing perhaps to the mobility of the parts within the abdomen: but it is easy to imagine how such a proceeding might cause rupture of the abscess, and extravasation of its contents into the peritoneum. I will give in the sequel a case of M. Dupuytren, where it possibly might be mistaken for aneurism.

Prognosis should be guarded, inasmuch as peritonitis sometimes occurs from the matter finding its way into the abdominal cavity: besides, when it discharges through the vagina or elsewhere, fistulous openings may remain, and the flow last for a time long enough to cause the patient to sink; and it also sometimes happens that the disease returns when the uterine functions are again called into play. Mr. Bell found, in 93 recorded cases, as

many as 22 deaths, which I think is more than the average mortality.

As to the sequelæ, I think it not improbable that future miscarriages may be induced, but in one of my cases the individual has had several children since, all at their full time, and has had no return of the disease; of the other consequences mentioned by writers I cannot speak.

Treatment.—The indications are two fold. To produce resolution, if possible; if not, suppuration and evacuation of the matter as soon as possible. Several published cases prove that resolution will often occur under proper treatment; we must remember at the same time that patients with this disease will seldom bear heroic remedies, as general bleeding, &c.: a dozen leeches, applied two or three times if necessary to the seat of pain, or a smaller number to the vagina, by means of the speculum, poultices, fomentations, injections of warm water into the vagina and rectum, and mercury so as slightly to touch the gums, may succeed in arresting the progress of the inflammation: the secretions must at the same time be attended to, not inducing, however, much purging, which gives pain: light bland food is to be given, opium may be necessary sometimes to soothe the pain, and in the form of enema or suppository is very useful for relieving the tenesmus, which is often very troublesome. If we do not succeed in arresting the inflammation, and that it is evident suppuration has taken place, we must endeavour to promote the formation of pus and its discharge. Poultices and fomentations are to be persevered in, and the abscess opened as soon as possible; this may be done either through the abdominal walls, through the vagina, by means of a speculum and guarded lancet, or through the rectum by means of a trocar and canula; and it will be right to select for operation that part to which, in the individual case, the matter is nearest, and the abscess most closely connected with: for instance, in the two cases in which I opened the abscess in the hypogastria, the matter was evidently pointing in that direction, but I think the vagina or rectum would be most eligible if the matter pointed in either of those directions. If there be no adhesion between the tumor and the abdominal parietes, I think the adop-

tion of Graves's plan, as recommended by Churchill, or that of M. Martin, to excite adhesive inflammation, objectionable, as, besides other reasons, such a proceeding would occasion delay, the matter possibly in the meantime finding its way into the peritoneal sac, and so inducing perhaps fatal inflammation. After the matter is discharged, of course the patient's strength must be kept up with generous food, wine, &c.; and, should any hardness remain, small doses of potass. hydriod. in decoct. sarsap. &c. should be administered.

CASE I.—Mrs. J —, aged 24 years, confined of her second child, 4th June, 1844: labour in every respect natural, and of short duration: after a few days she was able to resume her household duties, which she did without sufficient care. On 23rd, being warm from over-exertion, she exposed herself to a current of cold air; after which she had several attacks of shivering: the milk diminished in quantity, pain in the lower part of the abdomen, shooting down the left leg began to be felt; fever, with nightly exacerbations, set in. These symptoms were borne for some time, and I was not sent for until the pain assumed great intensity. I saw her July 1st, when she was suffering very much from pain in the left iliac fossa, with her legs drawn up: pulse about 110. I could detect no tumor in the iliac fossa then: indeed, she would not allow me to make sufficient pressure to ascertain the fact. I easily ascertained by vaginal examination the nature of the disease, and ordered a constant succession of poultices after leeching, aperients, and opiates to soothe the pain. The symptoms increased in intensity, when in a few days a brawny hardness was perceived in the iliac fossa. Poultices matured the abscess, which I opened through the abdominal walls on the 24th July; it freely discharged, and the opening healed in a few days: her recovery was slow but permanent: the child, however, had to be weaned, she never recovering her supply of milk. She has had three healthy children since, and no return of the disease.

CASE II.—Hannah Phillips, aged 36, admitted a patient of the Birkenhead Hospital and Dispensary, May 31, 1850. Says she was delivered of her sixth child five weeks before, by a midwife: that she had suffered very much at the time, particularly in her right flank, which

part gave her great pain for about five weeks previous to her confinement, which she attributed to over-straining herself: she states that the woman who attended her said that the after-birth "stuck fast," and that she did something, which gave her great pain, to remove it: believes she passed her hand into her; but her sufferings were so great, she is not sure of that. She now complains of great pain in the lower part of the belly, particularly on the right side; can only lie on her back, with her legs drawn up: she is worn and emaciated, with hardly any milk; has a constant desire to sit on the commode, with a feeling as if there is something to come away, although her bowels are frequently moved, but with great pain. When she falls asleep, she is bathed in a profuse perspiration; pulse about 100, soft and small. On examination externally, there was not, as in the last case, any hardness to be felt, but only an indistinct evidence of a deep-seated fulness, but not sufficient *per se* to indicate an abscess, which, however, was fully recognised by examination by the rectum, where the cause of the tenesmus was found to be a fluctuating phlegmon. Fomentations and poultices were applied; mild restoratives and opiates ordered: and on the 8th of June, while sitting in the night-chair, a large quantity of pus came away through the rectum; after which she experienced great relief. Light nourishing food and tonic medicines were prescribed; under which treatment she continues to improve in health and strength, being, however, unable to afford her child its natural nourishment.

CASE III.—March 27th, 1850. Attended Mrs. M—, a stout, strong woman, about 33 years, living some distance from here, in labour of her fifth child, which was, in every respect, natural and quick, requiring no interference on the part of the accoucheur. She was subsequently attended by a medical gentleman of her neighbourhood. I was sent for to see her on the 7th of June, when I found her much altered in appearance, thinner, complaining greatly of a severely painful lump in the right iliac region, which, at the commencement of uneasiness there, which was about a fortnight after her confinement, was treated by leeches, for inflammation of the iliac veins, and which latterly had been supposed to be

hernia; she being directed to procure a truss. The constitutional symptoms, though of a much milder form, were the same as in the last case: neither was there the same amount of tenesmus or difficulty of making water. I found the same brawny hardness as in the first case, and ordered her effervescing saline draughts, and repeated hot poultices of linseed meal, which, in a few days, forwarded the abscess so as to enable me to open it, which I did, giving exit to several ounces of healthy pus, which gave great ease; after which, with proper nourishment, she quickly convalesced.

(The two following cases of Professor Dupuytren are abridged from the *Journal Hebdomadaire de Médecine*, Tome 6.)

CASE IV.—(Non-puerperal). A young, well-formed woman was affected for some time with a malady having its seat in the pelvis, for which she could assign no cause: she had severe pains in the neighbourhood of the bladder and rectum, and in the groins. A physician she consulted considered her suffering from diseased uterus; but on admission into the *Hôtel Dieu*, in January 1829, and examining with the finger in the vagina and rectum as high as possible, and strongly pressing down with the other hand on the epigastrium, a painful tumor was perceived. She was submitted to antiphlogistic treatment, hot baths, cataplasms, &c.: after a few days the pain suddenly disappeared, without at first being able to account for this quick cessation: however, it was soon apparent that the urine contained a large quantity of pus, the emission of which was without pain or inconvenience. The discharge had not quite ceased when she left the Hospital. Dupuytren introduces this case with some interesting remarks on the manner in which abscesses open into hollow organs, which he shows to be valvular, as that of the ureter into the bladder, not allowing of the passage of the contents of the organ into the abscess, but permitting of the latter closing on being emptied of its contents.

CASE V.—A woman, two months confined, aged 36, was admitted into the *Hôtel Dieu*, November 2, 1829. She complained of pain in the right iliac fossa, where was a considerable swelling: no fluctuation could be detected: she could not extend the limb without great pain. Antiphlogistic treatment was di-

ected. After fifteen days, Dupuytren perceived deep-seated fluctuation. Mons. Capuron and others could not do so, which caused Mons. Dupuytren to delay the opening which he intended to make. Some days passed, during which a phenomenon supervened which tended to weaken the diagnosis of Dupuytren; the tumor became less painful and voluminous, at the same time, no discharge of pus taking place, either by stool, vagina, or bladder: however, soon the tumor resumed its former volume, and fluctuation became evident: it was now resolved to operate. M. Dupuytren made a small incision with a straight bistoury, below the crural arch, outside the femoral artery; a small quantity of pus escaped by the edges of the instrument, on withdrawing which, and introducing a female catheter into the abscess, an enormous quantity of matter was discharged, with great comfort to the patient. The matter continued to be discharged, the catheter requiring to be once more introduced, when some obstacle opposed its free exit: by degrees the discharge lessened, and she was discharged, cured, about the end of December, 1829.

CASE VI.—(Abridged from *Journal Hebdomadaire de Médecine*, Tome 3). A young woman, confined a month, was admitted into the *Hôtel Dieu*, April 7, 1829. She was seized, a few days after her delivery, with severe pains in the right iliac fossa, extending down the thigh, and rendering her unable to walk. A tumor afterwards showed itself in the fossa, extending into the groin through the femoral ring. On examining the tumor, which indicated fluctuation, M. Dupuytren perceived it to be the seat of strong pulsations, corresponding with the pulse, which extended from its inferior part to Poupart's ligament: these pulsations were remarkable along the anterior and middle part of the tumor, and also transversely along the superior and right portion of it. M. Dupuytren attributed these symptoms on the middle of the tumor to the presence of the superior portion of the crural artery; and the transverse beats to that of the circumflex artery, the pus the abscess contained having passed behind the vessels, by means of the lax cellular tissue so abundant in that situation. The next day the pulsations had disappeared; which he attributed to altered position of the arteries, in consequence

of the numerous examinations made by the pupils: however, he plunged a bistoury into the tumor outside and in the angle formed by the union of the crural and circumflex arteries. Some ounces of healthy pus escaped; but, owing to the sinking of the crural arch, which had been raised by the pus, the discharge became arrested. He then introduced a female catheter, by which he emptied the abscess: a mesh was then introduced, and the case progressed, in every way satisfactorily, to a complete cure.

CASE VII.—(*Désormeaux*, *Journal Hebdomadaire de Médecine*, Tome 7). D.—, 24 years, healthy during pregnancy, was delivered, January 13, 1830, after a painful labour of thirty-six hours, of a dead child; the second succeeding day slight pains were experienced in the abdomen, which, by appropriate remedies, were relieved, leaving, however, an uneasiness and feeling of weight about the rectum.

Feb. 1st.—Diarrhoea came on, the defecation being accompanied with much pain: by examination with the finger in the rectum a tumor was discovered to the left side of the gut; examination by the vagina also showed the existence of a tumor: leeches, &c. were prescribed; but the tumor increased more and more. M. Désormeaux was about opening the abscess, when it discharged itself, by the vagina, of a quantity of foetid pus, with great relief; when suddenly, on the 9th of February, very acute peritonitis set in, which soon ended in death.

At the post-mortem, the peritoneal cavity was found to contain a considerable quantity of phlegmonous pus, the stomach and intestines being healthy. The vagina being opened on its left side, was discovered an ulcerated opening, which led into a purulent cavity at the side of the vagina and rectum, the cellular tissue in the neighbourhood being extensively destroyed.

In the *Provincial Journal* for 1844, are several cases recorded by Mr. Edrington, of Birmingham,—one in particular, where abscess of the right iliac fossa recurred after different pregnancies, each time discharging by the bladder, the poor woman having also had very numerous miscarriages. In her case there was pain in the right iliac fossa before marriage: so that it may be considered as one of non-puerperal disease, aggravated by uterine excitement.

Birkenhead, July 2nd, 1830.

AN INQUIRY INTO THE
VIEWS OF CERTAIN MODERN
PHYSIOLOGISTS RELATIVE TO
THE LAWS AND ACTION OF
THE NERVOUS SYSTEM—
CEREBRAL, SPINAL,
AND GANGLIONIC;

AND HOW FAR SUCH VIEWS ARE SUB-
STANTIATED BY THE PHENOMENA
PRESENTED TO OUR OBSER-
VATION IN HEALTH AND
DISEASE.

BY WILLIAM SMITH,

Member of the Association of Medical Officers of
Hospitals for the Insane; formerly Resident
Surgeon in the Lincoln Lunatic Asylum, and
subsequently in the General Hospital at Lin-
coln.

DURING the last five months, whilst preparing the materials for the papers lately published in the *MEDICAL GAZETTE*,—"On the present state of lunacy and lunatic asylums, and on the nature and treatment of mental derangement," my attention has been almost exclusively directed to the physiology and pathology of the nervous system: many interesting phenomena, scarcely, if at all thought of, prior to that investigation, have forcibly and irresistibly rivetted my attention. The members of the Medico-Chirurgical Society of Nottingham, in addition to their urbanity and kindness in allowing me, an utter stranger, and unconnected with their efficient society, to take up their invaluable time, have unwittingly rendered me a service of still greater magnitude: by opposing very strongly the doctrines then propounded by me, they have set me upon a course of deep reflection. Believing most conscientiously that those views were strictly in accordance with the truth, and having had most ample opportunities (in the Lincoln Asylum and elsewhere) of seeing insanity, (and what are usually termed) nervous disorders, upon a large scale, in several different counties of England, I sat myself down to a careful and deliberate investigation of some of our most celebrated authors, in order to collect together into a narrow focus such views as appeared corroborative of, or not greatly dissimilar to my own. Although I had previously, on several occasions, read with much interest the

excellent volumes of Mr Travers, on "Constitutional Irritation," their perusal latterly has interested me more than ever. To the young surgeon just entering upon the gulfs and whirlpools, the *Scylla* and *Charybdis* of surgical practice, they are invaluable. Although no longer to be ranked among the juniors (seeing that my hair is now assuming a silvery tint), I have within the last two months strengthened my arguments most materially by facts and deductions contained in those carefully digested volumes. It would appear that Dr. Calvert Holland, in his *Philosophy of Animated Nature* (a not very favourable and, as I think, rather harsh review of which appeared in the *MEDICAL GAZETTE* not long ago), has merely enlarged upon and carried out in extenso many of the doctrines therein communicated. Mesmerism, however, is evidently a new birth; whether a legitimate offspring or of a surreptitious and spurious race, I leave to the worthy and learned doctor and his readers to decide. The philosophy has taken me a long time to study and digest, &c. There is an abundance of really valuable matter, and numerous experiments by various physiologists, most accurately given, with valuable deductions therefrom. Many of his views, or something vastly like them, are to be found in Dr. Henry Holland's *Medical Notes and Reflections*. Dr. Calvert Holland however, is more ultra in his opinions than either of the authors just named.

During my late investigations into the laws and actions of the nervous system, one thing has particularly struck me, viz. that modern physiologists, in their eager desire to enter minutely into a description of every portion of the nervous system, have carried their love of specializing too far, and thus made the separate parts of one system or unity to have an action altogether independent of, and beyond the cognizance of, the rest. This I believe to be the grand error—the besetting sin, of the present scientific age.

That sagacious and most experienced physician, Dr. Latham, in the preface to his excellent "*Clinical Lectures on Diseases of the Heart*," has observed:—"The study of our times has been to specialize and to localize disease, and it has had very useful results. But it

has had a tendency to narrow our views and to cripple our practice, by setting up as many several pathologies within the body as there are several organs. Yet no sooner do the diseases of different parts come to be treated, than they begin to claim their place in a common pathology. We cannot reach them and apply our remedies directly to them, in the isolated spots wherein we find them; but if they are to be reached, and treated at all, it must be through the vascular system, or through the nervous system, or through the digestive and assimilative system. For these are the common agents of life and increase, both healthy and unhealthy, and the common channels both of food and of medicine."

Now according to my ideas of the present state of knowledge relative to the physiology of the nervous system, the remarks of this accomplished physician may be brought to bear, with equal force, upon the present artificial and (as I think) most unnatural distinctions attempted to be made in the physiology of the nervous system. If we are to resign ourselves blindfold, and with implicit faith and confidence, into the hands of these imaginative gentlemen, who assert that the brain is the special and exclusive domain of the psychical principle; that the spinal cord has its *vis nervosa*, and is the *arbiter elegantiarum*—the high and mighty potentate, whom spasms, convulsions, and other lady-like diseases obey; that the sympathetic or ganglionic system is merely the larder or store-room from whence the various organs, tissues, &c., receive their nourishment, supplies, &c.; that the heart is not dependent in any way upon the nervous influence; that the muscular fibre contracts or displays irritability, not in virtue of any energy or principle derived from the nervous system, but simply from some *vis insita* or property inherent in itself;—if, I say, we are to adopt these views, we shall inevitably require a special pathology for each of these various organs and tissues: and this brings me to the exact argument advanced by Dr. Latham. It may perhaps appear the height of presumption in a medical practitioner (a sort of amphibious animal, like Falstaff's hostess neither fish nor flesh) like myself venturing to oppose the doctrines of learned physiologists; but, as far as my observation goes, these pro-

fessed authors, as a general principle, do not make good practitioners, and, *vice versa*, sound practitioners are seldom guilty of knowing much of the hidden mysteries of physiology. Had all the views of eminent physiologists been founded on facts, and deductions carefully and deliberately drawn therefrom, we should have had fewer discoveries in this department of science, and less theoretical knowledge to unlearn. A learned writer has well remarked, "How wild soever the wanderings, or great the departure from a strict logical procedure, the dreamy conjectures to which they give rise find naturally a home in the bosom of physiology."

Dr. Calvert Holland, in a recent work, "Practical Views of Nervous Diseases," has justly remarked:—"With the prevailing limited knowledge of the nervous system, how could the practitioner see the shortest road to the seat of the evil, even if familiar, which is not the general rule, with the doctrines of the physiologist?" By one school he is told that voluntary and involuntary motions are independent of any power derived directly from the nerves; that in fact they are the result of a *vis insita* in the muscular fibre: by another, he is taught that all these motions probably depend upon a *vis nervosa*, whilst the brain and its wonderful faculties have not the slightest connection with it, or with any nervous influence; the cerebrum being the exclusive domain of the physical principle. This immense organ, with which all parts of the nervous system are associated, from which it receives impressions, and to which it distributes the motive power, is, according to such reasoning, pervaded by no agent which is common to the whole. He is further informed that the functions of the nerves consist in conveying a series of vibratory movements, and that all vital phenomena are explicable on this view; or that the nerves do really transmit a something, but agreeably to what laws is not stated. He is, however, more frequently amused with the ingenuity and eloquence which demonstrate the perfect fallacy of all such opinions. The idea that the nervous system is animated by any power is ridiculed with pointed facetiousness:

"Is it possible, amidst this chaos of physiological doctrines,—this clash-

ing of conclusions on the nature of vital action, for the mind, how anxious soever it may be, to discover the direct road to the seat of disease? Which is the path, according to the received notions? One, groping in the search, acts energetically on the bowels, or thoroughly relieves the stomach of its contents; another detects the evil in the blood, and emits a copious stream; a third, ascertaining the dry state of the skin, determines the fluids in that direction. In none of these modes of treatment, or in others which it is unnecessary to notice, is there a clear and comprehensive view of the powers of life, or indeed a knowledge of the laws according to which they operate. This would point out the degree of efficiency which is exercised by each in the production of the morbid results, on an accurate conception of which all medical treatment must be founded. It would indicate to the practitioner the shortest course to the desired object."

I leave it to any person of reflective mind, conversant with the present state of medical science, and possessed of sufficient candour to speak the whole truth, to decide whether this be not a true and faithful picture of passing events and opinions. But if practitioners themselves are perplexed by such an *embarras de richesses*, as Mr. Travers not inaptly styles it, what is the state of the unfortunate medical student? How shall he escape the "*infames scopulos Acrocerania*," the "*mare turgidum*" of physiology? Verily, we may say with the elegant poet, "*Illi robur et es triplex circa pectus*" are needed for that man who dares to enter on the debateable land of modern physiology.

But let me illustrate my views by a quotation from one of the most eminent authorities of the present day. Dr. Carpenter, in his excellent Manual of Physiology, page 486, remarks:—"We have now considered the entire series of those operations which make up the vegetative or organic life of the animal; including the functions by which the germ is prepared, by which it is nourished until it can be left to its own powers, by which its continued development is effected until the fabric characteristic of the adult has been built up, and by which the normal constitution is maintained through a lengthened period,

so long as the necessary materials are supplied, and no check or hindrance is interposed, by external influences, to that regular sequence of change on which the continuance of its powers depends. In this survey it will have been perceived that the essential parts of these operations are, in animals as in plants, completely independent of the influence of that which constitutes the peculiar endowments of animals; namely, the nervous system.

"a. The reduction of the food in the stomach, by the solvent power of the gastric fluid, is a purely chemical operation, with which the nervous system has nothing whatever to do, excepting that it perhaps accelerates the process, by stimulating the muscular coat of the stomach to that peculiar series of contractions which keeps the contents of the cavity in continual movement, and favours the action of the solvent upon it.

"b. With the process of absorption, by which the nutritive materials, with other substances, are introduced into the vessels, the nervous system has nothing to do; this being a purely vegetative operation, partly dependent upon the simple physical conditions which produce endosmosis, and partly on a process of cell-growth.

"c. The assimilation of the new material, effected, as we have seen reason to believe, by another set of independent cells, can receive but little influence from the nervous system, and is obviously capable of taking place without its aid.

"d. The circulation of the blood, again, though dependent in part upon the impulsive power of a muscular organ, the heart, is not on that account brought into closer dependence upon the nervous system; for we have seen that the contractions of the heart result from its own inherent powers, so as to continue after it has been completely detached from the body; and that the capillary power, which is the chief agent in the movement of the blood in the lower animals, and which exerts an important subsidiary action in the higher, is the result of the exercise of certain affinities between the blood and the surrounding tissues, in which the nervous system can have no immediate concern.

"e. The act of nutrition, in which every tissue draws from the circulating

blood the materials for its own continued growth and development, and by which it incorporates these with its own substance, is but a continuance of the same kind of operation as that which takes place in the early development of the embryo long anteriorly to the first appearance of the nervous system,—namely, a process of cell-development and metamorphosis, which must be, from its very nature, independent of nervous agency.

"f. The same may be said of the secreting operation in general; for this essentially consists of the separation of certain products from the blood by cells situated upon free surfaces; which thus remove those products from the interior of the fabric.

"g. And the interchange of oxygen and carbonic acid which takes place between the atmosphere and the venous blood when brought into mutual relation in the lungs, and which is the essential part of the function of respiration, is an operation of a merely physical character, with which the nervous system can have no direct concern.

"h. Finally, the development of the reproductive germs in the one sex, and of the ova within which these are to be evolved in the other, the subsequent fertilization of the latter by the former, and the changes consequent upon that act, together making up the function of reproduction, may be all regarded as modifications of the ordinary nutritive processes; and are effected, like these, by the inherent powers of the parts concerned in them, at the expense of the materials supplied by the blood, without any direct dependence upon the nervous system."

"841. Still, although the various processes which make up the essential part of the nutritive operations in animals, as in plants, are no more dependent on any peculiar influence derived from a nervous system in the former than they are in the latter, it must be evident, from the details already given, that there must be in animals various accessory changes, which are requisite for the continuance of the former, and which can be only effected by the peculiar powers with which animals are endowed. Thus, to commence with digestion: this preliminary process, which the nature of

the food of the plant renders unnecessary for its maintenance, can only be accomplished by the introduction of the food into a cavity or sac, in which it may be submitted to the action of the solvent fluid. The operation of grasping and swallowing the food, wherever it is performed, is accomplished through the agency of the nervous system; and if it be checked by the loss of nervous power, the digestive process must cease for want of material. So, again, although interchange of gaseous ingredients between the atmosphere and the circulating fluid may take place with sufficient energy in plants and the lower animals, through the mere exposure of the general surface to the atmosphere; yet we find that, in all the higher animals, certain movements are requisite for the continual renewal of the air or water which are in contact with one side of the respiratory surface, and of the blood which is in relation with the other: for the direction of which movements, a nervous system is requisite. In the excretory processes, moreover, the removal of the effete matters from the body can only be accomplished in the higher animals by certain combined movements, the object of which is to take up the products that are separated by the action of the proper secreting cells, and to carry them to the exterior of the body, there to be set free; and these combined movements can only be effected by the agency of the nervous system. Lastly, in the act of reproduction the arrangement of the sexual organs in animals requires that a certain set of movements should be adapted to set free the germ from the body of the male, and to convey it to the ovule of the female; and further, that the ovum should be expelled from the body of the latter in a state of more or less advanced development. For these movements a special arrangement is made in the construction of the nervous system, and in the application of its peculiar powers."

However strongly it may subject me to the charge of presumption in thus daring to advance my views in opposition to those of so eminent an authority as Dr. Carpenter, still I cannot allow such doctrines as those just quoted to pass current without entering my respectful but decided protest against them. Although I may not feel disposed

to adopt in their totality and entirety the novel views of Dr. Calvert Holland, as enunciated in his "Philosophy of Animated Nature," yet I cannot but coincide with him on many important points, and in none more cordially than those wherein he maintains that the blood and nervous energy are indispensable to every vital function or action throughout the human frame. If these views be correct—and numerous facts and striking analogies may be adduced in support of them—what are we to think of the doctrines of Dr. Carpenter, who clearly and distinctly asserts (in his 12th Chapter, pages 486-7, 8), that digestion, absorption, nutrition, the circulation of the blood, secretion, excretion, respiration, and the interesting and most important changes concerned in the process of reproduction, all take place without the nervous system having any immediate concern in the matter? For my own part, I cannot help viewing such doctrines as medical heresies of a grave and serious character, calculated to mislead junior practitioners and the rising generation of medical men, who may now be in *status pupillari*, and therefore very likely to adopt implicitly, and without investigation, any theory propounded by so eminent an authority. As far as my own humble opinion is concerned, all my observation and experience of the human body, both in health and disease, irresistibly lead me to the conclusion, that the nervous system is very materially and intimately concerned in all the functions or actions alluded to by Dr. Carpenter; and, moreover, if I do not greatly err, many experienced and sagacious practitioners might be found, who entertain opinions very similar to my own on this subject. What I would ask, are the uses of that complicated and universally pervading element, the nervous system, which extends its numerous gauze-like filaments, and impenetrable net-work processes, over every minute particle of the human frame, *a capite ad calcem*, if not requisite for the maintainance of every vital function or action necessary to the animal economy? why should the heart, the lungs, the stomach, the uterus, and every other important organ in the body, be plentifully supplied with blood-vessels, and, furthermore, be abundantly furnished with filaments from the three great nervous centres—brain, spinal cord, and sympathetic—if their very existence,

their function (whether special or otherwise), and action, did not depend upon, and derive their origin from, the reciprocal or galvanic action of blood and nerve. Great part of my life has been spent in the discharge of duties connected with large public institutions; for more than fourteen years I have had ample opportunities of observing disease upon a large scale, and during the last four years I have been a hard reader: with the writings of Latham, Watson, Billing, Brodie, Travers, Graves, Burrows, Pinel, Conolly, and most of our standard writers on mental disorders, I am pretty accurately acquainted: I trust, therefore (after these preparatory trainings, after the approved fashion of pugilism, &c.), that it may be permitted me to have some sort of an opinion on these matters. It seems to me that a man may in some cases so far give way to authority, and be led away by an adulation of celebrated names, as finally to have no opinion of his own. That excellent physician, Dr. Latham, in his sensible little volume on "Subjects connected with Clinical Medicine," published in 1836, has the following pertinent remarks: "Knowledge may be an incumbrance as well as a help. Many men know more than they are able to wield. There is a point (I believe) in the acquisition of knowledge (and this point varies infinitely in different individuals) beyond which, if more be required, the whole mass becomes useless to its possessor. I am acquainted with men who never have done, and who never can do anything, because they know too much; and I am acquainted with men possessing comparatively small knowledge, so dexterous in its use, that they have ridden over the heads of others far, very far, their superiors in acquirement. Nothing is more common than to hear it said of some eminent and distinguished person, 'eminent and distinguished as he is, what would he not have been had he possessed the learning of such an one?' Whereas, if he had possessed one particle more of learning than he has, he would have been nothing at all; it would have weighed him down, and he would never have been heard of.

"Fortunate, indeed, is the man who takes exactly the right measure of himself, and holds a just balance between what he can acquire and what he can use, be it great or be it small."

But to return to our more immediate

subject. The perusal of Dr. Carpenter's excellent Manual of Physiology, has afforded me much valuable information; the style (unlike many works in this department of science) is remarkably clear, and it contains many references to actual disease: this I conceive to be an immense acquisition in works of this kind. Why not combine physiology and practice of physic in the same book (of what use is either separate?), as Sir Astley Cooper taught anatomy and surgery, together, in his excellent lectures? Look at the illustrious Sydenham, who ridiculed the idea of anatomy *per se*, asserting that it was fit only for a painter: and yet show me a physician of the present day who can give a more graphic portrait of disease. I have in the course of my professional life, spent in several widely-separated counties of England, become personally acquainted with some minute anatomists and crack operators, men who could carve the human body to a nicety; and yet, in medical affairs requiring a little nice discrimination, or clear perception of matters going on below the surface, they were immediately beyond their depth. Such men should live in the butcher's shambles (or, Nero-like, transfix flies), for they literally revel in blood! "Tis their vocation, Hal!" The learned Schiller has remarked—"a physician whose horizon is bounded by an historical knowledge of the human machine, and who can only distinguish, terminologically, the coarser wheels of this piece of intellectual clock-work, may perhaps be idolised by the mob; but he will never raise the Hippocratic art above the narrow sphere of a mere bread-earning craft." The philosophic Feuchtersleben, in the preface to his most admirable "Medical Psychology," p. 9, remarks: "Body and mind are most intimately blended in every part of the structure of the living individual; and as the disorders of the mind are often removed by pharmaceutical remedies, so, on the other hand, the diseases of the body as frequently require the aid of the psychological physician. In disorders of the nerve especially, the physician can often effect nothing, if he do not in the first place direct his treatment to the mind. The numerous varying symptoms which, under the name of spasms, act so conspicuous a part in pathology, and unhappily a still more conspicuous part in real life, are often removed most suc-

cessfully and effectually by judiciously directing, controlling, and taking advantage of the state of mind; and, how few disorders are there of any organic system in which the nerves do not, at least sympathetically, suffer! We see, therefore, how extensive is the application of psychical methods of cure throughout the whole domain of the art."

Now, if these views be correct, and if in reality we do find in our actual observation of disease (at the bed-side of our patients) such an intimate connection and reciprocal sympathy existing between mind and matter, how shall we implicitly and blindly surrender up our judgment and experience into the hands of men whose dogmata are the results of some bright and momentary inspiration, some meteor-like ray, the sudden coruscation of the study lamp, and not the patient observation of medical facts during a long series of years, or deductions carefully and elaborately drawn from them? From the pen or the lips of a Watson or a Latham, a Brodie or a Travers, we may and actually do hail with reverence and gratitude the carefully selected and well-digested results of a long life honourably and usefully spent in ministering to the indigent and friendless poor the aid of medical or surgical science; such men, from their prominent position in public life, and the rigid performance of their hospital duties, have an irresistible claim upon our attention. Not so, however, with some physiologists of the present day; these learned individuals, with a rare and happy gift (intuitive and instinctive, we presume) spring forth, Minerva-like, armed cap-à-pie, from the brain of their father Jove (some crack medical school or new university of modern Babylon), and forthwith commence an indiscriminate onslaught upon all time-honoured and experience-tested medical doctrines: their magic and professional *caduceus* (like Aaron's rod before Pharaoh in the land of Egypt) swallows up all the minor constellations, and they at once assume the dictatorial throne.

We hear every day of new and startling discoveries in medical science: cod-liver oil, glycerine, æther, chloroform, gun-cotton, medical galvanism, *et hoc genus omne*; and doubtless the morbid and prurient taste for novelties of the present restless and excitable age must be gratified in some way: but, for my

part, whilst the present unnatural and arbitrary distinctions between cerebral, spinal, and sympathetic systems of nerves, each having an action independent of, and beyond the cognisance or control of the rest, are upheld; whilst learned and indisputable authorities insist that the brain is the exclusive domain of the physical principle ("ψυχή or immortal soul"), and has no connection whatever with other parts of the human body; whilst the spinal cord is the exclusive seat of the *vis nervosa*, and the presiding Deity (*alma mater*) over all the convulsive and spasmodic disorders, having at the same time no connection or relation to the sensorial faculty; whilst the sympathetic is purely and solely the nerve of vegetative or organic life—an independent centre, and wholly unconnected with and beyond the control of the cerebral or spinal centres; whilst the heart is independent altogether of nervous energy, and will even maintain its accustomed action long after its removal from the animal body; whilst the contractility or irritability of muscular fibre is due to some *vis insita* or inherent property totally distinct from nervous agency; whilst "the reduction of the food in the stomach, by the solvent power of the gastric fluid, is a purely chemical operation, with which the person's system has nothing whatever to do;" whilst absorption, assimilation, the circulation of the blood, nutrition, secretion, respiration, reproduction, &c. &c.;—whilst all these important changes and vital actions, so necessary and indispensable to the animal economy, can take place in the living machine called man, without the immediate connection or aid of the nervous principle or energy (call it by any name you please), I cannot look forward to any comprehensive or systematic principle of therapeutics being adopted by the present race of learned practitioners. Whilst such novel and ingenious views of the animal economy are prevalent, and are even enunciated by the High Priests of our medical mysteries and solemnities, and from professional chairs and modern universities, can it be wondered at that general and country practitioners of the present day (like Guerilla chiefs in the famous Peninsular campaigns) should wage war against disease, each under his own special banner? One will (Sangrado-like) bleed his patients copiously, keep

them in bed, and dilute the fluids of the animal economy with barley-water, &c.; a second will clear out the porridge-pot (as an old country practitioner used to tell me) with emetics and drastic purgatives; some will try mesmerism; others hydropathy, &c. &c., just as the brize stings them. In fact, one cannot help congratulating the world of sick patients on the brilliant variety and "*embarras de richesses*" of remedial agents, which will henceforth be brought to bear upon them. Of course, where one part of the human frame is not dependent (so the physiologists would tell us) upon another; where vital actions take place without nervous energy; and where doubtless (on the same theory), blood has no reciprocal action on nerve—or, *vice versa*, nerve does not influence blood; but all the animal functions "are merely the result of some purely chemical operation;"—such being the case, there appears to me no necessity for fundamental principles of therapeutics; and we shall probably ere long be furnished with new theories of medicine, to accord strictly with the doctrines of modern physiology.

Belper, South Derbyshire,
April 1860.

ON THE TREATMENT OF TYPHUS BY THE
INTERNAL ADMINISTRATION OF ICE. BY
M. WAUNER.

THE following is the mode of employment of ice which has been practised by M. Wauner, in typhus, during the last three years:—The patient swallows, every minute or two, a particle of ice about the size of a sugar-plum: these, when dissolved, do not amount to more than a glass or a glass and a half of water each hour.

When the natural temperature of the body is restored, and notwithstanding that the patient acquires extreme distaste for the remedy (a sure sign of his amendment), its employment is continued from twelve to twenty-four hours longer, according to the severity of the case. To relieve headache, the forehead is occasionally sponged with ice-cold water. An ounce of ice-cold water is also administered every six hours. Every alternate night the patient is placed in a bath at 27° R., a little over 90° Feh.—*Chirurgie Rendus.*

* * * If similar success attend the use of ice under other hands, typhus must cease from among the registered causes of death. M. Wauner states that *all his cases were cured!* Were they all cases of typhus? x

**EXPERIMENTS
ON THE
POISONOUS PROPERTIES OF
LOBELIA INFLATA.**

BY MR. CURTIS AND DR. PEARSON,
of Maryland.

THESE experiments were performed for the purpose of corroborating the evidence given at the trial of John Wood, at the *Carlisle Summer Assizes of 1849*.—See *London Medical Journals* for August and September, 1849.

Fluid preparations of the drug were preferred for internal use, because more easily administered. The alcoholic tincture is evidently far more powerful than aqueous preparations: these last, however, were used in order that the alcohol might not interfere with the effects of the lobelia.

The strength of our fluid preparations is in each case estimated according to the quantity of the drug used in preparing it, which, in the aqueous preparations, was so large that all the active property of the drug could not possibly be taken up by the water; but large quantities were used, in order that the preparation might be as strong as possible, for we experienced great difficulty in administering it. The tincture, when evaporated to an extract, appears to lose a portion of its activity, an insoluble resin being formed.

The immediate cause of death in the case of Wilson, and in the different animals, appears to have been extreme congestion of the lungs; the symptoms and post-mortem appearances in each case led us to this conclusion. The severe inflammations in the different tissues would, there is little doubt, have ultimately caused death, provided the congestion had failed to do so.

Experiments on the Hedgehog.

CASE I.—40 minims of the official tincture (equal to 5 grains of lobelia) were given by mouth, June 9th, 1849, at 8h. 40m. P.M.: the respiration increased from 21 to 64 per minute, and afterwards declined in frequency. The energy and muscular strength continued greatly diminished at 3h. 30m. P.M. of the second day, when 3iij. (equal to gr. xxiii. of lobelia) were administered, and it died immediately. It is extremely probable that in this case the

first dose would ultimately have proved fatal.

Section.—Immediately after death. The mucous coat of the stomach intensely inflamed and corrugated, especially at the upper curvature: the small intestines greatly inflamed at the upper portion, but less so towards the lower bowels: congestion, however, pervaded the whole tube; inflammation of the mucous membrane of the bladder; kidneys congested; all the cavities of the heart filled with fluid black blood; lungs greatly engorged; brain and its membranes congested; the pulsation of the heart continued for a considerable period after the body was opened.

CASE II.—June 9th, 1849.—Administered by mouth at 7h. 30m. P.M. Tinct. Lobel. 3i.—gr. viiss. In half an hour the respiration increased to 73 per minute, and was very laboured. The animal continued in a very debilitated state until 4h. 30m. P.M. of the fourth day, when 3ii. of the saturated decoction were given, which (if water extracted all the active properties of the drug) should equal gr. xxx. of the powder; but this appeared to have little effect. At 9h. 30m. P.M. of the same day, therefore, Tinct. Lobel. 3i.—gr. viiss. were given, great care being taken to prevent choking, as, from its sudden effects in the last case, we thought it possible a portion of the liquid might have passed into the trachea. The animal, however, died in half an hour, and so quietly that it was difficult to say when it ceased to breathe, the heart continuing to beat feebly for some time after the respiration had apparently ceased.

Section 18 hours after death.—The mucous coat of the stomach intensely inflamed and corrugated; small intestines inflamed, especially the upper portion; large intestines healthy; liver greatly congested; gall-bladder full; urinary bladder enormously distended, slightly injected at the neck; lungs much gorged with blood; the cavities of the heart, especially the left ventricle, filled with black blood; the vessels of the brain slightly injected.

CASE III.—June 18th, 1849.—Administered Tinct. Lobel. 3iiij. = gr. xxiii. at 2h. 30m. P.M. Collapse rapidly came on, and the respiration increased from 16 to 31 per min. In one hour tremor of the limbs was very evident; and in three hours and a half it struggled as if

in severe pain. The respiration continued extremely languid until the morning of the fifth day, the flies having deposited their eggs in its mouth, when, appearing rather stronger, Tinct. Lobel. 3j.—gr. xv. were given, and it died almost immediately.

Section 24 hours after death.—The mucous coat of the stomach intensely inflamed and corrugated; small intestines inflamed throughout, but particularly so at the upper part; the vessels of the mesentery enormously distended, the external surface of both mesentery and intestines being intensely red; liver of a sooty tinge in places; gall-bladder empty; urinary bladder intensely inflamed, but not much distended; the cavities of the heart contained black blood; lungs much engorged, and their pleural surface discoloured in patches, as if from inflammation; trachea and bronchi much inflamed; injection of the brain and its membranes.

CASE IV.—June 18th, 1849.—Gave, at 3h. 30m. P.M., 3iv. of the saturated decoction, which should equal gr. xlv. of Lobelia. Collapse, with laborious breathing, came on rapidly; and in half an hour the respiration was 24 per minute. At 6h. 15m. P.M. of the same day it struggled as if in severe pain, and vomited a quantity of clear fluid: died about the fortieth hour.

Section made 14 hours after death.—Stomach slightly inflamed at the pyloric extremity; duodenum slightly inflamed; liver natural; gall bladder full; urinary bladder slightly injected; heart flabby, the right side containing black blood, the left side empty; lungs gorged with blood; brain, its membranes and choroïd plexus congested.

CASE V.—June 18th, 1849.—At 4h. 30m. P.M. administered 3ss. of decoction as an injection, which should equal gr. xlv. of Lobelia. Collapse gradually came on, and at 6h. 45m. P.M. the respiration was 46 per minute.

3rd day.—At 9h. P.M., the animal appearing vigorous, though the nose continued very dry, the same dose was repeated.

4th day.—At 11h. A.M. 3ss. of Infusion, which should equal 3j. of Lobelia; and at 5h. P.M. 3ij. of Infusion, with 3ij. Tincture, equalling together 3j. of Lobelia, were given by injection. In half an hour convulsive tremor of all the limbs came on.

5th day.—At 10h. A.M. the animal had partly recovered, when 3ss. of Infusion, equal to 3j. of Lobelia, was administered, but returned almost immediately with fæces: vomiting also took place. The dose was repeated at 7h. P.M., which caused free vomiting and collapse.

6th day.—At 10h. A.M. 3ss.—3j. was given, but immediately returned, when the dose was repeated, but without effect. 3ss. was administered at 8h. P.M.

7th day.—At 10h. A.M. 3ij. of Infusion, equal to 3ss. of Lobelia; and at 4h. P.M. 3ss. equal to 3j. were given by mouth; at 8h. 30m. P.M. 3ij. equal to gr. xlv. of Lobelia, were administered, and he died in a few minutes: probably, however, some of the fluid might get into the trachea.

Section 15 hours after death.—Stomach inflamed and corrugated, particularly at the pyloric orifice: duodenum inflamed: rectum intensely inflamed, and the other intestines much injected: the mucous coat of the bladder inflamed: the right side of the heart full of black blood; the left empty: lungs greatly congested: congestion of the brain and its membranes.

CASE VI.—June 19th, 1849.—Administered by mouth at 8h. 30m. P.M. 3j. of Saturated Decoction, equal to gr. xv. of Lobelia. In a short time the respiration became increased in frequency, and he vomited a clear fluid.

2nd day.—At 12h. M. it appeared less vigorous, when 3ss. of Decoction, equal to gr. viiss. was given, after which he again vomited a clear fluid. At 4h. 30m. P.M. 3ss. equal to viiss. grains; and at 9h. P.M. xl. minims of Decoction, equal to gr. x. were given, which produced no effect for fifteen minutes, when 3j. equal to gr. xv. more was given.

3rd day.—It appeared vigorous at 10h. 15m. A.M., when 3j. of Infusion, equal to gr. xv. was administered, but produced no effect. In half an hour 3ij. equal to gr. xxx. were given; and at 5h. P.M. the dose was repeated.

4th day.—At 10h. A.M. Infusion, 3ij. equal to gr. xxx. of Lobelia, were given, but vomited immediately, and it appeared shortly to recover from the effects of the dose. At 7h. P.M. Infusion 3ij. equal to gr. xxx. were administered, which caused immediate purging, and, in a short time, general tremors of the body.

5th day.—At 2h. P.M. the respiration

was 45 per minute, the animal lying on its side, in which position it continued until 1h. P.M. of the seventh day, when it died almost imperceptibly, the flies having deposited their eggs in its mouth during life.

Section made one hour after death.—The mucous membrane of the stomach very soft, intensely red, and in some degree corrugated, indicating decided and severe inflammation, which, in a less severe form, pervaded the whole mucous surface of the intestines, which contained a copious secretion of mucus: the peritoneal coat injected: bladder showed evident signs of inflammation: lungs collapsed, only slightly crepitant, congested: the heart continued to beat for more than an hour after death, its right cavity containing black blood, the left being empty: the brain and its membranes congested.

Experiments on Cats.

CASE VII.—June 25th, 1849.—In this case equal parts of Tincture and Saturated Infusion were given, of which a few drops were administered by mouth at 10h. 30m. A.M., which caused almost immediate vomiting, when \mathfrak{zss} . equal to \mathfrak{xlv} . grains, were injected per rectum, which in a few minutes caused violent vomiting, a portion of the injection being returned immediately; rigors, followed by lateral rocking of the body, continuing at short intervals of about half a minute until 12h. M. The injection was repeated at 3h. P.M., but was immediately returned.

2nd day.—At 10h. A.M. gave \mathfrak{xij} . equal to \mathfrak{xxiiss} . by mouth, but it was immediately returned. At 2h. 30m. P.M. \mathfrak{zss} . equal to gr. \mathfrak{xlv} . was injected into the stomach by means of a syringe and elastic catheter, and a similar dose per rectum; that by the mouth was immediately returned, as well as some of that given by rectum; at least \mathfrak{xij} . equal to gr. \mathfrak{xxss} ., however, must have been retained. In a very few minutes severe collapse came on, the respiration increasing to 88 per minute, and afterwards remaining at 44 per minute. Pupils dilated, the eyelids nearly closed, and the membrane of the eye stretched to its full extent over the eye. The rigors and rocking of the body returned more regularly than after the former dose, the animal making many feeble efforts to escape, but always falling from extreme weakness, giving us the idea

that she suffered from suffocation. The animal appearing rather stronger at 6h. P.M., \mathfrak{zss} . equal to gr. \mathfrak{xlv} . was given by mouth, and the same by rectum, which, being all retained, produced immediate collapse, the respiration in 15 minutes reaching 104 per minute, declining during the next half hour to 48: the pulsations of the heart too frequent to be counted; the rigors and rocking motions were absent, but there were continued twitchings of the ears after each administration: the respiration became gradually slower, and sometimes rather irregular, until 2h. A.M. of the third day, when it died: the whole body was very cold at 9h. P.M.

Section made nine hours after death.—On opening the abdomen, the vessels of the portal system appeared greatly distended: the mucous membrane of the stomach much congested, of a slight rose colour, and filled with a light green fluid, having a slight odour of alcohol: the small intestines contained a large quantity of *fæculent* mucus of a deep green tinge, the mucous membrane being of a deep rose tint, and much injected: the large intestines loaded with *fæces*, much injected at the *cæcum*, but becoming less so towards the anus: the bladder much injected at the neck, the vessels being distinctly arborescent: the other organs of the abdomen healthy: the heart flabby, as if it had undergone distension, the right cavity containing a small quantity of fluid blood, the left being empty: the lungs, collapsed to a great degree, slightly crepitant, containing much blood, and a large portion of the upper lobe of the right lung discoloured, as if from extravasation: the blood in this, as in all the preceding cases, was fluid.

CASE VIII.—July 2nd, 1849.—This animal was suffering from disease of the skin, about six months old, but small and thrivelless in appearance. Administered at 11h. A.M. Infusion, \mathfrak{zj} ., equal to gr. \mathfrak{xv} ., which caused vomiting in the course of four minutes, the respiration increasing in a short time to 44 per minute: after the vomiting had ceased, another \mathfrak{zj} . equal to gr. \mathfrak{xv} . was given, which was also vomited, and caused purging. The dose was repeated before 1h. P.M. and retained: at 2h. P.M. the animal showed some signs of debility, the membrane being expanded over the eyes, when the same dose was again given, and retained: evident prostration

came on, a little urine being voided, but with evident pain and difficulty. At 3 P.M. a rocking motion was observed in paroxysms of three minutes' duration, with very short intervals, the impulse being antero-posterior in direction. At 9h. P.M., the collapse not being quite so great—though she still seemed in extreme pain—she took a little milk: the tremors, however, still continued.

2nd day.—The animal appearing to have partly recovered from the effects of the former doses, at 11h. A.M. 3ij. equal to gr. xxx. were administered and retained. For some time there was little appearance of suffering; but at 4 P.M. she was suffering from rigors and rocking motions, the direction being lateral, and almost incessant. At 5h. 30m. P.M. was lying on its side, and breathing very slowly: died at 9h. 30m. P.M., 34½ hours after the first dose.

Remarks.—In this case the animal took into its stomach a quantity of infusion which should equal about 90 grains; but most of this was returned by vomiting.

Section 13 hours after death.—The vessels of the mesentery gorged with blood; the peritoneal surface of the intestines rose-coloured and streaked with vessels; the exterior of the stomach of a pale rose colour; the interior slightly corrugated; the mucous membrane inflamed throughout, and of a rose tint, especially at the smaller curvature, and about the pylorus; the intestines inflamed, especially the ilium, which, with the jejunum, contained a considerable quantity of black blood; the ilium also contained a vast quantity of jointed worms; colon inflamed, but no appearance of blood; the vessels of the rectum very large, and gorged with blood, the bowel containing a large quantity of mucus; the liver pale in places, and much injected, in others showing signs also of extravasation; substance of the kidneys pale, but their vessels injected; bladder nearly empty, its mucous membrane inflamed throughout, but particularly at the neck; the pericardium contained a large quantity of serum; the right side of the heart containing a small quantity of blood, the left being empty; the lungs gorged with blood, and containing soft nodules, but whether tubercle or not, doubtful; the vessels of the brain nearly empty, its structure natural.

Remarks.—This animal had evidently

been suffering from previous disease; therefore the results are not so valuable.

CASE IX.—July 9th, 1849.—Six grs. of extract were carefully prepared by evaporating 3j. of Official Tincture of Lobelia by a gentle heat; this was mixed with Mucilage and inserted under the skin of the back at 10h. P.M. At 11h. P.M. had vomited, and appeared less spirited.

2nd day.—11h. A.M.: continues much the same, having voided ordinary faeces during the night; took a little milk.

3rd day.—6h. P.M.: appeared to have quite recovered from the effects of the drug; therefore 10 grains, prepared as the former, were inserted in another place under the skin of the back: this caused severe vomiting in fifteen minutes, preceded and followed by rapid pantings, with the tongue protruded as in the dog. In half an hour the respirations were 88 per minute; and at 6h. 38m. P.M. the membrane almost covered the eyes. At 7h. 10m. P.M. there was free vomiting, followed by almost constant tremors. At 7h. 30m. P.M. the membrane continued over the eyes, but she made vigorous efforts to escape, and a large portion of the poison exuded from the wound.

5th day.—She appeared to have quite recovered from the effects of the previous doses at 4h. P.M., when 3ij. of Infusion, equal to gr. xlviii. of Lobelia, were given by mouth: this caused immediate purging, and was vomited in a few minutes; therefore the dose was repeated at 6h. 35m. P.M., but was again vomited in half an hour. At 7h. 35m. P.M. 3j. of Infusion, equal to gr. vi. was given; tremors came on in fifteen minutes: in half an hour 3j. equal to gr. vi. was administered by rectum, which produced a powerful effect: in half an hour after the last dose, another ounce, equal to gr. vi. was given by mouth.

6th day.—Gave 3j. equal to gr. vi. by mouth, and the same by rectum at 1h. A.M.; voided urine copiously in a few minutes; was very weakly at 11h. 30m. A.M., and evidently still suffering from the effects of the last doses, when the injection was repeated. At 12h. 15m. P.M. the respirations were 128 per minute: it died at 1h. 30m. P.M.

Section.—The mucous membrane of the stomach rose-coloured, corrugated, and softened; duodenum ecchymosed in patches; other portions of the small

intestines slightly so; rectum corrugated, and of a deep rose colour; bladder healthy; the right cavity of the heart containing a quantity of black blood slightly coagulated, the left side empty; lungs greatly engorged; brain healthy; pus in small quantity, had formed in the wounds of the back.

CASE X.—July 11, 1849.—*Experiments with Injections alone.*—At 12h. M. administered ʒij. of Syrupy Extract of Lobelia, prepared from the Official Tincture: vomiting came on within twenty minutes; tremors within half an hour; she vomited at intervals till 12h. 35m. P.M.; ʒijss. were then given: vomited in 11 minutes; a very small quantity of faeces was passed during vomiting: at 1h. 40m. P.M. appeared in very great pain, and passed what appeared to be a little urine.

2nd day.—4h. P.M.: had apparently quite recovered: a quantity of Syrupy Extract, prepared with the Spt. Vin. Rect., was now given: passed a quantity of urine in five minutes; vomited moderately within seven minutes; in twenty-three minutes tremors came on; nearly all the injection was voided within half an hour. At 5h. 15m. P.M. tremors, severe and continued: at 6h. 40m. P.M. gave ʒij; at 7h. 30. ʒj.: at 11h. 45m. P.M. the animal being in great pain, and to all appearance dying, we gave ʒij. in order to destroy it, the pulsation of the heart being at least 140: she extended her paw as if in search of something, and continued to do so until 1h. A.M. of the third day: the eyes were closed.

6th day.—At 12h. 5m. P.M. appeared quite strong: ʒj. of Infusion, equalling gr. xii., was given, and all voided in seven minutes; in four minutes had begun to breathe rapidly; in twelve minutes vomited. At 12h. 24m. dose repeated; also at 2h. 42m. P.M., both being returned before 4h. 20m. P.M.: she then suffered from pain, with tremors. At 4h. 35m. P.M. dose repeated: at 7h. 5m. P.M. tremors continue, with cries; dose repeated: at 7h. 20m. P.M. discharged urine copiously, as also after a former dose. At this time the symptoms were increased: at 7h. 47m. P.M. a large portion was voided; dose repeated: at 10h. P.M. dose repeated.

7th day.—At 3h. P.M. had become strong again; dose repeated; in seven minutes vomited. At 3h. 15m. P.M. dose repeated; passed urine at 4h. P.M.:

at 4h. 20m. P.M. dose repeated: at 5h. 3m. P.M. dose repeated.

Remarks.—All the injections were voided almost as soon as given. To relieve the animal from torture, we gave prussic acid, which caused instant convulsions and death.

NOTE ON DR. DAVY'S OBSERVATIONS ON
THE AMOUNT OF
CARBONIC ACID EXPIRED IN
CHOLERA, AND ON ANIMAL
PUTREFACTION.

By WM. FRED. BARLOW, M.R.C.S.

In my paper on the condition of the body after death from cholera, I have spoken of the defective formation of carbonic acid during the cold stage of that disease, and of the question of putrefaction being in any way connected with the rise of temperature which is reported to have happened in the dead.

Dr. Davy observes, in a very kind letter with which I have been favoured by him:—"In 1818, when I first witnessed cholera in Ceylon, I noticed the muscular contractions after death from that disease, and also, during life, the diminished proportion of carbonic acid formed in respiration. The muscular contractions were first noticed in Ceylon, if I recollect rightly, by an able and esteemed friend, now no more, Assistant-Surgeon Finlayson. I believe they may be found in the Medical Reports, at the office of the Director-General of the Army Medical Department, in St. James's Place.

"In the second volume of my 'Researches' there are some observations on the putrefaction of animal matter, proving that the process is attended with increase of temperature and the formation of carbonic acid, most remarkable in the instances of the fibrin of the blood and muscular fibre exposed to the air—vide vol. ii. p. 343. I refer you to them, seeing that you mention in one of your papers Dr. Taylor's negative results."

Dr. Davy says further, in another and subsequent communication:—"Since receiving your note of yesterday's date, I have referred to a copy which I happen to have of my letter on cholera, as I observed it in Ceylon thirty-one years ago. From this I find that in all the

trials of the air expired, the proportion of carbonic acid was very much less than in health; that the venous blood was unusually dark, and little disposed to coagulate; and that there was little difference in colour between the venous and arterial blood."

I find that Sir Gilbert Blane has, in the eleventh volume of the *Medico-Chirurgical Transactions*, alluded with commendation to the observations of Dr. Davy and Mr. Finlayson, and given some account of their results. The cases which I extracted from one of Dr. Elliotson's lectures were noted and described by the latter gentleman; and it is the more necessary to state this, on account of their having been extremely well marked, very carefully observed, and clearly described.

The experiments of Dr. Davy on the expired air, showing the deficient quantity of carbonic acid it contained, are of no little interest. When one reflects on the state of the circulation in the collapse of cholera, it seems *impossible* to discover how carbonic acid could be formed and heat maintained as usual; but no inference drawn from reasoning can be compared for value, in inquiries of this nature, to the issues of experiment. It can hardly be doubted that less carbonic acid is exhaled than usual in cases of severe shock where the surface is chilled, in the sinking state in general, and all cases in which the blood-current is considerably enfeebled or impeded.

The different results of Dr. Davy and Dr. Taylor are no doubt to be explained by differences in their experiments.* The *nature* of the putrefying part, the *time* of putrefaction, and the *degree* of exposure to the air, clearly affect the inquiry. It would appear from Dr. Davy's researches that heat was owing to the *formation of carbonic acid*; if so, the *immediate* cause of animal heat is the same in the *dead and putrefying* as in the *living* animal. Dr. Davy's conclusions do not appear to affect what I have ventured to say already, as to there being no evidence to show that the rise of temperature noticed to have happened after death by cholera is caused by its agency. This inquirer distinctly states that he never, in his *experiments*, noticed a considerable elevation of temperature

unless when *putrefaction was most active*. And as to long maintenance of heat, it must be observed of the Bristol case, that it was not until the body had "cooled" that signs of putrefaction appeared on the abdomen. It must be remarked, too, that the formation of carbonic acid in dead animal matter, *after* the commencement of putrefaction, is in nowise inconsistent with the view that carbonic acid may be formed in the dead *before* putrefaction commences, under some circumstances; and so explain a rise of heat. Dr. Davy observes (*Researches*, vol. ii. p. 367):—"In a former part of this work, 'On the Temperature of the Human Body after Death,' two instances are given of an extraordinary high degree of temperature observed in bodies which had been dead a short time, and in both of which the fibrin of the blood had disappeared, as it does in putrefying; and yet the bodies exhibited none of the usual signs of the putrid change having even sensibly commenced. I then concluded, in consequence, that the unusual temperature was not owing to putrefaction, or a post-mortem effect; and, reflecting on all the circumstances of these cases, and on the phenomena witnessed in my experiments on putrefaction, I am confirmed in that inference; and obliged to suppose that it either arose from the ordinary sources of animal heat being more energetic than usual, or from the cooling process being comparatively inefficient, or else to imagine some peculiar morbid change in the blood itself, on which, in common with the disappearance of the fibrin, the unusual temperature may have depended."

There are many other observations of our author in the able and elaborate paper "On the Agency of Atmospheric Air on Dead Animal Matter, and on the Putrefactive Process," which deserve the most careful attention.*

METROPOLITAN INTERMENTS ACT. APPOINTMENT OF DR. SOUTHWOOD SMITH. DR. T. SOUTHWOOD SMITH, who was the medical member of the General Board of Health during the period of the Orders in Council (as authorised by the Diseases Prevention Act), has been appointed the second paid member of the board, provided by the Metropolitan Interments Act.—*Observer*.

* Dr. Taylor merely confined himself, in his note to me, to a detail of the result of *particular* trials; and, I am quite sure, intended to convey no *general* inference.

* See *Researches, Physiological and Anatomical*, by John Davy, M.D., F.R.S. Vol. ii. p. 331.

MEDICAL GAZETTE.

FRIDAY, AUGUST 16, 1850.

THE case of ANN MERRITT, who was convicted in March last, at the Central Criminal Court, of the murder of her husband by poisoning him with arsenic, has been recently brought to public notice under somewhat peculiar circumstances. In a debate in the House of Commons on the abolition of the punishment of death, this case was referred to by Mr. BRIGHT as showing the danger of inflicting "an irrevocable punishment, which might depend upon testimony of the nature of a medical opinion, liable to dispute, and to much and unavoidable uncertainty." The honourable member stated that this woman was sentenced to death "on the opinion of a medical man who appears to have given a rash and unscientific opinion, and which was afterwards strongly contradicted by the opinions of others of the same profession. In that case the proof was at length so strong, not of her being innocent, but of her *not having been proved guilty*, that the sentence was very properly commuted."*

The "medical man" here referred to—Dr. LETHEBY, a Lecturer at the London Hospital—addressed to the editor of the *Times* a letter in reply, from which we make the following extracts:—

"Had the liberal member taken the trouble to make himself acquainted with all the facts of Ann Merritt's case, he would have discovered that my opinions *were founded on fact, and that my evidence was the expression of truth.*"

* Some well-written comments on Dr. Letheby's evidence in this case appeared in a letter from Mr. G. B. Wadsworth, published in the *MEDICAL GAZETTE* of August 2d, page 218. It is only due to Mr. Wadsworth to state that our attention has been more particularly called to this important case by his judicious and temperate remarks. We have not hesitated to incorporate some of his objections in our strictures on Dr. Letheby's evidence.

"I take leave to say that the opinions advanced by me have never been publicly contradicted since the grounds on which they rest were placed fully and fairly before my professional brethren."

"In point of fact, the more I think over this case the more I bring it into relation, not only with the results of my own experience, *which is not small*, but also with the published accounts of other cases,—the more fully am I convinced that my conclusions were founded on fact, and that my evidence was the expression of truth."*

In a letter subsequently addressed by Dr. Letheby to the same journal we find the following passage:—

"I am well aware of the fact that Sir Benjamin Brodie, Dr. Leeson, and Dr. Ure, were consulted either by the sheriffs or the under-sheriffs concerning my evidence in the case of Ann Merritt; and I believe that, while every one of those gentlemen admitted that the questions put to me by the judge placed me in a very unsatisfactory and unenviable position, they also entertained opinions somewhat adverse to mine: but on this head I beg to state, first, *that their opinions were founded on most imperfect data, viz., the mere conclusion*, and not the premises, involved in my evidence; and, secondly, that though these gentlemen, as well as Dr. Billing and Mr. Davis, are all deservedly considered to be among the most eminent in the medical profession, yet (excepting Dr. Leeson), from the special nature of their practice, *and a total inexperience in the action of this particular drug*, they were not among the best qualified to give an opinion upon the toxic effects of arsenic. And I say again, that since the facts of this case have been fairly put before the medical world, no one has ventured to question the solidity of my conclusions."

We pass over the very bad taste which this letter displays on the part of a junior member of the profession in thus writing of men who had attained a

* *Times*, July 15th.—The *italics* are our own.

high rank as chemists and physiologists before Dr. Letheby had even entered the profession.*

In commenting on the *medical evidence* in this case, it is not our intention to discuss the degree of criminality which attaches to the wretched woman, nor is it any part of our design to import into the discussion, the propriety or impropriety of retaining capital punishment for the crime of murder. The woman may have been guilty of the charge, and have deserved to suffer death according to the present state of the law: all this may be conceded; but the great and material question remains—Was the medical evidence given on this occasion erroneous, or was it, as the witness himself wishes us to believe, "*the expression of truth?*" In support of the former view, we have the *opinions* of Sir B. Brodie, Drs. Billing, Pereira, Ure, and Leeson; and in support of the latter, the *statement* of Dr. Letheby. The Secretary of State did not hesitate to decide between these two conflicting opinions, and the sen-

tence of death was not carried into execution. The woman's life has been spared solely, as it appears, in consequence of a mistake in the medical evidence: and here we must observe, that however guilty in a moral point of view, a person may be,—if the verdict of the jury be in any way influenced by erroneous medical evidence, there is fair ground for the commutation of a sentence. It would not be consistent with humanity, or the safe administration of the law, that life should be taken when the medical opinion which has led to a verdict of *Guilty* is unsound, and contrary to general experience.

The history of the case of Merritt is comprised in a few words:—

The man was in his usual health on the night of Wednesday, January 23. On the morning of the 24th he was seen at his own house at *eight o'clock* in the morning: he was then sick, and he ascribed the sickness to his tea. At a *quarter* after *eleven* one of these witnesses saw him eating some gruel: at *one* to a *quarter* past, deceased said he felt very sick and queer and that he should not be able to do his work. Between *five* and *six* in the evening the same witness saw him again: he was then ill in bed, and complained of sickness and cramp in his feet.

Another witness saw deceased at twenty minutes past *twelve* on the morning of the 24th: he was then sick and vomiting.

A third witness deposed that he was with deceased and his wife (the prisoner) at twenty minutes before eleven o'clock on the morning of the 24th. He saw Mrs. Merritt pour out something into a basin, which he supposed to be gruel: she appeared to be mixing up a basin of gruel. The deceased took none of it then.

A woman named Gillett, who lived next

* We need hardly say that Sir BENJAMIN BRODIE's reputation as a physiologist and surgeon, and as a practitioner of forty-two years' standing in the metropolis, fully justified a reference of the medical facts of Ann Merritt's case to him. Some of his earliest contributions to science were on the absorption and action of poisons. Dr. BILLING is a scientific physician of high repute, who was long senior physician to the hospital in which Dr. Letheby is now a junior lecturer. Dr. BILLING delivered lectures there nearly thirty years since. Of Dr. URE and Dr. LEESON, as scientific chemists, we need not speak: their capacity for judging of the correctness of chemical evidence in a question of toxicology cannot be disputed. Rumour states that there is another individual who agreed with these distinguished men in condemning Dr. Letheby's evidence as unsound—namely, Dr. PEREIRA; but probably Dr. Letheby was not aware of this fact, or he would have been more guarded in setting up his own judgment against that of Sir Benjamin Brodie and Drs. Billing, Ure, and Leeson.

On referring to the Medical Directory, we find Dr. Letheby's qualifications to be a Bachelor of Medicine in the University of London, of *seven* years' standing, and a Licentiate of the Apothecaries' Society, his license dating from 1838. It appears that he is neither a Member of the College of Surgeons nor of the College of Physicians, and has only the half qualification of the University of London; yet he talks of the "*total inexperience*" of such men as Sir B. Brodie, Drs. Billing and Ure, on the action of arsenic, &c. &c.

* In order that Dr. Letheby may have no reason to complain of the source whence we derive our information, we have procured the Sessions Paper of the Central Criminal Court, March 1850, in which the minutes of evidence were taken in an authentic form by short-hand writers, and published under the superintendence of the city authorities.

door, was called by the prisoner at *half-past eight* o'clock on the morning of the 24th. The prisoner told her her husband was *very sick*, and had the bile. Deceased was seen by this witness between *ten and eleven* o'clock: he seemed to be ill. The prisoner was emptying some thick gruel out of a saucepan into a basin, and pouring some water to it. The gruel was made of oatmeal, fetched by a son of the witness's from the corn-chandler's at Mrs. Merritt's request. This was shortly after deceased came home ill. The prisoner gave as a reason for preparing it, that the deceased was *very thirsty*. This witness saw no more of either of them until *ten minutes past nine* at night, when Mrs. Merritt came for her. She found the deceased in bed retching violently, complaining of a burning pain in his chest, and a violent pain in his stomach. The witness remained at the house until *half-past twelve o'clock* at night, when deceased died,—herself and the prisoner being the only persons present at the time.

As the prisoner's guilt is not the question which we propose to discuss, we may merely observe that she made various statements which tended to show a guilty knowledge of the cause of her husband's illness. Among other circumstances adduced in support of the charge, it was proved that she had shown great eagerness to procure money from a burial-club to which her husband was a subscriber. She denied having possession of poison; but it was shown that on the 19th of January, only five days before her husband's death, she had purchased two separate pennyworths of arsenic at a druggist's shop. She said it was to poison some mice. The druggist put the arsenic in two packets of *white* paper, the quantity of arsenic in each packet being half an ounce. They were labelled *Poison*. When asked to account for the disposal of the arsenic thus traced to her possession, she said she placed it in a cupboard where her husband kept his soda

and acid powders, which he was in the habit of taking in the morning; "that she emptied the powder out of the paper with the word '*Poison*' upon it, and destroyed that paper; she screwed the poison up in another piece of paper, and left it in the cupboard; and, if he had taken the poison, he must have taken it in a mistake; that she afterwards destroyed the whole of the powders that were in the cupboard, and put them in the fire; she said she bought it (the arsenic) in a pet, and she intended to take it herself if her husband had gone on in the way he was going on."

We now come to the *medical evidence* in support of the charge. The first witness called was Mr. Toulmin, a highly respectable practitioner at Clapton. He deposed

that he was sent for between *half-past ten and eleven* o'clock on Thursday night, 24th January. He found deceased in bed, complaining greatly of pain in his stomach and cramps in his legs: he was sick. His pulse was very low, and his skin below the natural temperature. On Friday morning he saw him dead. The prisoner objected to a post-mortem examination; but this was made on the 28th, under the order of the coroner, by witness and Mr. Weloh, a surgeon in the neighbourhood. The stomach, with its contents, was removed from the abdomen. When opened, it contained *a thickish matter slightly pink*.* This was poured into a stoppered bottle. On the coats of the stomach there were red spots, such as are observed in persons who have died of an irritant poison. The stomach and contents were delivered to the assistant of Dr. Letheby, at the London Hospital.

* In a letter addressed by Dr. Letheby to the Editor of the *Medical Times* of March 23d, it is stated that Mr. Toulmin found, on inspection, "*three-quarters of a pint of undigested gruel in the stomach*." Mr. Toulmin, in his evidence, makes no such statement. He neither speaks of the quantity found, nor of the contents consisting of undigested gruel. This is Dr. Letheby's version of Mr. Toulmin's evidence. Yet, in his correspondence with the *Times* newspaper, Dr. L. refers to this letter as placing the facts "fully and fairly" before his professional brethren!

The next medical witness was Dr Letheby. As the evidence of this gentleman has been so seriously impugned, it is only fair to give it verbatim from the Sessions Paper:—

DR. HENRY LETHEBY: I am a bachelor of medicine and professor of chemistry at the London Hospital. On the 29th Jan. I received from Mr. Long a glass bottle and earthen jar—I entered on an analysis of their contents—I experimented on the contents of the bottle first, and detected *eight grains and a half of white arsenic*—by one course of experiments I reproduced the arsenic in a metallic form—it is in this tube (*produced*)—the earthen jar contained part of a human stomach—I noticed a peculiar appearance in it, which I have noticed in cases of poisoning by arsenic—there was a very small portion of a *whitish powder adhering to the inner lining of the stomach*, too small a quantity to ascertain what it consisted of—I then examined the intestines that were in the jar—I subjected them to a chemical analysis—the result was the detection of a very small quantity of arsenic—there was also in the jar part of a human liver—I subjected about a quarter of a pound of it to experiment—I say that, that you may form a judgment of the quantity in the entire liver), and obtained a quantity of metallic arsenic (*produced*)—it was *too minute a quantity to weigh*—that in the stomach was the only quantity I weighed—that would be quite sufficient to produce death—I had an opportunity of witnessing a case where two grains and a half killed—the general quantity would be eight grains—I look upon that as an average dose—it would generally be fatal—vomiting is almost invariably the consequence of arsenic introduced into the stomach—a person attacked in that way would be likely to throw up a portion of the arsenic—looking at the quantity I found, and the parts I found it in, in my judgment *the arsenic I found had been taken not more than two or three hours before death*, but that is a matter of opinion: a dose might have been given before—it would depend on many circum-

stances how soon it would find its way to the liver. .

Cross-examined.—Q. About five grains of arsenic, you say, would cause death; do you mean taken together? A. Yes, or less; $2\frac{1}{2}$ grains have done so—I know nothing of this transaction but from the examination—I found a very small portion in the liver, perhaps about 1-10th of a grain in a quarter of a pound of liver—a liver weighs about 5lbs. on an average—supposing it was equally diffused, there would be twenty times that quantity; that would be two grains—*my observation is reference to the time it had been taken, has reference both to the stomach and liver.*

Q. Are the data at all safe? A. Yes, I will tell you why: I found in the stomach $8\frac{1}{2}$ grains of arsenic, and there was not much in the intestines; I conclude, therefore, *that there had not been time for it to have passed into the intestines, which would have been the case if it had been taken a long time before death; but there was only a trace in the intestines, so I conclude that it was a very short time before death*—that furnishes datum to me to form a judgment on the subject of hours—food remains in the stomach five hours before it passes into the intestines—I am able to say that the contents of the stomach pass into the intestines within four, eight, or ten hours—from experiments which have been performed on living subjects, *I have not the least doubt*—I saw the intestines; they were in the jar; they did not appear to be influenced by arsenic; they were slightly red, and there were traces of arsenic; I have reduced something that was in the intestines into a metallic state—I experimented upon it, and found it was arsenic—it was destroyed in the experiment—I was obliged to submit it to experiment to prove it was arsenic—it is not likely I should find arsenic in the liver without some being in the intestines—the time would not depend on the constitution of the person—digestion depends on the constitution, but I am speaking of the average—digestion is more or less rapid, according to the constitution of the person who has received the subject matter—I have heard of cases

in which matters which would not digest have remained three or four days, but those were solid matters—I think liquids pass into the stomach (intestines?) under all circumstances within five hours, as (after?) they are imbibed—there is a valve which prevents solid matters from passing into the stomach till they are digested—the arsenic was in a liquid state; all except a little white powder on the side of the stomach—I am obliged to have recourse to an average to form an opinion as to how long it would take—we have no means of dealing with an independent case, but by the average.

MR. BODKIN.—Q. What did the contents of the stomach look like. A. Thick gruel; they were filtered, and I examined the filtered portion—my opinion is that the arsenic had been taken two or three hours.

Our readers will now perceive that the main question at this trial was not whether the man died from arsenic, or any other cause, but *At what time was the poison taken?* If taken on the morning of the 24th, then there was a remote possibility that such a mistake as that suggested by the prisoner might have been made; or that the poison, might by some accident, have entered the deceased's body at breakfast. If Dr. Letheby's evidence, however, were, as he alleges, "the expression of truth," then it follows that the arsenic must have entered the man's stomach only *two or three hours before death*,—i. e. from half-past nine to half-past ten o'clock at night, his death having taken place at half-past twelve. If any doubt could exist as to the meaning of Dr. Letheby's opinion, he has entirely removed it by publishing a letter in the *Times* of July 15th, in which he says—"Now if we were to found our judgment on actual general occurrences, instead of assumed special instances, we cannot, I think, but conclude, as I did, that the poison and gruel found in Merritt's stomach after death had been administered to him *subsequent to his going to bed, and within four hours of*

his decease." It will be observed that Dr. Letheby here adds another hour; but it will be seen that, in his statement at the Central Criminal Court, which alone could influence the jury in their verdict, he most positively limited the time to *two or three hours*.*

Fortunately for the prisoner, it happened that a medical practitioner of some experience was accidentally in Court, and heard with surprise this extraordinary piece of medical evidence. After the conviction of the prisoner, this gentleman, as we are informed, communicated his suspicion of the unsoundness of the medical opinion to the proper authorities; and, as we have said, it was condemned as incorrect by Sir B. Brodie, Dr. Leeson, Dr. Billing, and others: and the woman was consequently reprieved.

As the prisoner was attending on the deceased at the time so positively assigned by Dr. Letheby for the "*administering*" of the poison, and there was no suspicion of suicide, it followed, if his view was correct, that the prisoner must have given the arsenic to him. All possibility of mistake or accident was entirely removed by an asseveration so solemnly made.

Our readers who have had any experience in cases of poisoning with arsenic, must be well aware that, as a general rule, it is not possible, from the examination of the stomach and its contents, to assign *the period at which the poison was taken*. In general, the occurrence of symptoms, and the history of their progress, are looked to; but Dr. Letheby has adopted the novel

* In the so-called explanatory letter of March 23d, addressed to the Editor of the *Medical Times*, the same liberty is taken with the period assigned for the ingestion of the poison. "I concluded," says Dr. Letheby, "that the matters in question (undigested gruel and arsenic) had not been in Merritt's stomach for more than *four hours* before death." This difference of time is very material; and it is wholly concealed from the editor and readers of that journal, that the time sworn to at the Central Criminal Court was *not more than two or three hours*!

plan of basing his opinion on the comparative insolubility of arsenic in organic liquids, and on the time required for the digestion of gruel in a *healthy* stomach containing no arsenic!

There are two orders of circumstances by which the correctness of an opinion carrying with it such weighty results to an accused person, may be tested:—1, the evidence of facts deposed to by non-medical witnesses unacquainted with scientific theories; 2, the consistency of the opinion itself with general medical experience.

Dr. Letheby's opinion on this occasion is inconsistent, not merely with the facts proved in the case, but with all medical experience. He substantially told the Court that the arsenic which he found in the stomach was administered to the man not earlier than from half-past nine to half-past ten o'clock on the night of his death. The general evidence shows that at *ten minutes past nine* the woman Gillett was called by the prisoner to her husband. This woman went in to the deceased, and found him then retching violently, and complaining of a burning pain in his abdomen. From half-past ten to eleven o'clock he was seen by Mr. Toulmin, the surgeon; and the symptoms caused by arsenic, were then well developed. Indeed, the whole of the evidence tends to show that the poison was taken by the deceased shortly before eight o'clock on the morning of the day on which he died. No other theory will account for the symptoms under which he laboured throughout the day. His entire illness lasted about seventeen hours: the presence of the usual symptoms of irritation caused by arsenic were deposed to by various non-medical witnesses, and there is no evidence to show that the man had taken any poison subsequently to the time at which he was first attacked in the morning. Dr. Letheby's opinion involves the gross

improbability that *twenty minutes before* the prisoner "administered" the eight grains of arsenic found in the deceased's stomach, she fetched the witness Gillett; and that in the presence of this witness, and of Mr. Toulmin, the dose of arsenic, dissolved in gruel, was administered by her to her husband! The evidence, however, plainly shows that the man was in a hopeless condition, and actually dying, from the effects of the poison some time before the *earliest period* assigned by Dr. Letheby for its administration!

This unguarded statement, however, is not more at variance with the circumstances of the case than it is with medical experience. As facts justifying his opinion, Dr. Letheby relies on the allegation that the arsenic was *not in a solid state* in the gruel,* but in a *state of solution*. He says, further, that the quantity of gruel amounted to three-quarters of a pint (sixteen ounces), and that the arsenic dissolved in it weighed eight and a half grains. We pass over the great difficulty which must necessarily exist in determining whether so small a quantity of arsenic was actually dissolved, or, as Mr. Wadsworth suggests, mechanically diffused through so large a quantity of a liquid, described by Mr. Toulmin as a "thickish matter, rather pink;" for it is in practice scarcely possible to arrive at a safe conclusion on this point when, as in Merritt's case, the quantity of arsenic is only *half a grain to one ounce* of "thickish matter." Admitting, however, that the arsenic was *dissolved*, excepting the "little white powder" which adhered to the stomach, and the nature of which was not determined by the witness,† we

* See letter in *Medical Times*, March 23, page 237.

† This was a very serious omission, as the witness made so great a point against the prisoner, that the white arsenic was *not in a solid state*, but in a state of solution. Any quantity of white powder which could be seen adhering to the stomach could have been easily tested for arsenic. If proved to have been arsenic, the inference

cannot perceive how this shows that the arsenic had not been taken "more than two or three hours before death." If a person swallows a large quantity of tea or gruel some hours after having taken arsenic, it is by no means unusual to find a portion of the poison dissolved in this liquid. The powder hanging about the stomach becomes more or less dissolved by all liquids which enter the organ, especially if they remain in it for two or three hours, or longer. The witness assumes that vomiting would have removed it: but vomiting may remove a part, although not the *whole* of the poison; and the solution of arsenic in the gruel might have gone on until death, or even until the body was examined, *i. e.* three days afterwards.* It would be preposterous to assume, in a case like this, that because the arsenic was dissolved in the gruel it was therefore given with it† only a few hours

from *entire solubility* of the poison in the contents would have been, of course, subverted. Dr. Letheby, in his letter to the *Medical Times* justifying his opinion, does not even give the wretched woman the benefit of the doubt, to which she was clearly entitled, that the small quantity of whitish powder adhering to the stomach, and not analysed by him, was really a portion of arsenic undissolved!

* We must here again call the attention of our contemporary, the Editor of the *Medical Times*, to the very incorrect version of facts which his journal has been allowed to circulate under the designation of a full and fair statement. Dr. Letheby represents the question thus:—"Is it probable that a dose of arsenic taken early in the morning would have remained in the stomach for upwards of sixteen hours, in spite of the incessant vomiting, drenching with tea, &c.?" Mr. Wadsworth has justly remarked that this question is quite irrelevant, and has no bearing whatever on the case. If Dr. L. was prepared to swear that the deceased had taken no more arsenic than was found in his stomach after death (eight grains), there might be something in the argument; but there can be no doubt that the deceased actually swallowed a much larger quantity, and that only a small part of the "dose" so taken remained in the stomach, the rest having been removed by the vomiting (there is no evidence of *incessant* vomiting, drenching with tea, &c.).

† We have met with a case in which arsenic was swallowed in powder at one dose, and death took place in nineteen hours after abundant vomiting and purging. In the stomach there were twenty ounces of a bloody liquid, holding dissolved (as it was supposed), five grains of arsenic. No portion of the poison was found in powder. It might just as reasonably have been inferred, in this case, that the few grains of arsenic had been given in the *bloody matter*, as a vehicle, two or three hours before death; be-

fore death. All the medical facts in Merritt's case are consistent with the assumption that only *one dose* of poison was taken in the morning, and that none was given subsequently. The man no doubt swallowed a large dose, and the fact that a small quantity of the poison was found dissolved in sixteen ounces of gruel is certainly not adverse to this view of the case; but whether this assumption be adopted or not, we hold that the fact of solution affords not the slightest support to the opinion so strongly expressed by Dr. Letheby at the trial. The gruel, according to the witness, was found *undigested* in the stomach: hence he infers that, as gruel is digested in three or four hours in a healthy stomach, it could not have been longer than that time in the stomach in this instance. Dr. Letheby therefore assumes that gruel is just as readily digested in a stomach violently affected by arsenic, as in one that is perfectly healthy! We altogether doubt the correctness of this conclusion, and believe, from the evidence, that the gruel may have been there four, seven, eight hours, or even longer. The period required for the digestion of food in a stomach poisoned with arsenic has not yet been determined; and we doubt, even Dr. Letheby's experience, which he tells the Editor of the *Times* "is not small," can help us to a solution of this question. The witness's opinion respecting the shortness of the period during which the poison had been in the body, was, he states, further strengthened by the *small quantity* of arsenic (a mere trace) present in the *intestines* compared with the quantity (eight grs.) found in the stomach. In cross-examination, he said that there had not been time for it (the poison) to have passed into the intestines.* Is it pos-

cause, as there had been active vomiting throughout the day, this might have been supposed adequate to remove the *whole* of the arsenic.

* This is quite adverse to the previous statement, that "a dose might have been given be-

sible to produce from the annals of forensic medicine a statement more reckless or more dangerous in its consequences than this? If we find a smaller quantity of arsenic in the intestines than in the stomach, it is, upon this view, to be affirmed that it has *not had time* to pass from the stomach into the bowels; and, therefore, that it must have been introduced in the former organ but a very short time before death! It does not appear to have entered into the witness's thoughts that there were no means of determining in this instance how much had been already expelled by vomiting and purging, and that the smaller quantity found in the intestines might have been owing to that which had been taken some hours previously, having been drained away by purging. The witness first constructs a theory that the arsenic could not have been in the stomach more than two or three hours, and then makes every other medical fact bend to this. Cases are by no means unfrequent in which the larger portion of the poison is found in the stomach, while a small quantity (only a trace) is found in the intestines. The exact distribution of the poison by vomiting, purging, and the ingestion of solid or liquid articles of food, is so much a matter of accident that no person possessed of sound experience on poisons, would venture upon so loose an inference from the relative proportion found in the stomach or bowels!

But we are told that arsenic was found in the *liver* in the proportion of about the 1:10th of a grain in a quarter of a pound; making, therefore, by calculation, about *two grains* of the poison in the whole of the organ. We have then the extraordinary assertion that

the witness's observation in reference to the *time* the poison had been taken, had reference both to the stomach and *liver*: and, when asked whether the data were *safe*, he replied,—Yes. Dr. Letheby must, we think, have presumed greatly upon the ignorance of those whom he was addressing at the Central Criminal Court, to assert that the detection of *two grains* of arsenic in the liver was a confirmation of his view that the poison had *not* been more than two or three hours in the body! According to good experience, the fact of so large a quantity of arsenic being found in the liver would prove the very contrary.

As a certain period of time is required for the absorption and deposition of poisons, it is a reasonable inference that when one-fourth as much arsenic is found in the liver as in the stomach, the poison, in order to have become thus deposited, must have been in the body many hours,—sixteen or twelve at the least. Dr. Letheby, however, would have us believe that the *two grains* which he says he detected in the liver, were actually dissolved in gruel and administered to the deceased only two or three hours before he died! In how many livers of persons who have died from arsenic in two or three hours, has Dr. Letheby found so large a quantity of the poison as that here assigned? If he has met with this extensive deposition of arsenic in the liver in a single instance, where death has been caused by arsenic within this very short period of time, it is our opinion that he stands alone in his experience; if he has *not* met with such an instance, why did he recklessly swear to a speculation on which the capital conviction of an accused person probably rested? It is rare enough that death is caused by arsenic in so short a period as *five* hours; but from the complete confidence with which this deposition of the poison in the liver is stated to be a *proof* that it

fore;" because, with this admission, there would have been time for the poison to have passed into the bowels, and to have left more than a trace, supposing, as Dr. Letheby appears to assume, that when once there it ought to remain there.

could not have been in the body more than two or three hours, we can only presume that Dr. Letheby must have had the opportunity of examining the livers of many persons who have died from arsenic in *two or three hours*! Nothing short of this extensive experience would, in our view, have justified his answer,* or the very bold assertion that the data were perfectly *safe*.

We have now gone through the details of a very painful case. We do not grudge the space devoted to it, because we think that the question involved was one which, for the sake of public justice, demanded serious examination. Our readers will now be enabled to judge how far Dr. LETHEBY has been justified in charging such men as Sir Benjamin Brodie, and Drs. Billing and Ure, with "total inexperience" on the subject of poisoning with arsenic, merely because they disputed the correctness of the statements made by him at this trial. Let them now consider how far the conclusions of the witness were

* A case bearing on this question will be found in the 38th volume of the Medical Gazette, page 424. Dr. Geoghegan, of Dublin, who communicated the case to this journal, informs us that death took place in *seven hours*. The quantity of arsenic found in the liver was *two-fifths of a grain*, as nearly as could be approximately determined. In another case, in which death took place in *nine hours*, the quantity of arsenic in the liver was very small—too small to be weighed, or more than conjecturally estimated. The fact is,—these rapid cases of death from arsenic are so rare, as not to allow the question to be determined practically; and yet we have a positive statement stoutly sworn to, as if the premises on which it was based were susceptible of demonstration! Contrary to Dr. Letheby's rule, toxicologists generally consider that the detection of arsenic in the liver, especially in the large proportion of two grains, is a clear proof that the arsenic has *not* been taken a short time before death, but that it has been in the body *many hours*! In a case examined some years since by two eminent French toxicologists, MM. Bayard and Chevallier, they found arsenic in the tissues of the stomach and intestines, but not a trace in the liver. Their conclusion is in accordance with true experience—"qu'il est présumable que cette préparation arsenicale n'a été ingérée que peu d'heures avant la mort, puisque le foie n'en contenait aucune trace, et qu'il n'y a pas eu d'absorption par cet organe." (Annales d'Hygiène et de Médecine Légale, vol. xxxv. 1, 1846, page 149.) Dr. Letheby may, however, object to these gentlemen, as labouring, like Sir Benjamin Brodie and Dr. Billing, under "a total inexperience in the action of this particular drug!"

founded, as he alleges, on *fact*, and whether his evidence was really the *expression of truth* as it is interpreted by others. Dr. Letheby may have sincerely believed that what he swore to was true; but it is precisely this want of experience or judgment which rendered his testimony on this occasion unsafe. We do not hesitate to assert that every fact in this case is explicable on the supposition that the man took the arsenic in the morning, and died from its effects at night, after about seventeen hours' illness. There is no medical proof whatever that he had at any time more than *one* dose. There is certainly no valid reason for supposing that the prisoner gave him any arsenic within two or three hours of death; nor does it appear from any part of the evidence that the poison was taken in a dissolved form in gruel. Dr. Letheby has put a wrong interpretation upon the facts, and strained them in a way to suit his view, which no sound experience can justify. As a proof of this, we would only point to his theory regarding arsenic in the liver. According to MM. Chevallier, Bayard, and, we believe, all toxicologists, the fact observed by Dr. Letheby of the presence of two grains of arsenic in the liver would prove the very reverse of what he asserted at the trial. On a balance of probabilities it incontestably showed that the arsenic could *not* have been taken a short time before death.

We should have imagined that the time which had elapsed since the trial would have allowed Dr. Letheby to perceive the gross error of which he had been guilty, and which, under other circumstances, might possibly have consigned an innocent person to the scaffold. There was in this case a heavy weight of moral evidence against the prisoner; but Dr. Letheby's medical opinions were not, of course, based on this. Had the man swallowed the arsenic by mis-

take for an acid powder, in the morning, and had the woman been otherwise perfectly innocent,—here was a medical practitioner swearing in the most positive terms that the deceased must have had a dose of poison within two or three hours of death, when the wife alone was attending him, and when no other hand but hers could have administered the poison!

It appears from the letters recently published in the *Times*, that after five months Dr. Letheby persists in the correctness of opinions which we believe to be fraught with error most dangerous to all who happen to be accused of crime, and most damaging to the character of medical science. Six eminent professional men, of long and large experience, assure him that he is wrong; but he persists that he is right. His letters to the *Times* show that he sets public and professional opinion at defiance. Thus in the *Times* of only the 15th July, we find him asserting "that the opinions advanced by me have never been publicly contradicted since the grounds on which they rest were placed fully and fairly before my professional brethren."

After the complete failure of Sir B. Brodie, Dr. Billing, and others, we do not hope to convince Dr. Letheby that he is in error; but our object will be answered if we put him on his guard in future, or if we can succeed in teaching him that whatever his own experience may have been, he is bound, in a question affecting the life of a fellow-creature, to consult the experience of others. Had he done this with earnestness, he would have found on this occasion that the experience of others was decidedly adverse to his own. Our belief, however, is, that he was anxious to give a positive answer to every question, without, perhaps, at the time perceiving how far it was inconsistent with the facts of the case and with the doctrines of medical science.

The Secretary of State has, we believe, exercised a wise discretion in declining to order the execution of the woman Merritt, when it was highly probable that the opinions of the jury, in returning a verdict of *Guilty* against her, were influenced by unfounded medical statements regarding the time at which arsenic was taken by the deceased. If this woman were really guilty of murder, she owes her escape from capital punishment to the mistaken opinions of a medical witness. While it is far better that this should happen than that one who is innocent should be convicted and executed upon erroneous medical evidence, we cannot too strongly impress upon the minds of our readers, that no mistake of this kind can be made on a trial for a murder, without being attended with the danger either of leading to the acquittal of a guilty person, or to the conviction, and perhaps execution, of one who is innocent! All may learn from the facts of this remarkable case, that there are certain questions in Forensic medicine which cannot yet be solved by medical science. It is surely better at once to state this plainly, and say that the question proposed does not admit of an answer, than to make a jump in the dark and give a speculative opinion, in order to avoid the appearance of ignorance.*

* In the letter published at the commencement of this article, Dr. Letheby says that Sir B. Brodie, and others to whom the case was referred, admitted that the questions put to him by the judge "placed him in a very unsatisfactory and unenviable position." It is strange that Sir B. Brodie should have made such an admission. It was not the questions, but the answers, which placed the witness in an unsatisfactory position. If asked, When was the arsenic administered? the witness was not bound to reply that it had certainly not been taken more than two or three hours before death; he might either have referred the taking of the poison to an earlier period in the day, when symptoms of vomiting first came on, or he might have declared that he was not able to state at what period the poison was administered. The case would have then gone fairly before the jury.

Reviews.

Homœopathy and its Principles explained.

By JOHN EPPS, M.D. Published by the English Homœopathic Association. 8vo. pp. 320. London: Piper, and Baillière. 1850.

Dr. Epps begins this confession of his belief as to medical practice with a "brief reference to the treatment which Truth has, in all ages, experienced when first introduced to the notice of mankind." Thus—"The opposition," observes the author, "always created upon the discovery and the diffusion of any truth, has been proportioned in strength, intensity, and amount, to the interests which the truth, by the very necessity of its nature, either must, or appears likely to, overturn." The truth, as regards medical science, we are informed, *was pointed out by the finger of God, to the GENUISED* eye of Hahnemann*.† The pretension, however, to a divine revelation, is not new in these days! Because there are those among us who, after a laborious investigation of every branch of medical science, cannot discover in the *assumptions* of Hahnemannism the credentials of a divine development of truth, it is the practice of homœopaths to range them in the same category with the popish persecutors of Galileo, the opponents of Newton, the impugnors of Harvey, and the enemies of Jenner! Dr. Epps forgets that in having recourse to this line of argument he and his co-advocates of homœopathy have no stronger ground of offence or defence, than the advocates of chronothermalism, hygeian pills, or of any other system of quackery, no matter how monstrous its pretensions: one and all adopt the same line of argument as Dr. Epps, and which, we may observe, has no relation whatever to the subject under discussion. What interest, we would ask, has the medical profession beyond the cure of disease? Are not their studies, and is not their remuneration, directed to it and dependent thereon? Noble Lords, M.P.'s, M.D.'s, and other members of homœopathic associations, cannot be supposed to be wilfully ignorant or forgetful of the fact, patent to all the world besides,

that the members of the medical profession have ever been among the first and foremost of the movers of the most important, and most beneficial, social improvements. Therefore, all the records of "flames, faggots, tortures, racks, fiery furnaces," which are adduced as the lot of the adherents of *truth*, may be regarded as so many mere rhetorical flourishes, useful only to conceal the weakness of an argument.

What is Homœopathy?

Dr. Epps observes—"To make the answer to this question clear, and thus to demonstrate the truth of homœopathy, it will be necessary to notice some particulars in connection with *LIFE*; for it is to life in its modifications that homœopathy has relation."

"The particulars in connection with life," to which the author refers, are comprised in a verbose digression on the animal kingdom, vitality, health, &c., containing the most trite statements, familiar to schoolboys.

Then follows a picture of the practitioners of the "old system," consisting in a gross exaggeration of the defects of the "system," and a culpable suppression of its true merits,—a picture perhaps adapted to the tastes of homœopathic readers, who, however, it would appear, are not allowed to swallow abuse of rational medicine "in homœopathic doses."

It is not our intention to burden our pages with many quotations from this essay; but we will take, as specimens of the author's fairness or ignorance, the first case we find:—

"A sick person is sleepless. He prays for something to cause him to sleep. The wish is natural, and the physician ought to be able to aid him. The antipathic physician seeks to aid him by giving him an opiate. In doing so, he acts empirically without science." (p. 14.)

We need offer no comment on this exhibition of Dr. Epps's knowledge of the "old system."*

To return to the question—What is Homœopathy? Dr. Epps thus expresses its fundamental principle:—

"Inducing a medicinal disease, in symptoms similar to those presented in natural disease, will cure the natural disease."

Let us examine some of the *facts* that the author adduces.

* We are at a loss for the interpretation of this word: it doubtless has a homœopathic meaning!

† The italics are our own.

* A case recently reported in this journal (page 42), throws some light upon the mode in which homœopathic globules act to procure sleep.

"You knock yourself."

"You rub the part knocked,—that is, you use a succession of gentle but rapid knockings."

This is pure Lingism ! or Kinesipathy.

"You are palsied."

"You use strychnine, which produces palsy."

Does strychnine produce no other effect than palsy? If so, why not say so?

"You are frost-bitten."

"You rub with *snow*,—a result of frost, but you do not expose yourself to the identical frost again."

Is the above-named principle acted upon here?

"You have sore throat."

"Belladonna taken, which, according to Mr. Wade, produces swelling of the tonsils and inflammation of the palate."

What can be the value of such a *fact* as this?

"You inhale chlorine gas, and have induced a violent wheezing cough, with expectoration."

"Chlorine has become famous in cough affections, particularly in phthisis."

Wherein has chlorine become so notoriously famous in phthisis, as in its failures?

"Your horse is about to shy from fright."

"You strike him, and by this other fright prevent his shying."

This is not good horsemanship, much less has the inference any logical truth.

Homœopathy, as we have been often told already, teaches that the action of medicines is in conformity to the following law:—

"Diseases are cured most quickly, safely, and effectually, by medicines which are capable of producing symptoms similar to those existing in the patient, and which characterise the disorder." In this supposed law the author rejoices greatly, and on it enlarges quite in the "Ercles vein." He is especially strong on its certainty: thus—

"to cure a disease, all that is required is to find a substance, either presented in nature or contrived by art, which, if taken by a person in health, will produce in him symptoms similar to those manifested in the disease: give that to the diseased, and the disease will be cured." (p. 65.)

This is just the point of its greatest *uncertainty*, and the source of all error in this "system." Here a fundamental fact is assumed, and then reasoned upon in the style we have already quoted, instead of establishing an induction from an extended series of observed facts. It will serve but to a very small extent to hide this defect, that the homœopaths collect a long string of what they call the "uncertainties" of

the "old system." Such constitutes the greater portion of Dr. Epps's work.

The following is a specimen of the author's line of argument respecting the virtues of trituration and infinitesimal division of doses:—

"Every one is aware of the facts,—that, if eau de Cologne is rubbed on the hand, it gives out a scent much more intense than that given out without such friction; if a leaf of geranium is squeezed, it gives out a scent excessively strong."

What becomes of the principle *similia similibus*, after the following assertion, which we have marked with italics?—

"Those opponents, then, who argue that medicines in infinitesimal quantities do not act upon persons in health, waste much time, and expend uselessly much trouble. *No homœopathist maintains that they do.*"

"The opponents of homœopathy," observes Dr. Epps, somewhat triumphantly, "allow that patients, whose diseases have resisted all other treatment, whether allopathic, antipathic, or both, *do get well under homœopathic treatment.*" True; and so a large proportion of patients would under any harmless system of do-nothing treatment, *if they have only faith enough therein to let themselves be cured.* But, on the other hand, how is it that, when

homœopathic treatment has been employed in acute disease, the practitioner has been obliged to have recourse to the "old system" either himself, or to call in the aid of some other practitioner really competent to treat disease?

Dr. Epps states several of the objections that have been urged against homœopathy, and, we suppose, considers that he has refuted them. We cannot join him in his opinion.

We looked for some solid facts in this exposition of homœopathy, put forth by one of its chief advocates; but we find nothing of the kind beyond the reiterated assertion of the principle with which the author starts, which is, however, entirely taken for granted, and which he seeks to maintain by the aid of a great deal of matter extraneous to the point, interspersed with very full accounts of every weak point in the practice or in the theory of the "old system."

In answer, then, to the question, what is Homœopathy? we have, instead of a "*Demonstration*," a very pretty theory, wonderfully clear, and easy to be carried out in practice, and very sure in its results, *if only it were true*: that it is so, Dr. Epps's assertions have not shown. That the very reverse is true of this system we affirm, from having witnessed its evil results in the fact that disease has been allowed to go on to the destruction of tissues and organs, without the slightest attempt at arresting its progress. The pure homœopathist, however honest he may be in his belief, is practising only an absurdity; he does nothing, but he also ~~un~~ does nothing. The system of Hahnemann is opposed to every principle of the "old system," and proceeds on theoretical grounds the very reverse of those long established by medical science. There is, therefore, no room for a compromise between the two "systems;" and yet, opposed as these are, there are practitioners who undertake to combine the two, or administer medicines to their patients according to the rules of either. Such practitioners, it is clear, must abandon the doctrines of both, and must also possess in a very homœopathic proportion an appreciation of ethical principles. With such laxity, however, we certainly cannot charge Dr. Epps.

We shall only observe, in conclusion, that from the proved inadequacy of "globules" to relieve the symptoms of

acute disease, it is obvious that no medical practitioner can attend such a case in conjunction with a sincere homœopathist, without rendering himself an accessory before the fact to the destruction of the patient. There is no *mezzo termine*: he must either expel the homœopathist and his globules from the sick chamber, or coolly acquiesce in allowing him to die for the want of proper treatment.

On Tic Douloureux and other Painful Affections of the Nerves: with Suggestions for their Treatment by means of the Aneuralgicon; illustrated by numerous Cases, and an Engraving of the Apparatus. By C. TOOGOOD DOWNING, M.D., M.R.C.S. Pamphlet, small 8vo. pp. 73. London: Churchill. 1849.

THE author in the first place gives the varieties of neuralgia and tic douloureux, their diagnosis and treatment, and then introduces to the reader's notice the method of cure which it is his purpose, by the present publication, to make known to the profession: thus—

"From considering tic douloureux as often a local disease, depending on a state of excessive irritability, sensibility, or spasm of a particular nerve, and observing the effect of topical sedatives, I was led to the conclusion, that the most direct way of quieting this state was by the application of warmth and sedative vapour—something to soothe the nerves, and calm them into regular action. For this purpose I invented the Aneuralgicon, and it appears to answer the purpose admirably."

The author explains its nature as follows:—

"The Aneuralgicon is a name (compounded of *a-neurpa-algos*) applied to an instrument used by me for the purpose of allaying pain in nerves. It is a kind of fumigating apparatus, in which dried herbs are burnt, and the heated vapour directed to any part of the body. It is very simple in construction, and consists of three parts, with their media of connection—a *cylinder*, for igniting the vegetable matter; *bellows*, for maintaining a current of air through the burning material; and *tubes and cones*, for directing the stream of vapour." (p. 27.)

The plants which have been employed by the author are belladonna, henbane, cannabis indica, tobacco, aconite, stramonium, hemlock, digitalis, &c. These materials, being ignited, are placed in the cylinder; the fire may be urged by the

bellows to any degree of intensity, so that, besides the sedative, a rubefacient effect may also be obtained.

Dr. Downing speaks very strongly in favour of this instrument from his own experience. Cases are added in which the author had effected cures when every other means had failed. If the employment of this instrument be equally successful in the hands of others, medical practitioners will have reason to be thankful to Dr. Downing for an efficient remedy in a hitherto almost intractable complaint.

Proceedings of Societies.

ACADEMY OF MEDICINE, PARIS.

August 6, 1850.

Acute Rheumatism.

M. ANDRAL observed that, in the late discussion on the subject of acute rheumatism, one of the most important points brought under consideration was the state of the joints. M. Andral read the notes of a case in which he had recently had an opportunity of examining the morbid alterations that had been produced by this disease. The shoulders had been the seat of the affection, which was simple uncomplicated acute rheumatism. The patient, an aged woman, was debilitated by an attack of pneumonia when the rheumatism occurred: she had died in about eight days without an apparently sufficient cause. The interior of both scapulo-humeral articulations contained homogeneous pus, having all the characters of that which is the result of phlegmonous inflammation. The surfaces presented innumerable minutely injected vessels, forming the closest network, except on the articular cartilages, where this injection was but slightly marked. The bursæ about the joints were also filled with pus. The parts external to the joints, as the muscles, ligaments, &c., were in their normal condition. The cavity of the right knee-joint contained a quantity of fluid somewhat doubtful in appearance; but all other articulations were carefully examined, and nothing abnormal found.

Application of Electricity in Diagnosis.

M. MARTINER read an essay on this subject, and stated the following conclusions:—

1. The explosion of contractility and

sensibility by the aid of electricity is of value in the diagnosis of those diseases in which these are compromised. 2. The degree of contractility exhibited during the passage of electricity in paralysis of movement, furnishes a means of diagnosis, as does also the amount of electrical sensibility in paralysis of sensation. 3. The presence of electrical contractility is the distinctive character of cerebral, hysterical, and rheumatic paralysis; its absence is an indication of disease of the spinal cord.

SURGICAL SOCIETY OF PARIS.

August 7, 1850.

Imperforation of the Rectum—Operation—Cure.

M. DENONVILLIERS presented an infant upon whom he had operated successfully. The child (a male) was taken to him three hours after its birth. The genitals were divided into two distinct lobes by a vertical line: these lobes, which contained each a testicle, resembled enlarged labia. The glans, devoid of prepuce, was situated at the superior part of the fissure. The raphe was displaced toward the left side. The situation of the anus was occupied by a depression presenting cutaneous folds converging to the centre, and which, when the child cried, showed the action of a sphincter. A minute orifice, whence exuded apparently sebaceous matter, existed a little to the right of the depression. There was no protrusion, as of meconium, beneath the depressed integuments. M. Denonvilliers made an incision through the situation of the anus, and with some difficulty found the rectum, which terminated at a short distance above the seat of the depression, and was displaced towards the right. When cut into, no meconium at first escaped; but, by the use of an enema, a considerable quantity, in a hardened condition, was removed. The operation was completely successful in establishing the functions of the intestine and anus.

A long discussion followed, which was terminated by M. Denonvilliers promising to prepare a memoir on this subject.

THE CHOLERA AT MALTA.

LETTERS from Malta of the 2d inst. state that cholera was still raging there. On the 2d there were 69 cases and 41 deaths. The local government was censured for not issuing bulletins of the daily progress and fatality of the disease. The fleet was still in its cruising ground.

Hospital and Infirmary Reports.

ST. BARTHOLOMEW'S HOSPITAL.

Atonic Distention of the Bladder, Stricture, and Abscess in the Urethra.

W. W., *et.* 53, has had a stricture in the urethra for twenty-three years, during which time he has been under the care of several surgeons, and has had catheters passed repeatedly, sometimes for the sake of merely dilating the stricture, and sometimes to draw off the urine which has been retained. Retention has occurred frequently, but generally without much pain, although his bladder has sometimes been so much distended that two pints of water have been drawn off on the introduction of a catheter. It is now six months since a catheter has been introduced, and he has suffered from great straining and increased difficulty in voiding urine, which has lately only passed in drops; for the last three months he has observed a swelling in the lower part of his belly. He came to the hospital on July 31st, and was admitted under Mr. Stanley. A considerable swelling, evidently containing fluid, occupied the hypogastric region, and extended upwards to the umbilicus; it was not painful on pressure, nor had it the defined firm outline of a distended bladder, but was loose and flaccid, and very much resembled ascites: in fact the patient had been under treatment for this disease prior to his admission. He did not experience the uneasiness and desire to make water which usually accompanies retention of urine; the skin was hot, the tongue tolerably clean, and the pulse natural; the spongy part of the urethra, at one point in the perineum, was hard and slightly swelled. Mr. Stanley introduced No. 4 catheter, but not without some difficulty; and, before entering the bladder, the instrument made its way through a collection of pus situated in the neighbourhood of the bulb, and communicating with the urethra: the pus escaped through the catheter, and continued to do so until it was fairly in the bladder, when the urine flowed freely, and sixty-five ounces were drawn off. The swelling in the hypogastrium subsided, and intestines, filled with flatus, took its place.

Aug 1st.—As no urine has passed since yesterday, the catheter was introduced, and two pints were drawn off; some pus escaped from the urethra; the catheter was fastened to the penis and allowed to remain in the bladder.

3d.—The catheter slipped out of the bladder last night, but he has since voided a pint of urine by the natural efforts: it

passed in drops only. The swelling and hardness has nearly disappeared from the perineum; the skin is moist, the tongue clean, and there is no swelling in the hypogastrium.

Dislocation of the Hip in a child eighteen months old.

The infrequency of dislocation of the hip at such an early age renders this case interesting. The little child was brought to the hospital on July 29th; its friends could not give an accurate account of the manner in which the accident happened, but they believe a slight fall was the cause. The head of the femur was resting on the dorsum ilii; there was great mobility of the limb, and it was reduced with the greatest ease.

Operations performed on Saturday, Aug. 3.

Carcinoma of the Breast.

The first patient brought into the operating theatre was Mary W., *et.* 47, suffering from carcinoma of the right breast. She is the mother of two children, and first discovered the disease about two years ago, when it was in the form of a small hard knot on the side of the breast, immediately beneath the skin; but it now occupies the whole mammary gland and a portion of skin which is adherent to it. It has lately increased rather rapidly, and an operation was deemed expedient. When the patient had become insensible from inhaling chloroform, Mr. Skey commenced by including the nipple and diseased skin within his incisions, which were carried round the mammary gland, and this was dissected from the pectoral muscle: some enlarged glands were then removed from the axilla; a few vessels were secured, and the wound was closed by a broad strip of adhesive plaster. The operation lasted but a short time, and very little blood was lost.

Chimney-sweepers' Cancer.

The next was a case of chimney-sweepers' cancer on the scrotum of a strong and otherwise healthy man, *et.* 33, of temperate habits, who resides in Essex, and has been a chimney-sweeper for twenty-five years. This is the second time he has suffered from the same disease: on the first occasion, also, he was in the hospital under Mr. Stanley, and had a similar cancer removed, only one year ago, from the left side of the scrotum; he then returned to Essex, after having been in the hospital a month, and was obliged to resume his occupation before the wound was quite healed; it healed, however, in about two weeks, and he remained sound for three months, when a small lump appeared near the lower extremity of the old cicatrix; the lump in

creased, crusted over, cracked, ulcerated, and discharged a thin offensive matter; the ulceration increased and became painful, and at last compelled him to give up his work and return to the hospital, where he was again admitted, under Mr. Stanley, on July 25th, 1850. The disease occupied a considerable portion of the integument of the scrotum, as well as some of that on the root of the penis. Its appearance was that of an irregular ulcer, with hard elevated edges; it inclined rather to the right side of the scrotum, and did not follow the course of the old cicatrix, although they approached each other at their lower part; the old cicatrix was quite sound; the testicles, spermatic cords, and inguinal glands, were also sound.

Chloroform was administered to the patient, and Mr. Stanley removed the entire morbid structure by dissecting it from the subjacent tissues: at one point it was found adhering to the right tunica vaginalis; a small portion of this structure was therefore removed, and the testicle consequently exposed. A few bleeding vessels were tied, and the wound was closed with sutures.

Lithotomy.

The operation of lithotomy was then performed by Mr. Lloyd on Wm. M'Clare, *et. two years and nine months.* This little patient has had symptoms of stone for six months; and although he has suffered less pain than is usual, he has been unable to retain his urine, which has continually dribbled from him: he is of a delicate frame, and frequently suffers from an extensive prolapse of the rectum. The introduction of a sound, a few days ago, though performed when the patient was under the influence of chloroform, brought on such straining as to cause the gut to be protruded to a considerable extent—a prospect not at all encouraging to the operator.

On his being placed on the table a curved staff was introduced, and the patient was quickly under the influence of chloroform. Mr. Lloyd proceeded to perform the lateral operation in the usual way, using a straight knife convex on its cutting edge, and with a somewhat rounded extremity. As was expected, the rectum protruded, but was avoided without much apparent difficulty. Two stones, rather larger than filberts, were extracted. A gum-elastic tube was introduced through the wound, and the patient was sent to bed.

Medical Intelligence.

ADMIRALTY CIRCULAR RESPECTING THE RANK AND POSITION OF ASSISTANT-SURGEONS.

"My lords commissioners of the Admiralty are pleased to direct that the following regulations be established with reference to the rank and position of the assistant-surgeons of the Royal Navy:—

"1st. The assistant-surgeons are to be divided into two classes.

"2d. The first class is to consist of all those who have completed three years' servitude from the period of their first entry, one year of which, at least, must be on board a commissioned ship, and the other two may be served in one of H.M. Naval Hospitals, and who have passed the examination for surgeon, either at home or abroad, as directed by the Admiralty Instruction (Chap. II. Art. 26, p. 26); but in case the service on which the ship may be employed, should render it impossible that three surgeons can be assembled for a considerable period of time, then the captain may give an order to the surgeon of the ship to examine the assistant-surgeon whose period of time has expired, and, if found competent, may grant him a temporary certificate until the regular officers be assembled.

"3d. Assistant-surgeons who have served more than three years, and who have passed the examination for surgeon under the above conditions, are to rank next to naval instructors, and are to mess with the ward-room officers, to be allowed cabins when the accommodation and space on board will admit.

"4th. The second class of assistant-surgeons is to consist of all those who have not served three years, and those who have not passed their examination for surgeon.

"5th. The cabins specified in their lordships' order of the 26th of February, 1846, are to be strictly appropriated to the officers mentioned in that order, and their lordships' instructions on that head are to be fully carried out.

"6th. First-class assistant-surgeons, who may be serving in small vessels commanded by lieutenants, are to mess in the gun-room with the other officers.

"7th. The above regulations are to be carried into effect on the receipt of this circular order.

"By command of their lordships,
" (Signed) J. PARKER.

"Admiralty, July 17, 1850."

As soon as the Admiralty minute was promulgated at Portsmouth, relative to the

award of the privilege of the ward-room to assistant-surgeons, Captain Sir Henry Blackwood, of Her Majesty's ship *Vengeance*, issued an order that the assistant-surgeons of the *Vengeance* should take their seats in the ward-room.

DEATH FROM POISONOUS FUNGI.

LAST week Mr. Pratt held an inquest at Wisbeach Fen upon the bodies of Patrick Hunt, an Irishman, 18 years of age, and Winifred Garvey, 28 years old, wife of — Garvey, also an Irishman, when the following particulars were elicited:— Patrick Hunt, on the 2d inst., when walking by the Thorney Road side, saw and gathered a small quantity of what he thought were mushrooms, which he carried to his lodgings. He ate a portion himself, and gave some to Mrs. Garvey, who also ate a few, and gave a small quantity to her child, a girl 2 years old. They did not experience any serious effects that day, but at day-light on Saturday morning they were all attacked by violent sickness and purging, accompanied by great pain in the stomach and bowels. Neither the sufferers or their friends appear to have apprehended any danger; but on Saturday evening Hunt became suddenly worse, and died about 8 o'clock. At that time Mrs. Garvey's husband returned from work, and finding his wife much weaker and in pain he became alarmed, and medical attendance was obtained. On Sunday morning, however, she died; the child lingered until Monday morning, when she died also. The fourth death was that of Thomas Howd, an Irish child in another family, who it seems also partook of mushrooms, but not a portion of those eaten by the three first attacked, nor, as far as is at present known, gathered at the same time. A *post-mortem* examination of the body of Mrs. Garvey was made, when extensive patches of inflammation were found in the stomach and small intestines, very similar to those which are seen in cases of poisoning by arsenic.

PRESENTATION OF THE SWINEY PRIZE GOBLETS TO DR. PARIS AND M. FONBLANQUE.

AT the late annual distribution of the Rewards of the Society of Arts, Lord Colborne in the chair,—two silver cups, executed by Messrs. Garrard, after the design of Mr. MacIise, were presented to Dr. Paris, President of the Royal College of Physicians, and Mr. J. S. M. Fonblanque—the two cups being in place of a single one, which, in accordance with the will of the late Dr. Swiney, was last year given to 1000 gentlemen as joint authors of the *last treatise on Medical Jurisprudence.—The Lancet.*

THE POSITION OF MEDICAL PRACTITIONERS IN TURKEY.

THE government of the Sublime Porte has ordered the creation of a class of salaried public medical officers, whose duty is to attend gratuitously both rich and poor. From the latter they are expressly forbidden to receive any gratuity; from the rich, however, they are at liberty to act as their judgment dictates. They hold their appointment on condition of forwarding to the government a quarterly report of the number of cases attended, the nature of their diseases, the number of deaths, &c., with a meteorological record of the same period, and hygienic observations in reference to epidemics, intramural interments, &c. X

RE-VACCINATION IN PRUSSIA.

RE-VACCINATION is systematically practised in Prussia. No child is admitted into a school without proof of vaccination, and every recruit is vaccinated on admission into the army. In the year 1848, twenty-eight thousand eight hundred and fifty-nine soldiers were vaccinated: of these, the vaccine disease was regular in sixteen thousand eight hundred and eighty-two; in four thousand four hundred and four individuals it was irregularly developed; and in seven thousand five hundred and seventy-three it did not take any effect.

EPIDEMIOLOGICAL SOCIETY.

THE first meeting of the Council of the Society for the Investigation of Epidemic Diseases was held at the residence of Dr. Babington, the President, on the 9th inst., when Dr. Addison was nominated as Treasurer, Dr. Roupell and Mr. Nussey as Auditors. Sub-committees were appointed to frame a code of laws, and for other purposes. Interesting communications were read from abroad by the President. The meeting was well attended, and the Secretary reported that several new members had joined the Society.

UNIVERSITY OF ST. ANDREWS.

LIST of gentlemen who had the degree of M.D. conferred upon them, 3d of August, 1850:—William David Adams, M.R.C.S. Ed.; John M. Ballenden, L.A.C. Eng., and M.F.P.S. Glasgow; George Phillips Bevan; Walter Thompson Boddy, M.R.C.S. Eng.; John Young Bown, M.R.C.S. Ed.; John Campbell, M.R.C.S. Eng. L.A.C.; Richard Chapman, M.R.C.S. Eng.; Thomas Clarke, M.R.C.S. Eng. L.A.C.; William Simpson Craig, M.R.C.S. Ed.; John Thomas Dolman, M.R.C.S. Eng. L.A.C.; William Dean Fairless, M.R.C.S. Eng.; Richard Ford Foote, M.R.C.S. Eng.; Joseph Wilcox Haddock, L.A.C.; Joseph

Hewitt, M.R.C.S. Eng. L.A.C.; Thomas Coke Higga, L.A.C.; James Harvey Lilley, M.R.C.S. Eng. L.A.C.; Andrew Loewner; John Lyall, M.R.C.S. Ed.; Henry Madge, M.R.C.S. Eng. L.A.C.; Robert Slater Mair, M.R.C.S. Ed.; Charles Robbins Maxwell, M.R.C.S. Eng.; Alfred Peckett, M.R.C.S. Eng. L.A.C.; Benjamin Francis Popham, M.R.C.S. Ireland; Robert Rooke France, M.R.C.S. Ed.; William Richardson, A.M. M.R.C.S. Eng.; Robert Ripley, M.R.C.S. Ed.; Edward Seemka, M.R.C.S. Eng.; John Thomas Small, M.R.C.S. Ed.; Alexander Macfie Smith, M.R.C.S. Ed.; Hugh Thomson, M.R.C.S. Ed.; George Brown Turner, M.R.C.S. Eng. L.A.C.; Henry Noble Watson, L.A.C.; James Whitehead, F.R.C.S. Eng.

FELLOWSHIP OF THE ROYAL COLLEGE OF SURGEONS.

At the last meeting of the council of the institution, Mr. Cornelius James Philbrick, of Kidderminster, was admitted a fellow of the College, having previously undergone the necessary examination for this distinction: this gentleman's diploma as a member is dated August 17, 1888.

ROYAL COLLEGE OF SURGEONS.

GENTLEMEN admitted members on the 9th inst.:—Messrs. T. A. Brandt.—R. T. Cashe.—B. Carrington.—T. Mawhinny.—J. C. Bury.—J. H. Smithwick.—M. M. McKensie.—A. Hill.—T. Scattergood.—T. Riordan.—T. Creed.

Admitted on the 12th inst.:—Messrs. G. H. Watts.—H. Behrend.—M. Egan.—W. H. Clarke.—G. Ridsdall.—W. T. King.

APOTHECARIES' HALL.

NAMES of gentlemen who passed their examination in the science and practice of medicine, and received certificates to practise, on Thursday, 8th August, 1880:—Thomas Scattergood, Leeds, Yorkshire—Sidney Henson, Hull, Yorkshire—Harvey William Dixon, Putney, Surrey—William Peck, Sharnbrook, Bedfordshire—Samuel Hayman Warren, Milverton, Somerset—Edward Ralph Allen, York—James Edward Dickinson, Hart Street, Bloomsbury Square—Herbert John Walker, Sheffield, Yorkshire—Henry George Allanson, Scarborough—James Croston, Atherton, Manchester—Nathaniel Watton Jones, Chichester—Charles Whetmough, Woore, Salop—William Blackborne, Clapham, Great Baddon, Essex—Christopher Nugent Spinks, Ireland—William Robey Strelley, Oakenhorpe, Derby.

BOOKS & PERIODICALS RECEIVED

FOR REVIEW,

(The List will be given in our next No.)

BIRTHS & DEATHS IN THE METROPOLIS

During the Week ending Saturday, Aug. 10.

BIRTHS.		DEATHS.	
Males....	707	Males....	497
Females..	684	Females..	500
	1391		997

CAUSES OF DEATH.

ALL CAUSES	997
SPECIFIED CAUSES	982
1. <i>Zymotic</i> (or Epidemic, Endemic, Contagious) Diseases....	28
<i>Sporadic Diseases, viz.—</i>	
1. Dropsy, Cancer, &c.	6
2. Brain, Spinal Marrow, Nerves, and Senses	10
4. Heart and Bloodvessels	6
5. Lungs and organs of Respiration ..	7
6. Stomach, Liver, &c.	7
7. Diseases of the Kidneys, &c.	3
8. Childbirth, Diseases of Uterus, &c. ..	1
9. Rheumatism, Diseases of Bones, Joints, &c.	1
10. Skin.....	1
11. Old Age	20
12. Sudden Deaths.....	5
13. Violence, Privation, Cold, &c....	20

The following is a selection of the numbers Deaths from the most important special causes

Small-pox.....	15	Convulsions.....	1
Measles.....	13	Bronchitis.....	2
Scarlatina.....	31	Pneumonia.....	1
Whooping-cough.....	26	Phthisis.....	1
Diarrhoea.....	152	Lungs.....	1
Cholera.....	15	Teething.....	1
Typhus.....	36	Stomach.....	1
Dropsy.....	17	Liver.....	1
Hydrocephalus.....	27	Childbirth.....	1
Apoplexy.....	23	Uterus.....	1
Paralysis.....	16		

REMARKS.—The total number of deaths was 34 below the average mortality of the 22d week of ten previous years.

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer 30.4
 " " " Thermometer " 62°
 Self-registering do. " Max. 98° Min. 47°
 " From 12 observations daily. " Sun.

RAIN, in inches, 0.16.—Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was 5° above the mean of the month.

NOTICES TO CORRESPONDENTS

Communications have been received from H. Lonsdale and Dr. Fearnside.

Dr. Fearnside's paper will be inserted, and proof sent.

The letter of Mr. Rose, and the papers of E. Snow and Mr. Bate, will appear in the following number.

CORRECTION.—In Dr. THT's paper, in last number, page 235, col. 1, 13 times *sum* for "hermetic," read "hemetic."

Lectures.

COURSE OF LECTURES

ON

DISEASES OF THE HEART.

Delivered at St. Vincent's Hospital during the Session 1849-50.

BY O'BRYEN BELLINGHAM, M.D.

Fellow of, and Member of the Court of Examiners of, the Royal College of Surgeons in Ireland, and one of the Medical Officers of the Hospital.

LECTURE X.

EXAMINATION OF THE HEART IN DISEASE, CONTINUED.

Abnormal sounds developed during the heart's action—Pericarditic friction-sounds—Endocardial or valvular murmurs—Bruit de soufflet—Circumstances under which bruit de soufflet is heard—Conditions under which bruit de soufflet is developed—Mechanism of production of bruit de soufflet—Bruit de soufflet at the right side of the heart—Conclusions from the presence of bruit de soufflet.

Abnormal sounds developed during the heart's action.

THE sounds which either replace or accompany the normal sounds of the heart, and which are new, morbid, or abnormal sounds, were first named *murmurs* by Dr. Forbes; and this term has been very generally retained since. They present different characters in different cases of disease, being sometimes blowing, sometimes resembling rather the sounds of sawing, filing, grating, whistling, &c.: and this circumstance has been deemed by some writers of sufficient importance to form the ground of their classification, and they have been divided into the "sounds of blowing," and "sounds of friction;" but as in every instance they are the result of *friction*, generally between a liquid and a solid, sometimes between two solids, they would be more correctly classified according to their seat. Thus, sometimes they have their seat exterior to the heart, and between the opposed serous surfaces of the pericardium: these are the *pericarditic friction-sounds*, or the *peripheral or exocardial murmurs*. Sometimes they have their seat within the heart, and at the orifices of the ventricles: these are the *endocarditic, endocardial, or valvular murmurs*. Sometimes, again, they have their seat in the large arteries; at others, in certain veins: the former are the *arterial*, the latter the *venous murmurs*.

The abnormal sounds developed during

the heart's action will be considered here in the following order:—

1st. The *pericarditic or exocardial murmurs*, or the *peripheral friction-sounds*; which accompany the movements of the heart, but do not interfere with its intrinsic sounds.

2d. The *endocardial or valvular murmurs*, subdivided into the *systolic and diastolic*, which either take the place of the normal sounds of the heart, or prevent them from being heard.

3d. The *arterial murmurs*.

4th. The *venous murmurs*.

It will now be necessary to describe these sounds. To be appreciated, however, they must be heard: no verbal description can supply its place.

Pericarditic friction-sounds.—The abnormal sounds which have their seat between the opposed serous surfaces of the pericardium are termed *attrition murmurs*, or *friction-sounds*, from their character; and *pericarditic, exocardial, or peripheral murmurs*, from their seat. Although all the abnormal sounds which we have now to consider are the result of friction, the *exocardial murmurs* are more particularly entitled to the name, because the friction here takes place between two solids: in all the others, one of the agents in the production of the sound is a liquid.

In a state of health, the opposed serous surfaces of the pericardium being exceedingly smooth, and constantly moist, glide over one another, during the motions of the heart, without producing any sound, or, at least, any which can be detected by the ear applied to the parietes of the chest. When, however, as the result of inflammation, lymph is effused upon this membrane, and its opposed surfaces become rough, unequal, or irregular, friction to a greater or less degree takes place during the movements of the heart; and this friction develops sound, which becomes audible when the stethoscope is laid upon the parietes, and is frequently sufficiently strong to communicate a distinct vibratory sensation to the hand, as was first pointed out by Dr. Stokes; and according as the action of the heart is violent or feeble, and as "the lymph effused is small in quantity, partially deposited, and forms a thin, smooth layer, or is copious, hard, and irregular, the sounds will vary from the slightest degree of rubbing, to a loud, harsh, grating, or creaking sound."

The attention of the profession was first called to the friction-sounds of pericarditis by Dr. Stokes.* M. Collin,† it is true, had described the *bruit de cuir neuf*, one

* Dublin Journal of Med. Sciences, Sept. 1833.

† Les Diverses Méthodes d'Exploitation de la Poitrine. Paris, 1834.

of the rarest of these sounds, and Broussais* had noticed a friction-sound in pericarditis similar to that of two dry bodies, like parchment, rubbing against one another; but their remarks attracted no attention: it was reserved for Dr. Stokes to point out the distinction between the physical signs of the two forms of pericarditis; to determine those which belong to the variety of the disease in which the opposed surfaces of the pericardium are coated with lymph; and to demonstrate that this form of pericarditis, which heretofore had been supposed to yield no stethoscopic signs, was really the one in which auscultation is of the greatest assistance to the diagnosis. Dr. Stokes's paper, entitled "Researches on the Diagnosis of Pericarditis," was published in September 1838; in the following year M. Bouillaud† description of the friction-sounds of pericarditis appeared; and, in the year 1835, Dr. Watson,‡ in giving the details of two cases of pericarditis, described the friction-murmur heard in them under the name of the "to-and-fro sound."

The friction-sounds of pericarditis have received various names. They may all, however, be included under the following heads, originally laid down by Dr. Stokes in the paper to which I have referred:—

1st. "A slight friction-sound, perceptible only at the very commencement and at the termination of each diastole and systole of the heart."

2d. "A rasping-sound, very similar indeed to that produced in the worst cases of ossification of the valves. In others the sound is similar to the frotement of pleurisy, only modified by the action of the heart."

3d. "The sound resembles the creaking of new leather, to which it was originally compared by Collin."

The slight friction-sound, the "bruit de frottement" of the French, resembles the sound produced by rubbing the hands together, or the rustling noise produced by crumpling the paper of which bank-notes are made, to which M. Bouillaud compared it, or the rustling of silk; or, when stronger, the crackling of parchment. According to Dr. Hope, this sound is produced by the presence of soft wet lymph upon the opposed surfaces of the pericardium. It seems to me to be a probable supposition, that in the cases of pericarditis in which this sound is alone heard, and where all through it preserves the same character, it is due to the lymph being deposited

merely upon one of the opposed surfaces of the pericardium.

The rasping sound.—The *bruit de rapement* or *bruit de frottement* of the French, has, as its name denotes, a harsh rasping or grating character; it often resembles accurately the rasping or the sawing of wood. The sound which it appeared to me most closely to resemble on several occasions was that which would be produced by scratching with the nail the surface of dry bone. It constitutes the to-and-fro rubbing sound of Dr. Watson. Dr. Hope refers this sound to the presence of firm and rugged lymph, the sound being loud in proportion as the lymph is rough, and the action of the heart strong.

The creaking sound of new leather, the *bruit de cuir neuf*, is one of the rarest of these sounds; it resembles accurately the creaking of a new saddle, but is not so loud; it may be imitated, Dr. Hope observes, "by rubbing together the fingers made sticky by resin." We had lately a case in hospital where a sound very similar to this was developed in the bronchial tubes, owing to their obstruction by tough mucus: here, however, it of course accompanied only the respiratory movements. This creaking sound was supposed by Dr. Hope to depend upon the adhesive nature of the lymph, and to be an indication that adhesions were about to take place. More recent observations have, however, shown that this is not necessarily the case. Dr. Copland* refers it to thickening or condensation of the sub-serous and serous tissues of the pericardium, especially of that portion reflected over the heart; and the formation of a dense and elastic false membrane.

Another sound has been described by Dr. Hope under the name of the "continuous rumbling murmur," which he refers to the presence of a small quantity of fluid in the sac of the pericardium, the opposed surfaces of which are also coated with lymph. He supposed that the agitation or "churning of the fluid during the motions of the heart produces the sound." It is more rare than any of the other pericarditic murmurs, with the exception, perhaps, of the *bruit de cuir neuf*.

Conclusions respecting the pericarditic friction-sounds.

When I come to speak of pericarditis, I shall have occasion again to recur to these sounds. It will be sufficient here to observe—

1. That they are almost limited to cases of inflammation of the pericardium; that

* Commentaires des Propositions de Pathologie, tome i. 1829.

† Dict. de Méd. et de Chir. Prat., art. Pericardite. 1834.

‡ LONDON MEDICAL GAZETTE, April 1835.

* Dict. of Pract. Medicine, vol. ii.

they are pathognomic of one form of this disease, and constitute the most valuable diagnostic signs of it.

2. That they have all, more or less, a friction or attrition character.

3. That they are almost always double, and are usually better marked during the period of the ventricular systole than during the diastole.

4. That they appear to be superficial and near, and are seldom audible much beyond the limits of the præcordial region.

5. That they do not, like the murmurs next to be described, replace the ordinary sounds of the heart, but are heard independent of them.

6. That their duration is usually short; frequently ceasing entirely after having been heard for a few days, and not unfrequently changing their character within the period that they are audible.

7. That they are frequently accompanied by a peculiar vibration or *frémissement* sensible to the hand laid upon the parietes.

8. That a bruit de soufflet at one of the orifices of the left side of the heart, the result of endocarditis, not unfrequently coincides with these sounds.

Endocardial or valvular murmurs.—The abnormal sounds which have their seat at the orifices of the ventricles, and which replace or accompany the normal sounds of the heart, constitute the most valuable signs of disease of the valves or orifices of the left side of the organ. They present several varieties in different cases,—being sometimes blowing, when they constitute the varieties of the bruit de soufflet or bellows-murmur; sometimes rough and harsh, resembling the familiar sounds of sawing, rasping, or filing; and at other times having a whistling or musical character, constituting the varieties of the bruit de soufflet musicale, or musical murmur.

Bruit de soufflet.—The bruit de soufflet or bellows-murmur, as the name denotes, has a blowing character, and is by many degrees the most frequent abnormal sound heard in diseased states of the valves or orifices of the heart. It was discovered and named so, by Laennec, from its similarity to this well-known sound, to which it often bears a most ridiculous resemblance. Bruit de soufflet does not always however, present the same character: sometimes it is perfectly smooth and soft; at other times it is loud and rough; sometimes it is a mere whiff: at others it is prolonged and lengthened out. It may accompany or replace either the first or the second sound of the heart, more frequently the first; or it may pass into a sawing, rasping, or musical murmur. In the vast majority of cases of disease, it is limited to the left side of the heart.

Circumstances under which bruit de soufflet is heard.

Bruit de soufflet is heard in various and different lesions of the valves and orifices of the heart; it is not, however, limited to diseased states of this organ, but becomes audible under a variety of other circumstances, as we shall presently see. Thus, it is heard when there is any obstacle to the free passage of the blood through the orifices of the left side of the heart, or when the valves imperfectly perform their functions, and permit regurgitation. It is heard in aneurism of the left ventricle and in congenital malformations of the heart, where a communication exists between the ventricles. It is heard in cases where the lining membrane of the arch of the aorta is diseased, and in aneurism of all the large arteries. It is heard in aneurismal varix, where a communication exists between an artery and a vein, in varicose aneurism where a small sac exists between them, and in the pulsating form of aneurism by anastomosis. It is frequently heard in cases where no disease of any kind exists in the heart or arterial system, but where the quality of the blood is altered; where this fluid has become more watery and less viscid than natural. Finally, it can be developed at any time in the heart and arteries by suddenly abstracting a large quantity of blood, as was first pointed out by Dr. Marshall Hall; or in a large artery by making pressure upon it so as to diminish its calibre.

Bruit de soufflet may be developed in the cavity of the chest by the growth of a tumor of any kind, which comes to press upon the aorta or its branches, or upon the pulmonary artery; or by malformation or deformity of the chest, owing to disease of its bony parietes, by which the heart's movements are impeded or the large vessels are compressed. Bruit de soufflet is said to have sometimes been heard for the first time, a short period previous to death, in the advanced stage of pulmonary or other diseases: M. Bouillaud refers it then to the formation of fibrinous concretions in the cavities of the heart, which interfere with the action of the valves, or obstruct the orifices. It is temporarily heard in violent palpitation in hysterical subjects, or in attacks of palpitation in cases where the left ventricle is hypertrophied, and its cavity dilated: here it disappears as the palpitation subsides. Bruit de soufflet, in a very marked form, is likewise heard from the fourth month to the end of pregnancy: here its site is usually the iliac fossa, sometimes on both sides, more frequently only on one side.

Dr. Graves* has noticed the remarkable

fact, that a bruit de soufflet is sometimes audible, in the second stage of pneumonia, over the affected lung, which disappears as the symptoms of the inflammation subside: it is not perceptible in the subclavian or carotid arteries of the same side, and probably, when present, depends upon pressure on the branches of the pulmonary artery. Dr. Latham* has called attention to the circumstance that a gentle bruit de soufflet, which coincides with the ventricular systole, is heard occasionally in phthisis: it is not perceived in the præcordial region, but in a circumscribed space above it and on the left side. M. Zehetmayer† has confirmed the correctness of Dr. Latham's observation, and would appear to have noticed this murmur previous to the publication of Dr. Latham's work. He says, "I have repeatedly heard, in phthisical persons, in the second intercostal space, a decided bellows-murmur, instead of the first sound of the pulmonary artery; and in a place where there was no doubt that tubercular infiltration was present. No alteration in the vessel could be detected on post-mortem examination." Dr. Latham supposes that this murmur may have its cause in the pressure exercised by the diseased lung upon the pulmonary artery, and its first branches. In both the latter cases, the condensed lung, having become a better conductor of sound, conveys a murmur to the ear which would probably be inaudible if the lungs were healthy. M. Gendrin‡ has called attention to the circumstance that, in the cold stage of intermittent fever, a bruit de soufflet, analogous to that heard in cases of chlorosis, is audible on auscultation of the heart. As individuals who have been the subject of ague for any length of time are, however, in general more or less anæmic, the murmur is probably due to this cause.

Conditions under which bruit de soufflet is developed.—Whenever bruit de soufflet is heard in the heart, it arises either from contraction or some other diseased state of the orifices or valves of the heart, which impedes the free passage of the blood; or from a condition of the valves which prevents them from closing the orifice, and permits regurgitation; or from the blood being impelled with increased force and velocity through the aortic orifice, or through an abnormal orifice, as in aneurism of the left ventricle, and congenital malformations of the heart; or from some alteration in the quality of the blood itself, or in its quantity; or from the formation of fibrinous or other concretions in the cavities of the heart, which interrupt the play

of the valves, or impede the orifices; or where morbid growths are developed in the cavity of the chest, which compress or displace the heart; or where the bony walls of the thorax are deformed, and the cavity is much narrowed.

Whenever bruit de soufflet is audible in the arteries, it arises either from roughness of the lining membrane of the vessel; or from the calibre of the artery being diminished, owing to pressure upon the part; or from an altered condition of the blood; or from an aneurismal sac springing from a large artery; or where an abnormal communication exists between a large artery and a vein; or where an aneurismal sac is seated between an artery and a vein, through which the blood passes from the one to the other.

Bruit de soufflet is therefore a symptom in endocarditis, where the aortic or mitral valves are swollen, thickened, or indurated; or where vegetations form upon them, or upon the orifices; as well as in the chronic forms of disease of the valves or orifices, accompanied by contraction of the orifices, by ossific or other deposit upon the valves, or adhesion of the valves to one another or to the parietes. It will likewise be heard in cases where rupture of a valve or of a tendinous cord takes place, as well as in cases of congenital malformation of the valves, one of the most frequent of which is a cribriform condition of their curtains. Bruit de soufflet is likewise a symptom in dilatation of the orifices of the left side of the heart, without any disease of the valves, or where they are prevented by any other cause from perfectly fulfilling their functions. It is a symptom in aneurism of the left ventricle; in congenital malformations of the heart; in cases of morbid growths in the cavity of the thorax; in aneurism of all the large arteries; in aneurismal varix, and varicose aneurism; in disease of the lining membrane of the arch of the aorta; and, finally, in an anæmic state of the system whatever be its cause.

Mechanism of production of bruit de soufflet.—Laennec attributed bruit de soufflet to spasm, or to a vibratory action of the heart; Bertin, to the increased friction produced by the passage of the blood through a contracted orifice. Other physiologists have endeavoured to explain it by referring it to increased velocity of the circulation, or to very energetic action of the heart, or to "a certain resistance given to the blood moving with a certain force." Others have referred it to modifications or alterations in the tissue or structure of the parietes of the heart, or to morbid conditions of the blood itself. M. Bouilland§ is

* Lectures on Diseases of the Heart, vol. I.

† Die Herzkrankheiten, 1844.

‡ Leçons sur les Maladies du Cœur, tome I.

* Archives Gén. de Médecine, tome XI.

of opinion that bruit de soufflet may be produced under three conditions—viz. increased action of the heart; narrowing of any portion of the canal through which the blood passes; or roughness of the surface over which the blood flows. Dr. Corrigan,* who has written at length upon this subject, and who made several experiments with the object of determining the question, arrived at the following conclusions respecting the conditions necessary for the production of this sound.

“1st. A current-like motion of the blood (instead of its natural equable movement) tending to produce corresponding vibrations in the sides of the cavities or arteries through which it is moving;” and

“2nd. A diminished tension of the parietes of the arteries or cavities themselves, in consequence of which their parietes are easily thrown into vibrations by the irregular current of the contained fluid; which vibrations cause, on the sense of touch, *frémissement*,—and, on the sense of hearing, *bruit de soufflet*.”

In every instance in which bruit de soufflet is audible, it is clear that some physical agent, capable of producing it, must be in operation: now the only physical agent that is invariably in operation, and under every variety of circumstances where bruit de soufflet is heard, is increased friction between the blood and the parts along or through which it passes; which appears to be amply sufficient, not only to develop murmurs, but to convert the normal sounds of the heart into them. Thus:

1. When the orifice of the aorta is contracted, or its valves are diseased, so as to impede the direct passage of the blood, there must necessarily be increase of friction; and the normal first sound of the heart will be converted into a murmur.

2. When the mitral valve is diseased, and permits a reflux current of blood into the left auricle at each ventricular systole, a murmur will be developed, which, from its louder tone, will obscure or render inaudible the first sound of the heart.

3. When the simular valves of the aorta imperfectly close the orifice, the blood must regurgitate from the aorta into the left ventricle at each ventricular diastole; and that this occurs with sufficient force to generate a murmur, which obscures or renders inaudible the normal second sound of the heart, is well known.

4. When the left auriculo-ventricular orifice becomes dilated, and the valve cannot, from this cause, fulfil its function perfectly, regurgitation of the blood occurs at each ventricular systole, and a bruit de

soufflet is developed, which is usually sufficiently strong to obscure the normal first sound of the heart.

5. When the blood is propelled with greatly augmented force, and with increased velocity, through the aortic orifice, the amount of friction between the blood and the parietes of this orifice must be much increased; and if it is sufficiently strong to generate a murmur, this will have the characters of *bruit de soufflet*, and will take the place of the normal first sound of the heart.

6. When the lining membrane of the ascending portion of the arch of the aorta loses its natural smoothness and polish, the friction between the blood and the part along which it passes must be increased; and, if the heart's action is strong, a murmur will be developed, which will be synchronous with the first sound of the heart, and will often be audible in the large vessels which come off from the arch of the aorta.

7. When the viscosity of the blood is diminished, and its watery parts are increased, a greater amount of friction will take place between this fluid and the orifice of the aorta, in its passage out of the ventricle, as well as in the large arteries, which is usually quite sufficient to convert the normal first sound of the heart into a murmur.

8. When the blood is propelled into an abnormal cavity, or through a preternatural orifice, and the action of the left ventricle is strong, the friction will be sufficient to generate a murmur which will obscure the first sound of the heart.

9. When the large arteries in the cavity of the thorax are compressed by the growth of a tumor within, or by deformity of the bony cavities from disease without, the diminution of their calibre is often sufficient to generate a murmur, which will have the character of *bruit de soufflet*, and will be heard at the period of the ventricular systole.

10. When lymph is deposited about the valves or orifices of the heart in sufficient quantity to prevent the action of the valves, or to obstruct the passage of the blood, there will necessarily be increased friction, usually sufficient to develop a murmur which may replace either the first or the second sound of the heart, or both.

11. When the fibrine separates, in the cavities of the heart, from the other constituents of the blood, and obstructs the orifices or impedes the action of the valves, if the action of the heart was strong sufficient friction would be produced to develop a murmur; but as this scarcely ever occurs, except during the last few

* *Dub. Jour. of Med. Science*, vols. x. and xiv.

hours or days of life, the current of blood is generally too feeble to develop a murmur.

12. The bruit de soufflet heard after the fourth month of pregnancy, which is not audible until the uterus has risen out of the pelvis, and which is generally known under the name of the "placental soufflet," appears to have its cause in the pressure exercised by the enlarged uterus upon the iliac arteries. Because it always accompanies the ventricular systole, it is audible usually only in one iliac fossa: and an exactly similar murmur has been heard by Dr. Montgomery* in cases where the uterus itself was enlarged from disease, or where an abdominal tumor compressed the aorta. In addition, M. Bouillaud has found that, by changing the position of the patient, the site of the murmur is altered; and Dr. Cowan† observes that the bruit de soufflet in these cases "can be detected in the femoral arteries immediately below the arch," and that "the sound may, by change of position, be transferred from one femoral artery to the other, always corresponding with the side of the uterine murmur."

In the majority of the foregoing examples the bruit de soufflet is heard at the period of the ventricular systole, and replaces or accompanies the first sound of the heart: the force with which the blood is propelled by the left ventricle being much greater than that with which it enters it, the friction between the blood and the parts along or through which it passes must be much more considerable in the former than the latter. In the former it is generally sufficient to generate a murmur, in the latter it only occasionally does so; and, when a murmur is developed, it has a different character. For instance, when the mitral valve or orifice is diseased, so as to permit regurgitation, a bruit de soufflet will be heard at the period of the ventricular systole, which quite obscures the normal first sound of the heart; when the aortic valves permit regurgitation, a bruit de soufflet is audible at the period of the ventricular diastole, which likewise obscures the normal second sound of the heart. But the two murmurs are very different: that which accompanies mitral regurgitation is loud, strong, and blowing; that which accompanies aortic regurgitation is soft, feeble, and whispering, because the force with which the blood is propelled out of the left ventricle is so much greater than that with which it enters the same ventricle.

Two or more of the conditions described above are not unfrequently combined in

the same subject; and we may have a murmur taking the place of, or obscuring both sounds of the heart; or the bruit de soufflet may pass into a sawing, rasping, or musical murmur; or, finally, the valvular disease may be complicated with an anæmic state of the system, by which the difficulty of the diagnosis is sometimes considerably increased.

Bruit de soufflet at the right side of the heart.—The foregoing remarks have reference, more particularly, to bruit de soufflet developed at the left side of the heart: in fact, this abnormal sound is very rare at the right side of the organ, because the force with which the blood enters or is expelled from the right ventricle is generally too feeble to generate a murmur; while valvular disease at this side of the heart is uncommon. Dr. Clendinning* has shown, from statistics in 100 cases, that valvular disease is sixteen times more frequent at the left than at the right side of the heart. When we consider, too, that in cases of anæmia the murmur is limited to the left side, and that murmurs from regurgitation are never, or scarcely ever, produced at the right side of the heart, we can understand why bruit de soufflet should be so rare at this side. For instance, mitral regurgitation is one of the most frequent causes of bruit de soufflet at the left side of the heart: on the other hand, tricuspid regurgitation is even more frequent; but it is not accompanied by any morbid sound, and is usually recognised by another sign,—viz. jugular pulsation. Again, aortic regurgitation is a frequent cause of a murmur at the left side of the heart, but a very uncommon cause of a murmur at the pulmonary orifice. "I believe (Dr. Hope† observes) diastolic murmur of the pulmonic valves to be exceedingly rare from disease of the valves themselves, as I have never met with a case, or been able to find one recorded." "From a rude numerical calculation (he adds) deduced from the cases that I have seen, I should think that there would be at least thirty chances to one against a murmur connected with the semilunar valves being seated in the pulmonic set."

Although the second sound at the right side of the heart is scarcely ever converted into a murmur, owing to regurgitation through the sigmoid valves of the pulmonary artery, this sound is sometimes altered, augmented, or rendered sharper, owing to hypertrophy with dilatation of the right ventricle, consequent upon disease of the mitral valve, as was first pointed out by

* Cycloped. of Pract. Med., art. Pregnancy.
† MEDICAL GAZETTE, vol. xvii. 1836.

* MEDICAL GAZETTE.
† Treatise on Diseases of the Heart, 4th edit.

Skoda and Zehetmayer.* According to Skoda, when the left auriculo-ventricular valve permits regurgitation, the second sound at the pulmonary orifice becomes more intense, or sharper; which he explains by the impeded circulation through the lungs reacting upon the right side of the heart, in consequence of which the blood in the distended pulmonary artery reacts with unusual force upon the sigmoid valves of this vessel. He is even of opinion that, from this augmentation of the second sound at the pulmonary orifice, we might diagnose disease of the mitral valve or orifice previous to any abnormal sound being heard at it. Zehetmayer considers that this phenomenon is never absent in cases of mitral regurgitation, and that the diagnosis of the latter cannot be positively made unless it is likewise perceived.

A murmur at the pulmonary orifice, or in the pulmonary artery, accompanying the ventricular systole and the first sound, is less rare than any murmur at the right side of the heart. It seldom, however, depends upon disease of the sigmoid valves; in general it arises from the presence of a tumor, or from a diseased lung compressing the pulmonary artery. Dr. Hope relates a case in which an aneurism at the origin of the aorta compressed the pulmonary orifice, and contributed to produce a systolic murmur; and another, where extensive ossification of the pulmonary artery gave rise likewise to one. Dr. Elliotson met with two cases where portions of cartilage in the pericardium pressed upon and contracted the pulmonary artery so as to produce a murmur. The other circumstances under which a bruit de soufflet is developed in the pulmonary artery or its branches, owing to disease of the lungs, have been already mentioned.

Conclusions from the presence of bruit de soufflet.

The value of bruit de soufflet as a sign of disease of the valves or orifices of the heart, depends—

- 1st. Upon the period of the heart's action at which it occurs.
- 2d. Upon the situation at which it is best marked.
- 3d. Upon its character or tone.
- 4th. Upon its being constantly present or not.

In organic disease of the valves and orifices of the heart, bruit de soufflet is in a great measure limited to the left side of the organ, and has its seat either at the aortic or mitral orifice. When it replaces the first sound of the heart, it depends, in almost all cases, either upon obstructive

disease of the aortic orifice, or regurgitant disease of the mitral orifice. When it occurs at the period of the ventricular diastole, and of the second sound of the heart, it almost always has its cause in a state of the aortic valves permitting regurgitation. When bruit de soufflet accompanies both the systole and the diastole of the ventricles, it may have its cause either in obstructive disease of the aortic orifice, combined with a state of its valves permitting regurgitation; or in the mitral and aortic valves, both permitting regurgitation.

The first sound of the heart is not unfrequently replaced by bruit de soufflet when no disease of the valves or orifices exists; but a murmur is never heard at the period of the second sound of the heart, unless in cases of disease. A murmur at the right side of the heart is, as has already been observed, extremely rare; and, when it is heard, it will be found to depend more frequently upon disease of other parts than of the valves or orifices of the right ventricle.

The situation in which murmurs developed at each orifice of the heart are best heard, together with the other characters belonging to them, will be most conveniently considered when I come to describe the symptoms belonging to diseased states of the individual valves.

CASE OF CHRONIC GLANDERS IN MAN.

A MAN, aged twenty-three years, had the charge of three glandered horses for six months. Two or three months subsequently he began to suffer from stuffing of the nostrils, and discharge of thick yellow matter, with tightness of breathing. About ten months later, a small tumor appeared on the arm over the insertion of the deltoid; it continued to increase in size for three months, burst spontaneously, and was healed in about three months afterwards. At this time the discharge from the nose, which had diminished, again increased, and a tumor, of the size of a small pigeon's egg, formed over the orifice of the left lachrymal sac, burst, and left a fistulous orifice. The same then occurred on the right side.

The disease was considered to be of a strumous nature, and iodide of potassium was given; but its progress was uninfluenced. Portions of the nasal bones were exfoliated, and the patient became emaciated. In 1849, he was admitted into the hospital of Louvain. The disease continued its course, and the patient shortly sank from fever, diarrhoea, &c. &c.—*La Presse Médicale.*

X

* Die Herzkrankheiten.

Original Communications.

ON THE CONTAINING-TEXTURE
OF THE BLOOD.

BY WILLIAM ADDISON, M.D. F.R.S. F.L.S.

[Continued from page 196.]

THE principal events demanding consideration in repair and inflammation are,—1. The accumulation of lymph or cell-particles, and, 2. The manner in which the coats of the existing blood-vessels are altered, so that blood, flowing in a multitude of new vessels, establishes cell-growth without effusion or loss of its red particles. The latter phenomenon might occur in two supposable ways—either by extension of the coats of the vessels through the newly appearing lymph-bed, or by openings effected in them becoming continuous with the new vessels. What are the facts bearing upon these alternatives?

We have seen school-boys wind a piece of twine tightly around the finger, forcing the blood with great pressure into its extremity,—they have allowed the string to remain on for some time, and, upon its removal, there did not follow any effusion or subcutaneous hæmorrhage. A very vascular nævus may be firmly pressed so as to drive almost all the blood out of its vessels, upon the return of which none of them are found to have been ruptured. The new bloodvessels traversing a lymph or granulation structure, would not for an instant bear the 1000th part of any such treatment without rupture and obliteration. Moreover, we can seldom succeed in injecting the new vessels of very recent lymph, on account of their softness or incoherency. These facts seem incompatible with the supposition that the new vessels which appear in repair or inflammation are formed by an extension of the coats of the pre-existing ones, and, in the course of our researches, we have seen nothing to lead to such a conclusion; on the contrary, the coats of newly formed vessels have always appeared altogether different from those of the older ones. The former are pulpy, soft, inelastic, and composed of colourless cells; the latter tough, elastic, and fibrous. And not

only is this the case, but it has moreover appeared, so far from the new vessels partaking of the nature of the old, *that the old become assimilated to the conditions of the new.* But let us examine, first, respecting the accumulation of the lymph particles.

The prevailing doctrine with respect to effusion of lymph and accumulation of lymph particles appears to be this:—that effusion of lymph takes place by a species of transudation or exosmosis through the coats of the vessels, and that lymph particles arise from “germs” in the fluid, which grow first as “nuclei,” and then as “cells,” these being developed “as in a blastema.” This account explains nothing as regards the principal and second-mentioned topic of the investigation, inasmuch as nothing is affirmed respecting any change in the coats of the vessels,—a change that must of necessity take place for blood to flow into new channels. There can be no doubt whatever that the fluid element of blood under various circumstances transudes the coats of the vessels,—*simple effusion*,—the coats of the vessels remaining, their elements continuing, their form unchanged. Nor do we assume to deny the *possibility* of cell particles being developed in the effused fluid “as in a blastema.” But we contend that the fact has never been proved, resting only on assumption. On the other hand, the evidence of the senses is not to be repudiated, to the effect that, after irritation, lymph, and lymph particles from the blood, accumulate on the *inside* of the irritated vessels. Having examined the changes produced in the vessels of the *conjunctiva* in purulent ophthalmia, we have found their coats altered, swollen, thickened, and made pulpy,—brought nearly to the condition of embryoniform vessels by the interposition of cell particles among the natural fibrous elements, and these have extended from the innermost boundary of the vessels where they touched the stream of blood to their outermost border.* A change of the same kind occurs in the vessels of the gums when they become soft and spongy; and in ulceration where vessels bleed upon the slightest touch, the blood which escapes containing an unusual abundance of colourless cell-particles.

* Healthy and Diseased Structure, plate 3, figs. 6 and 7.

It is not necessary, however, to the prosecution of our present purpose, to determine the question, whether, and to what extent, lymph particles are generated, as in a blastema, in the effused fluid *outside* the vessels; or whether, and to what extent, they accumulate *on their inside* by separation from the blood within. We hold to our conclusions upon this point; and the facts upon which they are based have elsewhere been discussed. It is sufficient that it be granted that cell particles surround and invest the coats of established vessels before new vessels appear.

The inquiry proposed, then, is with respect to the agents which effect the alterations in the older vessels necessary for the establishment of new vascular lymph-growth, and to the opening out of communications between coherent and tough-walled vessels (those of areolar tissue, for example), and the soft, inelastic new ones (those of repair and inflammation, of lymph foci and granulations). The evidence required in the investigation has already, to a great extent, been made known.

In the present state of opinion, however, we begin with two preliminary propositions, which may be safely affirmed—1. That new blood-vessels, traversing freshly-accumulated lymph, can make their appearance only after some change (or opening) has been made in the coats of the pre-established vessels; 2. That wherever lymph-particles are accumulated, there some species of vital action, of growth, nutrition, secretion, or absorption, is going on.

In the first period of repair, the action is from blood to the texture: there is effusion, lymph-particles accumulate, new blood-vessels and embryoniform growths appear. There is *deposition* and swelling. In the second stage of repair, the continued accumulation of lymph-particles ceases, cell-growth is arrested, a great many of the new vessels disappear, fibrous consolidation ensues, and redundant material is removed. There is *absorption*, and swelling subsides. Here are not simply two stages or two periods of one thing, but two distinct things. At first there is clearly an assemblage of matter and forces incompatible with the welfare of established forms, which, considered in themselves, without reference to what may ultimately appear, constitute no

reparative process at all, quite the contrary: the action observed during the first four or six days, of a process of repair, by *simple continuation*, becomes morbid. The truly reparative part of the process commences, not whilst lymph-growth is in progress, but when it ceases; when the specific type, the fibrous form, of the containing texture of the blood appears. Such being the case, the first period of repair giving birth to unnatural growth, is morbid; and therefore, whatever may be concluded from our inquiry, applies not only to the first period of repair, but also to inflammation. Forasmuch as there does not appear any necessity for assuming a difference in the forces in operation, or in the agents employed in the one and in the other.

Now, *deposition* and *absorption*, growth and destruction, cannot be supposed in progress together, at the same spot, and at the same time, if we speak of the same matter or the same thing; but with respect to two distinct things, there are numerous facts in the economy of the living body—physiological and pathological,—proving that these two actions may be and are concurrent. The alimentary mucous membrane is both an absorbing and an excreting surface—so is the skin. There are times when the elements of food are passing into the circulation from the intestine, concurrently with elements of secretion which are passing out from it. The influence of cantharides, from a blister-plaster, is frequently exercised through the medium of absorption, upon the urinary organs, at the time when there is a copious discharge or effusion from the vessels of the absorbing surface. One species of matter deposited or accumulating, another absorbed or taken away. Many other medicinal substances are well known to be absorbed and carried away in the circulating fluid under similar conditions. The substance of a necrosed bone is in part absorbed and taken away whilst new bone is forming. In the lung, blood absorbs from the air elements which renovate its properties, and at the same time gives off a vaporous exhalation, incompatible with its arterial character and constitution. All these we conceive to be examples of the concurrence of absorption and deposition.

But the phenomena which come

nearest to those, the nature and agents of which we would fain discover, are comprehended under the terms abscess and ulceration. In an abscess there is plainly absorption of the surrounding tissues at the time pus is accumulating; and in ulcers the pre-established textures disappear, whilst discharges are flowing away. In an abscess there is tumor or swelling, not because deposition or the generation of cell-forms is the sole phenomenon, but because the new matter being confined, its accumulation forms the most prominent appearance; in ulceration the new matter is not confined, but being too incoherent to retain a form, it falls away and is discharged, whereupon the loss and disintegration of the original texture becomes the most striking effect. The appearances, therefore, are widely different; but in both the same two physiological actions are concurrent, cell-particles accumulating, and pre-established forms disappearing. When a deep abscess makes its way to the surface and bursts, not only the skin, but multitudes of the blood-vessels of the skin, become gradually thinner and thinner, until at length their continuity is broken without hæmorrhage. Such a phenomenon as this cannot be referred to the action of absorbent vessels brought into play by the influence of pressure, for this must be equal on all sides of the abscess, whereas absorption of established textures, and obliteration of blood-vessels, is determined chiefly in one direction, that which is shortest to the nearest surface and safest for the patient. Again, when ulceration is destroying the natural textures, it will often sever blood-vessels of considerable size without any bleeding. Loss of blood from ulceration is the exception, and not the rule. Here, then, we have more than a simple concurrence of deposition with absorption; for a provident lymph-growth closes and heals the ends of the dis severed vessels in the midst of an otherwise destructive process. These phenomena bear the stamp of cell-agency; and, as each cell, though a microscopic, is nevertheless a whole or individual organism, so mixed changes occur—deposition and absorption—within microscopic areas, which, by the congregation of cells, are effected with greater celerity. The inference here seems borne out by the fact, that in chronic scrofulous abscess, where ab-

sorption or thinning away of the superjacent integument comes to a stand, the cell-particles it contains are found broken, collapsed, irregular in figure, and evidently effete “unhealthy pus:” whereas in an acute abscess, where the process of absorption of pre-established structure is almost as active as the deposition of the new material, the cell-particles are round, plump, and unruptured, constituting “laudable or healthy pus”—a matter clearly only a short way removed from lymph, and this more by the greater abundance of cell-particles than by any other feature of difference. Arguing from these facts, the accumulation of cell-forms is no bar to the absorption of pre-established textures. On the contrary, the essential phenomenon of inflammation appears to be the appearance of cell-particles, upon and surrounding the coats of the vessels, which displace the fibrous elements. And, in the absence of any more direct proof upon the subject, we must conclude that, where new blood-vessels are about to appear, the coats of the old ones are previously altered, their fibrous elements absorbed or removed; so that they come to partake, and at length to be constituted of, the elements of the lymph-material in which the new vessels will appear; these replacing elements being the accumulated lymph-particles,—and this whether the accumulation has accrued *outside* the vessels by generation, “as in a blastema,” or *inside* by deposition from the blood, or partly by the one process, and partly by the other. By these operations—accumulation of cell-forms and absorption of fibrous-forms—blood-vessels are prepared for the formation of new vascular tissue.

And it is to this change in blood-vessels, when their fibrous coats are infiltrated, and more or less completely supplanted by cell-particles, that we have applied the term *retrograde metamorphosis*, from its analogy to certain changes and transformations well known to prevail in vegetable structure. The containing-texture of the blood, prior to the appearance of new vascular growth, truly receding from its specific or concluded form or quality to its primitive or embryoniform condition; whereupon new blood-vessels form, subject to the same laws and provisions as prevail in the embryo-mass originally. And the readiness or facility

with which this retrogradation occurs, and the speed with which new vessels appear, would seem to constitute no mean part of the evidence of our conclusion, *that blood forms its own containing-texture, bloodvessels being correlatives of the colourless elements of blood.* In the first period of repair we regard this (the inflammatory process) with reference to the second period, when the containing-texture of the blood reassumes its normal form and characters unaided by medical art; but in morbid inflammation we anxiously desire to stop it, from the uncertainty of the extent to which it may go.

Upon this interpretation we have evidence of *two kinds of deposition*, and *two kinds of absorption*; the one characteristic of the first period of repair, the other of the second period of repair, or of the cure of inflammation. In the first, cells accumulate, and the specific form of the containing-texture of the blood disappears: new blood-vessels and new cell-growths arising. In the second, cell forms, cell-growths, and most of the bloodvessels administering to them, disappear, are absorbed or abolished, the specific form of the containing texture of the blood at the same time being re-deposited or restored. Thus we have the clearest proof that can be furnished of antagonistic forces. The welfare or permanence of embryoniform cell-growth excludes the normal fibrous structural type of the containing-texture of the blood:—the welfare or establishment of this, on the other hand, excludes the cell-growth: that is to say, the luxuriance of adventitious cell-growth is destructive of natural forms, the reappearance of natural forms destructive of such cell-growth.

The first period of repair (the establishment of cell or embryoniform growth), is necessary to the second, (the restoration or reappearance of the fibrous type),—in conformity with the laws of original growth; but can be considered as a healing operation only in the same sense that the embryostucture, which may be obliterated by a touch, is a chicken or a man. Results, in all these cases, are coming forward, which at length establish a concluded type. In the meantime, we cannot define the perfect by the qualities of the incomplete, and say that the first period of repair is a healing operation.

It appears, then, that at all periods

of life blood may be determined in unusual quantity to any part of the body upon irritation applied to its containing-texture. This, if the bloodvessels undergo no essential change, is simple congestion, or, to use the technical phrase, *hyperæmia*: it becomes inflammation when cell-forms accumulate, and the coats of the vessels are losing their concluded form. Under various circumstances effusions also take place: these are simple if the blood vessels maintain their normal constitution; but inflammatory, if cell-forms are supplanting fibrous elements.*

Inflammation and organisation are accepted terms, and they comprehend two distinct classes of facts. We refer organisation to an antecedent power or force, and call it "germ-force," "organic life," &c. But, with respect to inflammation, we are not in the same logical position; for we use the word sometimes in the sense of an agent; at others, as merely expressing the phenomena. Thus, we are apt to say that inflammation does this or that, and also that it consists in this or that. Now it is perfectly allowable, in a philosophical discussion of the subject, if we refer organisation to an antecedent force, to do the same for that class of phenomena comprehended under the term inflammation.†

But we confine ourselves to a simple statement of the phenomena. The elements of blood, under normal circumstances, are subject to the forces which are operative in organisation and natural growth; but in inflammation, or the first period of repair, they emerge from this subserviency, and establish embryonic, and therefore retrograde forms of growth. These, in a process of repair, are limited in extent and continuance, conforming in due time to the laws of natural growth: but, in inflammatory and scrofulous disease, they spread and are persistent, prevailing over the forces which govern and sustain the concluded type, or the fibrous form of the containing-texture of the blood.

In inflammation, abortive repair, and scrofulous disease, there is ample room for speculation whether the inherent organising force is too weak or inade-

* Illustrative facts are detailed in our work *On Healthy and Diseased Structure*, p. 284.

† Upon this point see Whewell "On the Inductive Sciences," quoted p. 90, *supra*.

quate in some absolute sense, or relatively only to an increased energy on the part of the elements of blood. This is the question substantially at issue when we speak of asthenic inflammation, poverty of blood, and scrofulous diathesis. Chemical investigations here come in aid; but we think it must be allowed that variations in the constituents of blood and the secretions, within the limits of health, are too wide for chemical analysis yet to be our guide in practical medicine.

As regards the term retrograde metamorphosis.—In the “scale of organisation,” animal bodies which have bones rank higher than those with only fibrous textures, and these higher than the corpuscular or cell-textures, which are esteemed the lowest. But the elements of the lowest forms of organisation—viz., cells—rank higher in the *scale of vitality* than any element of fibrous, tendinous, or osseous tissue. Every cell is a whole organism; whereas bones, tendons, fibrous textures, and blood-vessels, can be parts only of a whole. In the highest, and in all the intermediate forms of living beings between the lowest and the highest, the particular substance of the secreting organs—the liver, kidneys, &c., the most energetic portions of the brain and of the blood—consist of cell-organisms; and it is evident, from all the facts of the case, that a broad distinction must be preserved between elements of *form* and elements of *vital action*. The former have the lowest—the latter the highest vitality. Bones, fibrous-textures, skin, and vascular-tissue, clearly “degenerate” when they become supplanted by particles of lymph or pus, inasmuch as their form and organisation are lost. But, on the other hand, the replacing matter, soft and incoherent though it is, can be said to be “degraded” only in a certain sense; for every corpuscle *which is effective* in the lymph or pus has, far more “vital energy” than any portion of the ministerial textures they supplant. Hence the term retrograde metamorphosis refers, not to vital properties, but to condition or form. And be it remembered, that “life is made manifest to us, not by form, but by acts. It cannot, therefore, be considered as an attribute of uniform character or unchanging intensity.” And, judging of the intensity of life by the prominence

of the effects, there is far more of life in an acute abscess or ulcer than in the natural structures they supplant, but it is not of the kind or co-ordinated to the species required. The gradual accumulation of pus in spite of many opposing forces of a physical kind—its determination, as it were, to push aside and thin away opposing obstacles—are phenomena of life: and we witness here an exhibition of forces analogous to those which enable the tender plumule of the plant to upturn a giant clod of earth. The organised textures reproducible by the process of repair, and which appear in the products of a cured inflammation or scrofulous disease, hold no very high place in the acts of life. Bones are merely crutches for support, and are notoriously modelled and channelled by the softer tissues; tendons are simply cords attaching the motor-tissue to the bones; and vascular-tissue is subordinate in natural, and even in unnatural growth, to the particular substance—the parenchyma of the organ. For if the brain-substance be malformed or deficient, its vascular-tissue and its bones are generally malformed and deficient likewise; and, in monstrous growth, if brain-substance be unnaturally evolved, exceeding the specific bulk, its vascular-tissue is unnaturally expanded also; and, moreover, the bones of the skull are abnormally expanded, and their number, too, is much increased.

Thus we return to the high significance of embryological researches, where sentient and motor forms are the first moulded; and being so, blood appears. After these follow the containing-texture of the blood; and, lastly, tendinous and osseous structures;—all in subservience to the leading forms, and these last again subservient to the “power” inherent in the “germ.” If this be crippled in the evolution of sentient-matter, vascular-tissue, tendons, and bones, conform to the deficiency. On the other hand, if the nervous centre luxuriate in growth, vascular-tissue and bone luxuriate with it. If the “germ-power” be so much blighted that the leading forms (spinal cord and cerebral ganglia) be not moulded, blood does not appear. Blood not appearing, there can be no vascular tissue; and without this there can be neither tendons nor bone.

It may be objected to the conclusion that bone is a metamorphosed form of

vascular-tissue, that if either a wing or a leg of the embryo-chick, when first budding forth on the fifth day, or either of the extremities of the human embryo, when these are not much larger than a mustard-seed, be gently compressed between two slips of glass, and examined with a microscope, all the bones will be seen symmetrically arranged and moulded to their proper shape before any blood or blood-vessels can be seen near them: and, in our figs. i. and ii., p. 194, the dorsal plates are shown as existing at the fortieth and fiftieth hour of incubation. These facts, which might be supposed to militate against, do in truth serve to establish our conclusions. For the substance thus shaped and moulded is a *germ-mass*, entirely composed of soft corpuscles or cells. It is not bone: there is nothing hard—nothing resembling bone. Bone-substance does not appear until some time after this primary form has been permeated in all directions by blood and vascular tissue. So that here, though the form and outline of the future member is cast before blood circulates through it, still *ossification* does not commence except through the agency of the containing-texture of the blood.

FATAL HÆMORRHAGE FROM THE UMBILICUS IN NEW-BORN CHILDREN.

In two cases observed by Dr. Bowditch the children died of hemorrhage from the navel (on the twentieth and fourteenth days after birth). They were of both sexes; while males, according to authors upon the hemorrhagic tendency, are more liable than females. In both cases the cord came off quite well, and without external injury; one on the third, the other on the fifth day after the hemorrhage commenced. In both, the parts seemed entirely cured, and presented no discharge for several days before the fatal oozing took place. In both, all local applications seemed, in a very short time, to excite rather than to check the disease. In both, very violent surgical operations were resorted to without the least benefit. Death took place in one on the third, in the other on the sixth day after attack. One had white dejections from birth; both had purpuric extravasations and bloody dejections at last. At the autopsy of one, disease of the liver and non-coagulated blood were found; no opening or apparent disease about the navel or vessels leading thereto was found.—*American Journal of Medical Sciences*, Jan. 1850.

ON NARCOTISM BY THE INHALATION OF VAPOURS.

BY JOHN SNOW, M.D.

[Continued from last vol. p. 627.]

PART XIV.

Chloroform passes off unchanged from the blood, in the expired air—its detection in the urine—in the dead body—in an amputated limb—Remarks on the process for its detection.

At the end of the last paper, reasons were given for concluding that the effects of narcotic vapours were not due, as some had supposed, to the hydrogen and carbon they contain, combining with the oxygen of the air dissolved in the blood; and evidence was adduced to show that if such combination do take place, this would not explain their narcotic action. It still remained desirable to determine by experiment, if possible, whether these bodies are decomposed in the system, or pass off unchanged in the breath, or in other ways. With this view the following experiment was performed:—

Exp. 54.—Ten minims of chloroform were put into a hydrogen balloon, holding 300 cubic inches. The balloon was filled up with air, which I breathed backwards and forwards, in the way in which nitrous oxide gas is taken, for probably about two minutes. The word probably is used, because, after observing the watch for a minute and a half, I lost the recollection of what I was doing, and on recovering so as to observe the watch again, I found that another minute had elapsed, and that I had carefully lain aside the balloon in the meantime. Half a minute after this, and three minutes after beginning to inhale, I commenced to pass the expired air through a tube of hard glass, which was placed in readiness in a charcoal fire. To the further end of the tube were fitted other tubes connecting it with two Woulfe's bottles, each containing a solution of nitrate of silver. The expired air was taken in by the nostrils and breathed out by the mouth, passing first through the red hot tube, and afterwards through the solutions of nitrate of silver. This process was continued for four minutes.

The solution was rendered turbid, more especially that in the first bottle; being at first white, but shortly afterwards of a dark violet colour. At the end of twenty-five minutes from the inhalation, and when scarcely any appreciable effect of the chloroform remained on the feelings, I again breathed the expired air through the red-hot tube, the Woolfe's bottles having been removed, and a small tube moistened inside with solution of nitrate of silver having been attached. A slight precipitate of chloride of silver immediately appeared in the tube. The precipitate in the Woolfe's bottles having been washed and dried on the filter, was found to weigh 1·2 grain.

I have on other occasions, after inhaling chloroform, made the expired air to pass at once through a solution of nitrate of silver without the intervention of the red-hot tube, when not the least precipitate was occasioned; consequently, the chlorine which combined with the silver in the above experiment was the result of the decomposition of chloroform in the hot tube, and not in the circulation. As upwards of half a minute was allowed to elapse, during which several inspirations were taken between the conclusion of the inhalation and commencing to breathe through the tube, the lungs must have been completely emptied of the air taken from the balloon, and the vapour of chloroform must consequently have been exhaled from the blood. The further part of the experiment, performed twenty minutes later, more strongly proves this, and also shows that chloroform continues to be exhaled as long as any appreciable effects of it remain.

If all the chlorine of the chloroform united with the silver, the quantity of chloride obtained in four minutes, in the above experiment—viz. 1·2 grain, would indicate only 0·476 grain of chloroform. But I have found that on passing the vapour of a known quantity of chloroform through a red-hot tube, only about one-third of the chlorine is liberated, chiefly in the form of hydrochloric acid gas, and combines with the silver, as will be more fully explained further on: consequently, the above quantity of chloride of silver may be taken to indicate 1·428, or nearly a grain and a half of chloroform. It would not be easy to continue to test

for the whole of the vapour exhaled by the breath. Indeed, breathing through the tubes and liquids for four minutes, in the above experiment, was attended with some inconvenience. But when it is considered that part of the chloroform used must have remained in the balloon, that a further part must have been exhaled before beginning to breathe through the red-hot tube, and that the vapour was still being exhaled twenty-five minutes after the inhalation, the experiment must help to confirm the view that by far the greater part of the chloroform inhaled is exhaled again by the breath.

It is probable that a small portion of chloroform passes out by other channels than that of the expired air: the latter, however, offers such a ready and expeditious outlet, that the quantity excreted in any other way is, most likely, very minute. I have on four occasions examined urine passed after the inhalation of chloroform, by boiling it in a flask, and passing the vapour, first through a red-hot tube, and afterwards through a tube moistened inside with solution of nitrate of silver, and I only on one occasion obtained a very slight precipitate of chloride of silver.

The presence of chloroform can be detected in portions of the body removed by the surgeon, when the patient is under its influence, and in the bodies of animals killed by it. And as this part of the subject is interesting in a medico-legal as well as in a physiological point of view, I shall enter a little more minutely into the account of it than I might otherwise have done. In the *Journal de Chimie Médicale* for March, 1849, a process for the detection of chloroform in the blood is described in the following terms:—"In order to recognise the presence of chloroform in the blood, we take advantage of the property which this body possesses of being decomposed at a red heat, in giving rise to chlorine and hydrochloric acid. In order to perform the operation, it is sufficient to boil an ounce of blood for some time in a glass flask over the water bath. The vapour must pass through a tube heated to redness at one part, and of which the extremity is smeared interiorly with a mixture of iodide of potassium and paste of starch. A strip of paper moistened with the same mixture may also be put into the tube. If any chlorine be produced by

the decomposition of chloroform, the strip of paper will be turned blue. In this way one part of chloroform in 10,000 of blood may be discovered." It is not stated in this article whether the chloroform detected had entered the blood during life, or had been added after its removal, though the former was probably meant.

In employing this process I substituted solution of nitrate of silver for the starch and iodine test, considering that to obtain some of the chlorine as chloride of silver would be more satisfactory, in a medico-legal point of view, than merely showing the presence of something which decomposes the iodide of potassium. I find, also, that the nitrate of silver possesses other decided advantages. In the first place, it is a much more certain and delicate test. The iodine test is not acted on by hydrochloric acid, but only by the free chlorine, very little of which is produced by passing the vapour of chloroform through a red-hot tube, and that not constantly. Again, if there be a trace of chlorine to set free a little of the iodine, a little warm vapour, which is very apt to rush through the tube, whilst it does not affect the chloride of silver, may either prevent the blue colour of iodide of starch being developed, or suddenly discharge it, as I have seen. And lastly, the nitrate of silver test allows of a quantitative analysis being made, whilst the other does not admit of it. Dr. Alfred Taylor has, however, suggested to me to combine the two tests with a third one, by introducing a slip of starch paper moistened with solution of iodide of potassium, and also a slip of blue litmus paper, into another part of the tube, where it is not wet with the nitrate of silver. Used in this way, these additional tests may tend to confirm the evidence, and to meet objections that might possibly be made to the nitrate of silver test when used alone.

Before relating the experiments in which the presence of chloroform was detected in the body, it will be preferable to give some account of the decomposition which takes place when the vapour of that substance is passed through a red-hot tube. Soubeiran, when treating, in 1831,* of the body afterwards named chloroform, said, that

on passing it, in the form of vapour, through a tube of porcelain filled with small fragments of porcelain, and made red-hot, that a good deal of charcoal is deposited, and that a gas is produced formed almost entirely of hydrochloric acid; and that there is found besides a very small quantity of chlorine and of an inflammable gas. He added, that, unless the pieces of porcelain are so arranged in the tube as to delay the passage of the vapour, without obstructing it too much, there is more chlorine liberated, and a substance left in the tube which stains paper like an oil. Liebig† says of chloroform, "when its vapour is passed through a red-hot tube it is decomposed into carbon, hydrochloric acid, and a crystalline body which appears in long white needles." On another occasion† he says that this crystalline body is probably the perchloride of carbon discovered by Mr. Faraday.

I performed the following experiments with a view more particularly to ascertain whether any appreciable quantity of free chlorine is produced during the decomposition of chloroform at a red heat:—

a. Ten grains of chloroform were put into a dry retort, made out of a small green glass tube, and capable of holding only a drachm. The retort was heated gradually in the water bath. Its beak was kept red-hot by the flame of a spirit lamp, and communicated with two Woollf's bottles, containing solution of nitrate of silver. Charcoal was deposited in the beak of the retort at the part where it was red-hot: half an inch from this part, on each side, there was a copious deposit of long, white, needle-shaped crystals, and, after a time, a reddish-brown oily-looking liquid appeared. The precipitate of chloride of silver, which was found almost exclusively in the first bottle, weighed, after being washed and thoroughly dried, 12·5 grains.

b. Ten grains of chloroform were put into a similar retort and treated in the same way, except that the beak of the retort opened under a receiver in the mercurial trough. The deposits in the tube of the retort were the same as before, and 0·15 cubic inches of gaseous matter were obtained in the receiver.

* *Annales de Chimie et de Physique*, t. xlviii. p. 135.

* *Turner's Chemistry*, 8th edit. p. 1009.

† *Annales de Chimie*, t. xlix.

The tenth of a cubic inch of water being passed through the mercury, 8.5 cubic inches of the gas were absorbed by it. Solution of potash absorbed one-tenth of a cubic inch more, and the remainder consisted almost, or entirely, of air expelled from the retort.

a. Ten grains of chloroform were treated in the same way as before, the beak of the small retort communicating with two Woolfe's bottles, the first of which contained only thirty minims of distilled water, and the second some solution of nitrate of silver. A very slight cloudiness was merely produced in this solution in the second bottle. The water in the first bottle being added, at the end of the process, to a solution of nitrate of silver, and the precipitate occasioned being boiled in nitric acid, washed, and thoroughly dried, was found to weigh 11.45 grains.

If one of the three atoms of chlorine which were contained in the chloroform were to combine with the single atom of hydrogen, the hydrochloric acid thus produced from ten grains would weigh 3.04 grains, and would suffice to form 12.08 grains of chloride of silver. In experiment *a*, the chloride of silver obtained exceeded this by a very little. In experiment *b*, any chlorine which might be developed would be absorbed by the mercury, and the 8.5 cubic inches of gas absorbed by the small quantity of water must have consisted of hydrochloric acid. The weight of it would be 3.24 grains—a very little more than ought by theory to result from the combination of one of the atoms of chlorine with the hydrogen of the formyle; and it would combine to form 12.7 grains of chloride of silver. In experiment *c*, the thirty minims of water, whilst they absorbed the hydrochloric acid gas, could absorb but a very minute quantity of chlorine, certainly less than the tenth of a grain, and consequently if a greater amount of chlorine than this had been evolved it must have passed on to the second bottle, and there caused a precipitate of chloride of silver. On precipitating with nitrate of silver, it will be observed that the quantity of chloride obtained was very nearly that which ought to be formed by the hydrochloric acid produced as suggested above. These experiments, then, tend to show, that if chlorine be produced by passing the vapour of chloroform through a red-hot tube, it must be in extremely small

quantity, and that consequently the proper tests to employ are those which indicate the presence of hydrochloric acid.

The following is a brief account of the experiments for the detection of chloroform in the body:—

Exp. 55.—Two kittens about a fortnight old were placed in a glass jar holding 120 cubic inches. Twelve minims of chloroform were dropped on a piece of blotting paper in the jar, and it was closed. In two minutes the kittens were both insensible, and in two minutes more one of them had ceased to breathe; the other continued to breathe feebly and irregularly for six minutes longer. On the following day one of the kittens was opened: there was no odour of chloroform perceptible in this, any more than in the numerous other animals that I have killed with it.

a. The lungs, liver, and kidneys of this kitten were placed in a wide-mouthed glass flask with two or three drachms of water. The flask was placed in the water bath, to which (common salt not being at hand) was added a little chloride of calcium, to increase the temperature somewhat. A tube passing through the cork of the flask was connected with one of hard glass, which was kept red-hot in the flame of a spirit lamp, and to the end of the latter tube was attached one wetted inside with solution of nitrate of silver. About the time that the contents of the flask began to boil, a white curdy precipitate appeared in the latter tube. This precipitate was rendered dark-coloured by the light. It was insoluble in nitric acid, and very soluble in ammonia.

b. Two days after the death of the kittens, the lungs, heart, liver, and kidneys of the other animal were treated in a similar manner. Soon after the water in the flask began to boil, a precipitate of chloride of silver appeared in the tube.

c. Three days after their death, the brains of both kittens were put into a flask without any water, and heated in the chloride of calcium bath, as the other parts had been. On this occasion the tube moistened with solution of nitrate of silver ended in a Woolfe's bottle containing a few minims of the same solution. By the time that the liquid which had exuded from the brains began to boil, a precipitate began to appear in the tube. In a short time there was a slight cloudiness in the bottle.

The brains were kept boiling in their own serosity for an hour. On the following day heat was again applied to the flask containing the brains which had not been removed; the tube and Woolfe's bottle having, however, been cleaned and supplied with a fresh solution of nitrate of silver. Not the slightest precipitate was obtained on this occasion, although the brains were kept boiling for two hours.

d. Five days after its death one of the kittens was skinned, and the flesh of the limbs, together with the greater part of that of the body and neck, was stripped off and put into the flask and treated as before, with the exception that, instead of the solution of nitrate of silver, a slip of paper moistened with a mixture of starch and solution of iodide of potassium was placed in the farther end of the tube. After the flesh had been made to boil for a little time in its own juice, a small part of the paper was turned blue.

e. Six days after its death the skin of the other kitten was removed, and its flesh put into a flask and treated as above; on this occasion, solution of nitrate of silver being used as the test. The serosity of the flesh had scarcely begun to boil, when a precipitate of chloride of silver began to appear, and was soon as copious as on any previous occasion, both in the tube and Woolfe's bottle. At this time the intestines of the kittens were beginning to be offensive, although the flesh used in the experiment was not at all decomposed. The bodies had lain on a table since the time of death, at the beginning of last May, when the temperature was cool. From the size of the animals, the quantity of chloroform inhaled by each was considerably less than a grain.

To try the delicacy of the above process, a grain of chloroform was dissolved in a hundred drops of rectified spirit, and one drop of this solution was dropped into a flask containing a thousand grains of water. On treating this as above described, a distinct precipitate of chloride of silver was obtained in the tube, thus indicating the presence of the hundredth part of a grain of chloroform in a thousand grains of water.

Exp. 56.—On May 9, some portions of muscle, nearly sufficient to fill a three-ounce bottle, were taken from the calf of the leg of a little boy, about five years old, which had just been amputated by Mr. P. Hewett, under the influence of

chloroform, in St. George's Hospital. About four hours afterwards the pieces of muscle were put into a flask, and treated as before described, solution of nitrate of silver being the test applied. When the liquid exuding from the muscle had been boiling for about ten minutes the precipitate began to appear, and was soon very distinct.

On July 2d, I assisted Dr. Taylor, in the Laboratory of Guy's Hospital, in applying this process to a little of the blood of a man whose death had been occasioned by chloroform, six days previously. The blood, which had been kept in a stoppered bottle, measured six and a half drachms, was of a dark red colour, fluid, but rather thick, and did not smell offensive. It was put into a clean Florence oil flask, from which a tube proceeded which was made red-hot, and a further tube moistened inside with solution of nitrate of silver. The flask was heated in the water bath, to which, after a time, common salt was added. The process was continued for twenty minutes or more, and although a slight cloudiness was observed in the tube, no distinct precipitate of chloride of silver was obtained. It should be remarked that this small quantity of blood must necessarily have been exposed to the air, before it was put into the bottle, by which means it would lose a part of its chloroform.

At the suggestion of Dr. Taylor, some chloroform (about 8 drops) was put into a flask with an ounce of water, and in the further tube were placed, first, a slip of starch paper moistened with solution of iodide of potassium; next, a slip of blue litmus paper, and the distal extremity of the tube was wetted inside with solution of nitrate of silver. The intermediate tube being made red-hot, as soon as heat was applied to the water bath, the two pieces of paper and the solution of nitrate of silver began to be affected, almost simultaneously: the starch paper being rapidly rendered very blue, the change of colour beginning at one end and travelling rapidly along it.

On the same occasion, in order to try the delicacy of these tests, a drop of chloroform, which is equal to the third of a grain, was agitated in a minim measure with fifty minims of alcohol. Five minims of this solution were added to an ounce of water in a flask, which

would consequently contain the thirtieth part of a grain of chloroform. A fresh tube being attached, containing the three tests before employed, and the flask being heated in the water bath, a decided effect was, in a little time, produced on all the tests. The starch paper was rendered blue; the litmus was turned red; and a very distinct precipitate was obtained in the solution of nitrate of silver.

EXP. 57.—July 13: Half a drachm of chloroform was diffused through a jar holding 870 cubic inches, and a kitten, weighing a little over thirteen ounces, was put in. In two minutes it was quite insensible, and at the end of ten minutes it died. On the 15th the kitten was opened, and the viscera of the chest, the liver, and the brain, weighing together nearly two ounces, were put into a flask and heated in the salt water bath. A tube coming from the flask was kept red-hot, and a further tube contained a slip of starch and iodide of potassium paper, and a slip of blue litmus, and terminated near the bottom of a Woolfe's bottle containing a few minims of solution of nitrate of silver. At the early part of the process, the edge of the starch paper seemed to be slightly changing colour, but after a little time no change of colour could again be observed in it. The blue litmus was very soon reddened, and the solution of nitrate of silver began to be turbid, and the turbidity increased for some time. The viscera were kept boiling in their serosity for half an hour.

On the following day other six ounces of the same kitten were put into the same flask; the intestines, skin, and larger bones being only left. Fresh starch paper was put into the tube which terminated in the bottle containing the same solution of nitrate of silver. After a little time the starch paper was decidedly darkened, at the corner nearest the flask, but only to a limited extent, which did not increase. The parts were kept boiling in their serosity for two hours, when the process was ended by the breaking of the tube at the part where it was red-hot, owing to a little condensed steam being projected against it. At the same moment the limited blueness of the starch paper was discharged. The tube being left lying on the table, it was found next day that the starch paper was very blue throughout its entire extent, from what cause I do

not know. The precipitate of chloride of silver was separated by filtration, and but for an accident would have been dried and weighed. There appeared to be not less than the twentieth part of a grain of it.

There is no deposit of carbon in the red-hot part of the tube in this process, as the apparatus always contains sufficient air for the formation of the carbon into carbonic acid. The white needle-formed crystals previously mentioned are deposited, but not in sufficient quantity to be of service as a test. It is desirable to make the tubes proceeding from the flask incline a little upwards, so that the vapour which is condensed before reaching the red-hot part may flow back again. I consider that the solid organs of the body should be taken for analysis, in preference to the blood in a separate state, as that contained in the minute vessels is protected from the action of the air. The parts should be cut in pieces, and put into the flask, without any addition. The stomach should not be selected for examination by the above process, as the gastric juice contains a minute quantity of free hydrochloric acid, and hence the evidence would be liable to objection. The intestines also do not seem suitable parts for examination, as the sulphuretted hydrogen they might contain would interfere, more or less, with the tests. In other respects it matters little what part of the body be used, further than that the most vascular parts are the best. As regards a quantitative analysis, it results from some of the experiments, detailed in an early part of these papers,* that, in a case of death from chloroform, a quarter of a pound of any organ of average vascularity would contain about the twelfth part of a grain, which, if the whole of it were separated and decomposed, would produce about the tenth of a grain of chloride of silver.

The process above described does not prove the presence of chloroform itself, but only that of a volatile compound containing chlorine. In this respect it resembles the processes for the detection of arsenious acid and corrosive sublimate in the tissues, which prove only the presence of a compound of arsenic, or of mercury. The only compounds containing chlorine which are volatile at

* MED. GAZ., vol. xlii. p. 415.

the heat of boiling water, are substances such as chloride of ethyle, Dutch liquid, and some others, which resemble chloroform in their effects, but are none of them in common use. In order to be quite certain that the precipitate is no other salt of silver than the chloride, besides the tests of ammonia and nitric acid, solution of potash might be added to another portion of it, as recommended by Dr. Taylor, in treating of hydrochloric acid.* Potash does not change the chloride of silver without heat.

With these limitations and precautions the process is, I believe, liable to no fallacy. There are chlorides in the body, but they cannot be decomposed, except at a high temperature, and not till the part under examination should become dry, which, in the method here described, could not take place in the most protracted examination. Besides, I have made several examinations of parts not containing chloroform without meeting with anything that produced the slightest effect on the nitrate of silver, or on the starch or litmus test. The bodies of two kittens killed with the vapour of ether were submitted to the process, by portions at a time, which were made to boil in their own serosity for an hour or two, but not the least effect was produced on any of these tests. Hearing, in the beginning of May last, that chloroform was suspected, by some of the coroner's jury, to have been used in the case of a woman who was found dead, under mysterious circumstances, in the Wandsworth Road, I applied to Mr. John Parrott, who was polite enough to send me some portions of the body, including part of the brain and liver. They had been kept in a covered jar from the time they were removed from the body. The chemical examination commenced four days after death, whilst the parts were fresh, and although very carefully conducted, not the least effect was produced, either on the nitrate of silver or starch and iodine test.

[To be continued.]

APOTHECARIES' HALL.

NAMES of gentlemen who passed their examination in the science and practice of medicine, and received certificates to practice, on Thursday, 15th August, 1850:—David Philbrick Morris, Colchester, Essex—Henry Ward, Northampton—Benjamin Ward Richardson—Joseph Dixon, Reigate, Surrey.

* Medical Jurisprudence, p. 91.

NOTES ON THE STRUCTURE, &c. OF TEETH.

BY C. SPENCE BATE.

Dentine.

I. THE resemblance of the tooth-substance, or dentine, with that of true bone, has long been a subject of discussion. The opinion of a supposed analogy has been based upon microscopic research among various characters of teeth belonging to different tribes of animals.

Chemistry also has been made use of in order to assist towards a correct conclusion; and analysis shows that the difference between the two substances exists but in a matter of a relative quantity of the parts respectively of which they are both composed.

Whenever there is any difficulty in defining the character of a structure when complete, the surest means to obtain the knowledge is to study its progressive development and formation, by tracing the analogy which it holds to that of the development of other structures among which it is presumed to hold a place; that, if we cannot interpret the sign when complete, we may if we watch their putting together.

II. In the development of bone we find cells generated in temporary cartilage,—a homogeneous non-vascular plasma, which obtain their calcareous contents by secretion from the blood-vessels which ramify and inosculate with one another in that part of the structure which has already been converted into bone.

III. But if we examine the manner in which the tooth is developed from the pulp, we find that a somewhat different process is pursued.

There is no true temporary cartilage to form the bone of tooth; but this substance is represented by the pulp, which only approaches to its character in a limited degree.

IV. From the very first formation of the papilla, the pulp is a highly vascular organ (Fig. 1). Blood-vessels, which supply it with the means of growth and development, pass into the centre of its own substance. The growth of the pulp continues until it has reached the diameter of the future tooth when fully developed: having ex-

tended to its furthest limits, ossification commences, and not before.

FIG. 1.



FIG. 1. Pulp of human permanent under incisor at birth, showing its vascularity.

This takes place at the apex, and proceeds to extend over the periphery of the pulp in the character of a scale; thus differing from the development of bone, which ossifies first towards the deepest part, or centre of the cartilage.

Thus we see that the mass of the pulp is not converted directly by ossification into bone, but that it undergoes a transformation previous to this change, which transformation takes place normally only at the periphery.

V. If we examine a section of the pulp of a tooth cut horizontally, we find that its substance is not composed of isolated cells in a homogeneous plasma, like temporary cartilage, but that, there ramifies throughout the mass a cellular tissue, areolar in character, which sometimes, if not always, ossifies with the rest of the pulp (*vide* fig. 8). But if we make a section of the same tooth, which shall embrace the surface of the pulp at that point which approaches nearest to where conversion into ivory has already taken place, we observe that the character has changed: the cells unite together in a very uniform character, so as to form a layer without any inter-cellular substance.

If we trace this layer of cells towards the dentine deposited, we cannot define the exact point at which ossification commences, so gradual is the transition; but we may shortly observe that between the cells an opacity is visible. By tracing these transition-cells still further into the bone, we perceive distinct spots, which in some instances

may be seen to be hollow spaces admitting light. Still deeper in the dentine we find that these spots become united to lines of a less diameter than their own: these lines are the commencement of the tubes of Retzius, Owen, and Tomes,—the beccated fibres of Nasmyth.

Having obtained two sections,—one of which was horizontal with the pulp, the other at right angles, or rather diagonal with the structure of dentine,—our next object is to obtain one which shall be in a direction corresponding with both the dentine and the pulp. To procure this, the better way is to make a section of a tooth at the apex, taking care not to dislocate the pulp from its connection with the ivory. After many attempts, the one from which the accompanying illustration is taken was obtained from a lamb at birth.

FIG. 2.*

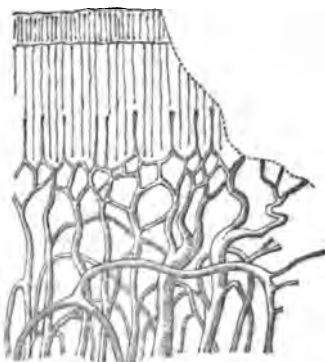


FIG. 2. Pulp in connection with dentine, demonstrating tubercular structure of loop, from lamb at birth.

In this section the blood-vessels may be traced to the confines of the pulp, where they form loops, from the extremity of which, processes extend into the substance of the dentine for a considerable distance, thereby demonstrating the original tubular character of the fibres in dentine.†

* The sketch from which this cut is taken was made as carefully as the author was capable; and the fragment of tooth from which it was drawn is still preserved.

† It is stated in the *Annals of Natural History* for April, that Professor Owen lately, in dissecting an elephant which died at the Zoological Gardens, fully verified the idea of the tubular structure of ivory, inasmuch that he, in removing the pulp from its connection with the dentine, distinctly saw processes of the pulp pass into the tubes in the ivory.

VI. The cells of which the pulp is mostly composed are various in size and form, but assume a uniformity in both as they approach the external surface of the pulp, where they are endowed with the power of secreting the bone-earth, which, upon having fulfilled their office in becoming charged with calcareous matter, the dentine may be said to be completed; as a large tusk of ivory is but a repetition of separate calcareous scales or layers, the demarcation of which may be observed by the naked eye in the rings which are seen upon a section of an elephant's or hippopotamus's tooth, when cut in two at right angles with its length, and which bear a resemblance to the rings of annual deposit in trees of exogenous growth.

These layers may be more distinctly observed in a thin section fractured forcibly from the mass, in which they may be seen to succeed each other in an imbricated manner.

These layers continue to be deposited until the whole pulp has been converted into dentine, which, in some kinds of teeth, is completed within a definite period, whilst in others it is extended over an unlimited space.

In man, and all animals in which the fangs of the teeth decrease towards a point, the growth of the tooth is limited, although conversion of the pulp into dentine continues slowly to be carried on during probably the greater portion of existence; but this is wholly within the tooth: the pulp canal gradually decreases, and in old age approaches obliteration.

But in those animals in which the pulp lies in an open fossa, the tooth continues more or less slowly to increase in length,—a supply set up at one extremity to meet a demand at the opposite, in the waste of dentine occasioned by mastication.

This is well known to be the case in all the Edentata class of mammals; but it is also the case in some of the Rodentia. This is well exemplified in the case of a hare shot in this neighbourhood (Swansea). It by some accident had long since lost the under incisor teeth; consequently, those of the superior jaw increased in length to such an extent as almost to complete a circle, which it probably would have done had the animal not been taken until a few years later.

FIG. 3.*

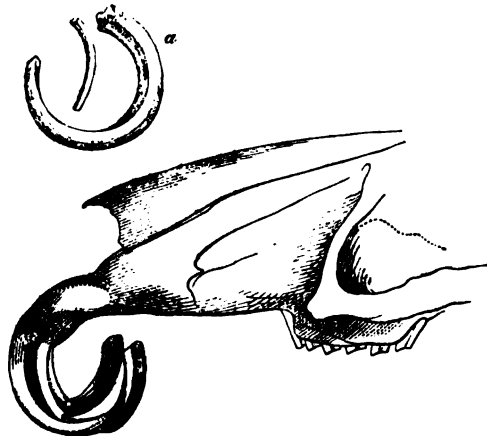


FIG. 3. Hare (*Lepus timidus*), the superior incisor teeth of which had grown to an extraordinary length; (a) tooth removed from the socket.

Neither can this be confounded with the prolongation of teeth such as we find take place in the human mouth

upon the loss of an antagonistic tooth, which is not growth of bone, but an elevation of the tooth in its own socket, from which it generally, by an effort of nature, is ultimately expelled.

VII. The cells of the pulp pass into

* This animal was shot a year or two since by Mr. Gage, and the skull is now in the cabinet of L. Dillwyn, Esq.

dentine, and by their transition compress the vascular system into a smaller area: the bloodvessels which first nourished the spot now hardening, are reduced, so that they send only a branch or process, which, upon dentine being further developed, becomes still more reduced in diameter, so as to preclude the passage of a single blood-disc. The remains of the vessel ossify, and become the parietes of the tubes.

There is another fact, besides those already mentioned, which shows that the original diameter of these tubes was much greater. If we take a section of dentine which shall cut the tubes at right angles with their length, we shall observe that a transparent areola surrounds the opaque centre; the circumferences of these areolæ appear to mark the area of the original tubes (*vide* fig. 8).

VIII. If we compare this structure with other animals, we find in fish the same fibres or tubes present; and it is but reasonable to presume that in all cases, whether fish or mammal, the process of their development is the same.

There is among the former a species of dentine termed vascular, or vaso-dentine, in which we lose the fine hair-like tubes, and in their place have large canals. These canals I believe to be the original blood-vessels of the pulp, which latter has become ossified without compressing them into tubuli smaller than their original diameter.

Fig. 4.*

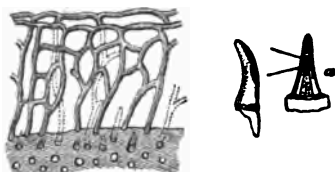


FIG. 4. Dentine, from section of tooth of the Hake (*Gadus maluccius*); (a) natural size.

It is scarcely possible for any one not to be struck with the remarkable similarity between the annexed cut and that of fig. 2; fig. 4 being the ossification of the pulp without compression of

* I picked up the specimen in a jaw stranded upon the sea-shore, and am not an ichthyologist sufficient to identify beyond doubt the genus to which it belongs.

the blood-vessels,—that of fig. 2 the blood-vessels in the pulp previous to ossification.

But a still greater resemblance to the former figure may be seen in those teeth of fish, such as the shark of the extinct genus *Lamna*, wherein an external layer, something approaching true dentine in character, is formed external to the vascular dentine, wherein the tubes may be seen to terminate in the large canals,—the presumed blood-vessels of the pulp previous to ossification,—and which may still continue to be so in this modified form of the tooth-substance.

Fig. 5.

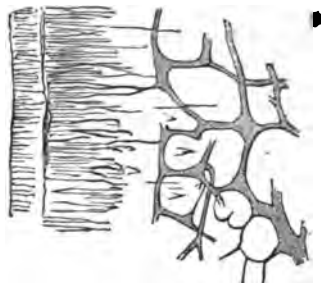


Fig. 5. From fossil tooth found in the Isle of Sheppy.

IX. It is owing to the compression of these canals, which trace the path of the compressed blood-vessels, that the waves in the tubuli known as the primary curves are due: but those which are recognised as the secondary, owe their origin to the bends occasioned by the alternating position of the cells of which the dentine itself is composed.

The relation which they bear to each other may be seen in the two annexed engravings,—the one (fig. 6) being from the pulp of a human deciduous tooth at birth, that of the other* (fig. 7) from the dentine of a permanent human tooth, a

Fig. 6.



Fig. 7.



FIG. 6. Surface of human pulp of a tooth at birth.

FIG. 7. Human dentine, saturated in dilute hydrochloric acid.

* Fig. 7 was suggested in order to verify a similar experiment of Mr. Nasmyth's.

section of which had been saturated for a few hours in dilute hydrochloric acid.

With regard to the baccated character of which Mr. Nasmyth writes, much I think may be attributed to the manner in which his objects were prepared, most having been obtained by violence, some by being pounded in a mortar, others being shavings obtained by the turning lathe. These would most probably produce the appearance such as Mr. Nasmyth has figured, and which he so strongly urges.

But the appearance of a baccated structure may occasionally be due to other causes; as, for instance, should the cells adapt themselves so that the interstices of one row may fall opposite to those in the next, instead of alternating: or, should the secondary curves be partially rubbed away in the preparation of the section, a similar modification would be the result.

X. It must have been noticed by the least accurate observer that these lines are the only opaque markings in dentine, the remainder of the substance being semi transparent. It is also upon this, among other reasons, that Nasmyth has joined issue with Retzius, and others, as to the tubular character of these lines; and it is not without justice that the following argument is made use of in Mr. Nasmyth's posthumous work, that "Retzius and his followers remark that, when a section of a tooth, made transversely to the direction of the fibres, is viewed upon a dark ground, a white spot is perceptible in the area of each of the so called tubes. Now this is the fact, but the inference to be drawn from it is directly opposed to that which Retzius endeavours to establish; for it is quite evident that if we view an object pierced by an aperture, instead of being white it should be black, while the contrary proves that it is no hole at all."

XI. I wish now to draw attention to a peculiar appearance often met with in the fangs of individual teeth, when the periosteum has been the seat of an inflammatory action, chronic in character, and of long standing. The appearance to which I allude must have fallen under the notice of all who study the diseases of the teeth. The dentine of the part affected loses its ivory-white appearance, and becomes transparent and corneous.

Upon a section being made of den-

tine so acted upon, it will be found, both to the naked eye and beneath the lens of few diameters' increase, that these opaque lines are white, and that it is to the size and number of these that the more or less whiteness of ivory in different animals is due.

XII. Perceiving that these lines are capable of being removed or obliterated without any apparent action upon the intertubular structure, I obtained, through the kind assistance of Mr. Napier of Glasgow, a chemical analysis of the diseased and healthy parts, in order to obtain the relative proportion of calcareous salts they might contain when compared with each other. Making use of his experiments, as well as the table given by Mr. Nasmyth upon the authorities of Professor Thomson of Glasgow, and his nephew Dr. Thomson, I find that there is a deficiency of carbonate of lime in the dentine from which the fibres or tubes are absent, when compared with that found in normal ivory.

Taking the average of perfect dentine to contain 7 per cent. of carbonate of lime, that of the diseased or corneous dentine appears to be about 5.25 per cent.; and I think, if we could obtain good samples, and moreover remove the cementum, the proportion would be still less.

I do not mean to put forth these experiments as conclusive, since the quantity of carbonate of lime in dentine is variable. In some experiments the result has been as high as 8.23 per cent., and again as low as 2.86. But I believe that this has been owing to indiscriminately taking teeth of all ages, and which have been more or less attacked by the disease above described.

XIII. Upon testing these observations by comparative anatomy, it will be found that in the hippopotamus, the whitest of all ivory, the opaque lines are most numerous, and the carbonate of lime very abundant, as much as 9.14 per cent. So, also, does the white or external portion of the walrus contain more carbonate of lime than the internal, in the proportion of 4.38 to 1.08; and it appears to hold good as a rule, that white ivory contains more carbonate of lime than the more transparent or green ivory; or, in other words, that the whiteness of ivory is dependent upon the quantity of carbonate of lime which it contains.

XIV. Viewing these facts in connection, I think there is a legitimate basis wherewith to argue that these opaque lines are due to the presence of carbonate of lime; that they are tubes in the earlier part of their formation, but they ultimately become filled, more or less, with the calcareous salt; and upon their filling up depends the perfection of the dentine.

XV. Long after I had arrived at this conclusion, I found the idea had already been suggested to the observant mind of Mr. Tomes, but who has since found reasons to abandon it. Believing, however, that his experiment assists to prove the fact, I shall quote his own words:—

“In the year 1837 I was engaged in examining the structure of teeth, and then came to the conclusion that the dentinal tubes contained an amorphous salt of lime. My opinion was grounded on the following experiment:—After preparing a thin section of human tooth, I placed it in the field of the microscope, and then added a little muriatic acid. No sooner was the acid in contact with the section than evidence of chemical action was rendered visible by the appearance of bubbles of gas, and these emanated not only from the external surface of the section, but also from the interior of the tubes, from which bubbles of gas were seen issuing in quick succession. When the action ceased, the tubes no longer presented the appearance of opaque dark lines, but were indistinctly seen filled with transparent fluid.

At the time, much struck with the result of the experiment, I was led to the opinion that the gas generated in the tubes was produced by the decomposition of their solid contents, which I supposed to be carbonate of lime. However, upon more extended observations, I was induced, from examination of teeth in which the tubes are large, and also from the examination of tubuli of the human tooth with a high power, to modify my first opinion, and was compelled to adopt the views advanced in the former part of the lecture, and to regard the evolution of gas from the tubuli as evidence of the facility with which fluids are admitted into their interior, and to consider that the source of the gas existed in the decomposition of the parietes of the tubes rather than their contents.”

The idea is also approximated by

Prof. Owen, who states, in his *Odontography*, that the tubes ultimately become filled with a granulous material.

And it may be inferred, from the following sentence, that a similar opinion was held by Retzius, who says, “The calcareous salts of the tooth exist not merely in the tubuli, but also in the intermediate substance, which contains, at all events, the greater part of the calcareous earth, either chemically united with cartilage, or deposited in it in an invisible manner.”

I have previously shown that, in the early formation of dentine, these tubes are formed by the compression of the blood-vessels of the pulp by the dentine cells. When the blood ceases to penetrate the structure, the membranous walls then ossify with the rest of the structure, and according to the thickness of the membrane would be the more or less perfect filling up of the tubes. In those animals in which the tubuli of the dentine is large, the probability is that they would be less perfectly filled than in others where the tubuli are smaller: hence the cause which led Mr. Tomes to abandon the idea.

To this cause may be attributed the perfection of the hippopotamus ivory; the tubes are more numerous and minute, while the carbonate of lime is more abundant than in other animals; whilst the phosphate of lime is rather less than in some dentine, which is neither so hard nor durable.

XVI. It is, I believe, a fact that carbonate of lime is not formed in the blood. If this be true, then we must presume that this salt, as found in teeth and bones, must be formed during their development.

The super-phosphate of lime, we know, is secreted from the blood by a power (similar, probably, to endosmose) in the cells; but it appears that in the teeth carbonate of lime is only present in combination with membranous tissue, such as the parietes of cells, blood-vessels, areolar tissue, &c. An example of the last may be seen in the following figure, in which the areolar tissue of the pulp is seen to form part of the structure of the dentine* (Fig. 8).

* The tooth from which the section was taken was free from caries, and extracted from the mouth of an individual between 50 and 60 years of age.

Fig. 8.

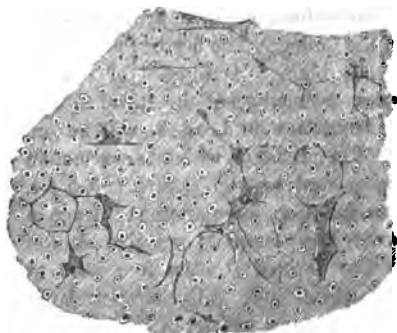


FIG. 8. Section of human dentine, exhibiting the ossification of the areolar tissue of the pulp with the rest of the tooth: from a specimen kept some months in writing ink.

XVII. Thus carbonate of soda is present as an amorphous salt in combination with animal tissue, and by its presence converts the parietes of the different classes of vessels which make up the pulp into bone, while the cells have the power of secreting the phosphate only.

In corroboration of this idea, I would wish to draw attention to the ossification of distinct centres of dentine in the pulps of teeth. These apparently commence in a single cell; and as such I have often met with them in the pulps of human adult teeth: so, also, in a well-grown calf; but they seem to be more common in the cachalot, if we may judge from the nodular appearance of the internal surface of the teeth of this mammal, which become more or less incorporated with the true dentine as development takes place, and the latter encroaches on the magnitude of the pulp.

It seems that an isolated cell obtains the power of secreting calcareous matter, similar to those in which the action appears to be normal, only when in connection with the periphery of the pulp.

These centres have all the appearance of being single cells, and, being so, are transparent, and in texture resemble the intertubular structure of dentine. These, being once formed, become the nucleus of a deposit of dentine distinct from the dentine of the tooth: but why some cells have the power of secreting calcareous matter, and others that of reproducing more of their own kind, is

yet to be made known. That it is a condition of the cells, and not its position in relation to the pulp, which induces the secretion, is apparent from the fact that these isolated centres do exist.

XVIII. That the dentine of the teeth are developed in a similar manner to bone can only be considered an approximation to the truth, and that not of the nearest: whereas, by a consideration of their origin from the mucous membrane, and the whole manner in which their development is carried on, little can, I think, be observed, except the presence of a calcareous salt, that can separate it from the remainder of the external skin of animals; to which it may be compared, as holding a relation similar to that which the scales of a fish do to the soft skin on which they are attached, of which the bony scales of the sturgeon (*Acipenser Sturio*) affords the most ready example.

XIX. In fact further to substantiate this remark, I would draw attention to the formation of the calcareous dermal skeleton of crustacea.*

No real difference, I think, can be observed between the development of the shell of the crab and that of the teeth in higher animals. They both are developed from a pulp consisting of areolar tissue, blood-vessels, and nucleated cells, which last have the power of reproducing others smaller than themselves, which ultimately combine without any inter-cellular plasma to form a pavement, into which structure calcareous matter is deposited.

But if we confine our observations more immediately to the teeth of this tribe of animals, which are identical, both in composition and manner of development, with the rest of the shell of which they form a part, no difference can be made out between their formation and that of true teeth, except that an arrest of development takes place in the crab with the deposit of the scale or layer of osseous tissue, which arrest continues for a definite period, when development again proceeds.

To this analogy reference will again be made under the head of "Shedding."

[To be continued.]

* Dr. Carpenter, in his manual, has noticed the analogy which exists between dentine and the calcareous shells of crustacea; but his remarks relate to the density of the shell, and bears no reference to its development, which, as

MEDICAL GAZETTE.

FRIDAY, AUGUST 23, 1850.

We have now before us the Bill for the Incorporation of the General Practitioners, as it has been recently laid before Parliament by Mr. Wyld and Colonel Thompson. If we do not reprint it, it is because its clauses have already in substance been fully and widely circulated in the various editions of Sir James Graham's Medical Reform measures. As embodying the views, however, of a respectable portion of the profession, we consider it to be our duty to advert to the principles, and to give an outline of the clauses of the bill.

The new Incorporation is to be designated the Royal College of General Practitioners in Medicine, Surgery, and Midwifery. The College is to consist of an unlimited number of members, and to be governed by a President and Council. The right of membership is to be extended to all practitioners with a single, double, English, Irish, or Scotch qualification, provided it be claimed within one year after the passing of the Act by residents, and within two years by those who are non-resident in the United Kingdom. The order of precedence is to be settled by the date at which membership is claimed, and to be determined by the date of the new diploma issued by the College.

New members will be admitted only after they have attained the age of twenty-two years, and have completed

the course of study, and undergone the examinations, required by the Council of the College. The fee for the diploma is left blank. Graduates of Universities, or members of the Royal College of Physicians in London, will be entitled to admission without examination (*ad eundem*) on the payment of a fee, provided they have attained the age of twenty-two years, and can give proof of having devoted at least five years to professional study. A Registration clause provides that a register of the names of members shall be published yearly in the month of March: and a correct list is ensured by the infliction of penal disqualification on the omission of a member to send his name, place of abode, with change of residence, &c., yearly to the Council.

The Council is to consist of forty-eight members, of whom not fewer than twenty-four must reside within ten miles of the city of London. The President, after the first nomination, to be elected by the Council and to hold office for three years. The election of Members of Council is to be placed in the hands of members of *five years'* standing, or whose diplomas (from other Colleges) shall show that they have been five years in the profession. The qualification of President or Member of the Council is to be fifteen years' membership, or fifteen years' possession of any diploma, by virtue of which he may have become a member of the College.

The powers of the Council will be much the same as in other Colleges: there will be an annual general meeting for considering and reviewing the bye-laws made for the government of the College, &c.—the bye-laws to have no legal force until sanctioned and approved by the Secretary of State. The 24th clause provides for the registration of students on the payment of a fee of half-a-crown by each student. The examiners are to be annually appointed by

far as I am aware, has never previously been observed to originate in a pulp. The only place that I am conscious of its development being at all referred to, is in Dr. Carpenter's Principles of Physiology, wherein he mentions having seen the new shell of a crab soon after the exuvia had been cast, and imagines that it must have had its origin in cells.

the Council, and no President or Member of Council shall be eligible; but the President, one of the Vice-presidents, or some member of Council appointed for the purpose, shall be present and shall preside at, but not interfere with the examination.

Another clause provides for the expulsion of members guilty of misconduct.

The 27th clause supersedes the existing rights of licentiates of the Apothecaries' Society, and of the members or licentiates of all other Colleges or Universities in the kingdom, excepting those of members of the Royal Colleges of Physicians and Surgeons of England. Thus all appointments to Hospitals, Prisons, Infirmaries, or other public institutions in the United Kingdom, are reserved to those who are registered under this Act, with the two exceptions above mentioned. The exemption from serving on juries is also similarly reserved to registered surgeon-apothecaries or general practitioners. Perhaps we have not clearly understood this clause, but, as we take it, a member of the Royal College of Surgeons of Edinburgh or Dublin would not be entitled to take the office of surgeon to any Hospital or Infirmary in the United Kingdom, unless he had previously enrolled himself as a general practitioner in the new College, or had procured the diploma of the Royal College of Surgeons of England. Again, as we understand it, the power of granting medical certificates in any case required by law is limited to members of the Colleges of Physicians and Surgeons of England, and of the new College of Surgeon-apothecaries or General Practitioners.

We may here just inquire whether the Irish and Scotch Colleges have consented to this act of self-immolation for the benefit of English surgeon-apothecaries, or whether there has been

an oversight in the construction of the clause.

The power of recovering charges for medical and surgical advice and attendance, and for medicines prescribed or administered to patients, is reserved to the enrolled members of the new College, and to those who legally enjoyed this privilege before its foundation.

The 29th Clause is not very intelligible. It proposes to impose a penalty of twenty pounds on *unqualified* persons, but it substantially inflicts this penalty on *every surgeon and apothecary, or general practitioner, not qualified according to the provisions of the act*, who shall unlawfully or knowingly practise in any medical or surgical office to which *he has been appointed*. The summary process of the County Court is for this purpose placed at the option of the prosecutor.

We must here pause to inquire whether this *persecution* of surgeon-apothecaries or general practitioners can be seriously intended by the framers of the bill: A man licensed by the College of Surgeons and the Society of Apothecaries may entertain a distrust of the new College, and decline to surrender his connection with old institutions for the membership of a new and untried foundation. This clause, it appears to us, would render him liable to a penalty of twenty pounds, recoverable in a County Court, if, without having qualified himself according to the provisions of the act, he practised in any medical or surgical office. There is no penalty under this clause on any person *not* being a surgeon-apothecary or general practitioner, for knowingly acting or practising in such office.

Admitting that, by the 27th clause, a Member of the College of Surgeons of England might possibly claim exemption from the penalty inflicted by the 29th, the latter exposes to the tricks of all informers, men qualified

and licensed to practise by the English Society of Apothecaries and by the Scotch and Irish Colleges; while it shields from prosecution those who have the good luck not to belong to any College whatever, and who have received no medical or surgical education! The 27th clause should therefore be entitled, —one for imposing a penalty on *qualified persons* who have neglected or have deliberately refused to join the new College.*

The 30th clause imposes a penalty of twenty pounds upon all persons who practise as surgeon-apothecaries or general practitioners in England and Wales, but who are not registered under the act.

Lastly, a "General Practitioner" is defined under the act to be any qualified person "practising the science and practice of medicine as an apothecary, or the art and science of surgery, and the science or practice of medicine, conjointly, with or without the art or science of midwifery." Hence it comprises all licensed physicians, surgeons, and apothecaries.

Such, then, is an outline of the proposed new act on Medical Reform. It abolishes the Society of Apothecaries, and it practically annihilates the rights and privileges of all the medical corporations in the United Kingdom, excepting those of the Royal Colleges of Physicians and Surgeons of England.

We believe that it will not meet with a favourable reception in any one of the three kingdoms. The English Colleges of Physicians and Surgeons will resist the licensing by examination in medi-

cine and surgery, the effect of which would be in the end to deprive them of their present position by withdrawing candidates for their diplomas. The reserving of the rights of members in case, would be no compensation for withdrawing members in *posse*: and it is a fair question whether by such a change the public would not be serious losers in the *surgical* department of the profession.

The Scotch and Irish Colleges would be even more damaged by the provisions of this bill. Their members would be compelled to enrol themselves as general practitioners, or to forego the rights and privileges which they now enjoy—if not by law, at least by custom.

Setting aside the opposition which such a bill is likely to encounter from all the medical corporations of the empire, its provisions will not, we believe, be found in accordance with the wishes of the majority of the profession in any one of the three kingdoms. There is therefore not the slightest prospect of its becoming law. The profession desire, not a new College reaching only the level of the Society of Apothecaries, and falling far below that of the Royal College of Surgeons, but a modification of the laws by which our medical corporations are at present governed, and a reduction in the number of licensing bodies.

The great obstacle to the settlement of the question of Medical Reform is, that it is difficult to find even ten practitioners who agree to the amount and nature of the modification which the laws and regulations of our Colleges should undergo. The present bill does not remove this obstacle. There is a restricted suffrage, with a triennial presidentship and membership of council. The Council are too numerous for a practical working body: the *senior* would soon take the lead of the *extra-urbs* members; and their proceedings regard-

* The looseness with which this clause is drawn up, reminds us of one in an intended penal act for altering parish registers. A clause specified that *one-half* of the penalty should go to the informer, and the other half to the Queen: another clause described the said penalty to be "seven years' transportation." The blunder was fortunately discovered before the measure became law, or Her Majesty would have only escaped three and a half years' transportation by a dearth of informers.

ing fees, examinations, the disposition of funds, and the restriction of privileges, would, if conceded, give rise within six months to as great dissatisfaction as now exists with respect to older and tried institutions. For all these reasons, we believe that the Secretary of State will pause before he adopts this bill as a remedy for the grievances of which the profession complains.

In the extract of a letter published in the leading article of our last number,* Dr. Letheby states that it was admitted by Sir Benjamin Brodie and others, that the questions put to him by the judge placed him in a very unsatisfactory and unenviable position. In a note at the conclusion of the leading article, we expressed our surprise that such an admission should have been made, because obviously it is not a question, but the answer, which is liable to place a witness in an unsatisfactory position. We have heard from an authentic source, that such an admission as that imputed to him by Dr. Letheby was not made by Sir Benjamin Brodie.

INTERCOSTAL NEURALGIA IN PHTHISIS.

M. BEAU relates some cases whence he deduces the conclusion that a neuralgic intercostal pain is one of the earliest indications of phthisis. The seat of pain is usually in the sixth or seventh intercostal nerve, and most frequently on the left side. M. Beau also states that a neuralgic pain between the shoulders may be regarded as almost constantly a premonitory symptom of phthisis, and that this results from the dyspepsia which precedes the deposition of tubercle.—*L'Union Médicale*. X

ARREST OF HEMORRHAGE BY MATICO.

THE stump of an arm, amputated near the shoulder, bled secondarily to a dangerous amount, from the whole surface. Pressure having failed, a paste was made by moistening powdered matico, and this was spread over the part. It acted as an artificial coagulum, and checked the flow of blood, which did not afterwards return. The only other remedy would have been to tie the subclavian artery.—*Dr. Hartshorne, in American Journal of Med. Sciences, Jan.*

Reviews.

A Practical Treatise on Diseases affecting the Skin. By the late ANTHONY TODD THOMSON, M.D., F.L.S., &c. &c. Completed and edited by EDMUND A. PARKES, M.D. 8vo. pp. 440. London: Longman and Co. 1850.

WE are informed by the editor's preface that for more than a year previous to his last fatal illness, Dr. Thomson was engaged in the preparation of a Treatise on Diseases affecting the Skin. About three hundred pages only of this work were printed, the articles Acne and Syccosis also being left in MS. Dr. Parkes has undertaken the task of completing the work. No fitter or more competent person could have been found for the purpose. Dr. Parkes is nephew to the late Dr. Thomson, was several years his assistant in his museum and laboratory at University College, received his private instructions when a student of medicine, has since highly distinguished himself by his professional acquirements, and, lastly, has become intimately familiar with Dr. Thomson's views on the subject of the present work, by his close association with him in the daily labours of his latter years. We can therefore confidently assure our readers that Dr. Parkes's additions will faithfully convey the spirit of Dr. Thomson's pathological and therapeutic opinions and practice.

The work itself is preceded by a Memoir of Dr. Thomson, in which, although anonymous, we think we can trace the pen, and the affections, of the accomplished authoress of "The Life of Sarah, Duchess of Marlborough."

In order to present our readers with a general view of the contents of a work the nature of which almost forbids quotations, we shall in the first place adduce the author's arrangement.

1. Febrile contagious diseases non-recurrent, except in exceptional cases, —including the exanthemata.
2. Febrile contagious diseases capable of recurrence many times during life, —including the varieties of erysipelas.
3. Non-febrile contagious diseases—porrigo, scabies, &c.
4. Non-contagious febrile diseases—erythema, urticaria, &c.
5. Non-contagious, non-febrile dis-

eases, chiefly connected with, and characteristic of, derangements of the digestive, assimilating, and secreting organs. This is divided into six orders of eruptions—papular, pustular, vesicular, squamous, tuberiform or tubercular, and hæmorrhage.

6. Diseases not included in the former division, or of doubtful position.

7. Syphilides.

8. Non-eruptive morbid states of the skin,—including hypertrophies of its tissues, pigmentary alterations, abnormal conditions of the glands, acne, sycosis, abnormal conditions of the hair-follicles and hair.

9. New formations of the skin.

10. Diseases of the nails.

An appendix by the editor contains the results of the most recent investigations into the minute anatomy of several forms of cutaneous disease.

Dr. Parkes's remarks with reference to the classification of *porrigo favosa*, *lupinosa*, and *scutulata*, certainly exhibit the almost insuperable difficulties which attend any general arrangement of skin-diseases,—a difficulty obviously felt by Dr. Thomson, and not removed by the classification adopted in the work before us. We believe that the natural arrangement proposed by Erasmus Wilson will be found to form ultimately the best for practical purposes. The classification of skin-diseases according to their contagiousness or non-contagiousness is peculiarly open to objection. Thus, *erysipelas* forms a class by itself in Dr. Thomson's arrangement, on account of its contagious character; but it is well known that this is a property which this disease does not invariably possess, and which, when manifested, is superinduced by certain unfavourable concomitant conditions; such as the impure air of imperfectly ventilated apartments, neglect of cleanliness, peculiar epidemic constitutions of seasons, &c.

Our readers will perhaps be surprised to find doubts expressed on the influence of the *acarus scabiei* in the causation of itch. Dr. Thomson observes—

"That the disease is contagious there is no doubt; and, in my opinion, it is equally certain that it is the result of a morbid poison; and that the fluid of the vesicles or the pustules is the agent transmitting the disease from one person to another. This opinion, however, is strongly opposed in the present day. Among other opinions, Abinzoar, in the 12th century, sug-

gested the idea that the disease originated from an insect which he first described. His opinion was revived by Gabucinus, Ingrassias, Laurence, Joubert, and some other writers of the 15th and 16th centuries, all of whom saw and described the insect, which they regarded as an *Acarus*. A fact adverse to this opinion, however, was pointed out by Moutet, who found that the insect did not reside in the vesicles or pustules, but only near them; an observation that has been subsequently confirmed. There is now no doubt of the existence of the insect; it was examined under the microscope, and figured, first by Hauptmann in 1654, then by Bonomo in 1683. But, although the existence of the insect cannot be doubted, yet there are many reasons for not regarding it as the origin of the disease; although the disease may be transmitted by the conveyance of the poison necessarily attached to the insect when it passes from one person to another. I would refer those who are desirous of tracing the discovery, the nature, and the habits of this animalcule, to a most excellent monograph on the subject, at the end of Mr. Erasmus Wilson's *Treatise on Diseases of the Skin*. But awarding to Mr. Wilson the merit which he justly deserves, . . . I cannot accord with him in attributing the disease to that insect. Were scabies of insect origin, we must suppose that the ova floating in the atmosphere would be deposited and find their nidus, and the insects generated from them their proper food, in numberless individuals: hence we should have daily and numerous instances of spontaneous scabies, which, if it ever occurs, is very rare."

Impetigo, the author observes, is often mistaken for scabies. That the insect has a closer relationship with the disease than is here admitted by Dr. Thomson, we consider the whole history of the animalcule to testify. Granting that the entry of the insect into the skin of one individual may convey the fluid from the body of another, and so be the means of performing the veritable inoculation for which Dr. Thomson contends; yet this only more strongly confirms the view of its being the cause of the disease; and reasoning by way of exclusion, in what other relation has the author shown the insect to stand to the disease? The existence of the insect is admitted; its residence also in the itch-vesicle is admitted, or it could not be the medium of the transmission of the morbid fluid; it is not found elsewhere;—it must

therefore either be a cause or a consequence; how the latter, we can in no way imagine, except by supposing the presence of ova on everybody's skin, ready to be developed into acari when the right sort of eruption shall occur.

Dr. Thomson's observations on the symptoms and diagnosis of cutaneous diseases are comprehensive yet precise: these are in every case followed by the consideration of the cause of each disease, and this in turn is succeeded by the study of its prognosis and treatment. In discussing the latter, the author's long pursuit of the study of *materia medica* and therapeutics furnishes him with an abundance of means of treatment. We cannot, however, concur in the frequency or extent to which the author advocates depletion, both local and general.

Among Dr. Parkes's contributions to this volume, we may observe that the chapters on Elephantiasis, and on Syphilitic Eruptions, are peculiarly exact. In point of strictness, the latter should not have been disjointed from analogous forms of disease noticed in previous classes: a notice of their special characters under the head of cause, in some previous order, would have done less violence to the author's arrangement. In like manner we can see no reason why Acne and Sycoosis should not have found places in one or other of the preceding classes or orders.

The inconveniences, however, which would otherwise have been experienced from a faulty classification, are very much obviated by the copious index which the editor has furnished. In the performance of his editorial duties, Dr. Parkes has faithfully represented the author's views, and used his utmost efforts to render the work acceptable to the profession. We entertain no doubt that many will find this work a useful assistant in a class of cases most tedious in their course, and difficult of management: they will, as we have already observed, find in it copious and valuable details in diagnosis and therapeutics.

The Principles of Surgery. By JOHN A. ORR, A.B., F.R.C.S.I., one of the Surgeons of the City of Dublin Hospital. Small 8vo. pp. 496. Dublin: Fannin. London: Churchill. 1850.

THIS volume, which is published in the style of Churchill's *Manuals*, is intended

to set before the student, in a condensed form, the present state of surgical science. The following extract from the preface will afford some indication of the nature and extent of the author's intentions in its publication:—

"To make a strictly scientific classification of the various subjects that require notice in a treatise on surgery, would perhaps be impossible. The arrangement which I have adopted is at least simple. In the first part, the theory and management of the inflammatory process are detailed; in the second, the various kinds of injuries, commencing with wounds and their consequences; then fractures and luxations, burns, and scalds; and, lastly, contusions, which I have placed in that position from having adopted Velpeau's classification of those injuries. The third part includes the diseases commonly called surgical, beginning with those which affect the constitution at large, and proceeding to those which affect particular systems and organs.

"To the conclusion of each chapter I have appended the names of some of the principal authors who have written on the subject described; and, in enumerating different modes of treatment, I have generally added the name, either of the inventor, or of the most distinguished authority who has adopted it. In many cases I have introduced considerable quotations from the best authorities, which may occasionally appear rather abrupt; but I preferred giving the *ipsissima verba*, even with this disadvantage, to remodelling the language of another writer."

Having looked through this treatise, we can speak favourably of it as a compendium of the most recent information on the science of surgery, and on the treatment of surgical diseases and accidents.

The Causes, Symptoms, and Treatment of Eccentric Nervous Affections. By WILLIAM JOHN ANDERSON, F.R.C.S. 12mo. pp. 199. London: Churchill. 1850.

THIS work is composed of a series of cases selected for the most part from the practice of Dr. Seymour at St. George's Hospital, with observations or commentary thereon; and is dedicated to the same physician as a "grateful acknowledgment of the kind and valuable instruction received from him by his former pupil, the author." It consists of six chapters, which treat of—

1. The intimate connection between the nervous and circulating systems; a general description of the phenomena of the circulation, and of assimilation, and of the nervous system. 2. The difference between centric and eccentric nervous affections; with illustrative cases. 3. Hysteria and hypochondriasis; their causes, treatment, &c. 4. Epilepsy. 5. Collapse, from shock, delirium traumaticum, and tetanus. 6. Delirium tremens and hydrophobia.

We commenced the perusal of this book with the hope, from its title and the promises of its preface, of learning something which we did not before know on the subjects on which it professes to treat; but we have been disappointed therein. We are, indeed, entirely at a loss to discover the object of the publication of a work so devoid of originality, otherwise than as a literary exercitation for the author's own benefit, and the opportunity it affords for a dedication.

Proceedings of Societies.

ACADEMY OF MEDICINE, PARIS.

August 12, 1850.

Disease of the Urinary Organs.

M. CIVIALE read a report on a case of chronic inflammation of the bladder which had been transmitted by M. Mordret, surgeon of the Military Hospital of the Isle of Oleron.

The patient was thirty-eight years of age, had suffered from infancy with difficulty in micturition. Complete retention proved fatal. After death the bladder was found to be hypertrophied, multilocular, and the seat of extensive inflammatory disease.

M. JOBERT DE LAMBALLE reported on two cases of the operation for stone transmitted by Dr. Cazenave, of Bourdeaux, in which, on account of the size of the calculi in the one case, and of the presence of a tumor in the other, it was necessary to have recourse to numerous unusual incisions.

Preparation of Pills of Iodide of Iron.

M. LECANU read a report on the means proposed by M.M. Gille and Blanchard to protect iodide of iron, in the form of pills, from the action of the oxygen of the air,

and which consisted in varnishing the pills with a solution of balsam of tolu in ether.

SURGICAL SOCIETY OF PARIS.

August 7, 1850.

Amputation of both Superior Maxillary Bones.

M. MAISONNEUVE had performed this operation on a man, aged sixty-nine years, suffering under cancer of the bones. The same operation had been successfully performed on a young girl affected with necrosis from the vapour of phosphorus. M. Maisonneuve detailed the steps of the operation, and its result, in the first case. A vertical incision was made in the middle line, dividing the nose from its root, and terminating at the upper lip; a transverse incision was then made from the internal angle of the right to that of the left eye; two large quadrilateral flaps were thus left to be dissected down, following the edge of the eyelid to the external angle on both sides: when these were everted the disease was fully exposed. A chain saw was passed through the spheno-maxillary fissure; another was passed through the root of the nose, traversing the os unguis on both sides. In this manner the bones were detached laterally and superiorly. Posteriorly, the velum palati, which was healthy, was detached with a bistoury: then, by the help of Liston's forceps, the entire mass of the bones was detached. The result of the operation at first promised favourably, but in a few days the patient died.

Disease of the Bones of the Hand.

M. CHASSAIGNAC presented a case of disease of the bones of the hand, the nature of which was somewhat obscure, and in which, as symptoms of tubercular disease of the lungs were also present, it became a question whether amputation should be had recourse to. After some discussion, it was deemed advisable to administer internal remedies, and to postpone the operation for the present.

ACADEMY OF SCIENCES, PARIS.

August 12, 1850.

Physiology of the Heart.

M. FATON transmitted an essay containing his researches on the sounds and movements of the heart. The following are among the chief conclusions at which the author had arrived:—

The entire volume of the vascular system

of the thorax undergoes no sensible change from the play of the different parts of the heart whilst that organ performs one complete beat; the heart itself undergoes but little change of form or situation by the contraction of its different parts. The changes in the capacity of the auricles and ventricles result principally from the displacement of the auriculo-ventricular septum, which is more extensively displaced than either of the walls of the heart during its movements. The dilatation of the cavities results from the antagonism of the fibres on each side of this septum, from the tendency to contraction in the lungs, and from the afflux of blood into the cavities of the heart. The base only of the ventricles is displaced during their systole. The heart exerts the action of suction on the veins, by which it influences the circulation in these and the lymphatics. The heart and thoracic vessels are augmented slightly in size during inspiration, and diminished during expiration: the respiratory movements contribute to the force and rapidity of the circulation of the blood. The rapidity of the circulation is not in relation to the frequency of the pulse. The venous blood continues to flow into the heart during the contraction of the auricles. Expiration produces no reflux in the retrograde course of the circulation.

Vesico-uterine and Utero-vesico-vaginal Fistula.

M. JOBERT DE LAMBALLE submitted a memoir, in which he stated, as the result of his researches into the nature and treatment of these accidents, that the injuries thereby incurred, and which have been hitherto regarded as incurable, are no less curable than vesico-vaginal fistulae, by the same means as employed for the latter, by himself.

Presence of Urea in the Liquor Amnii.

M. BALARD stated, in the name of M. Regnaud, that the latter chemist had, by the qualitative analysis of some carefully procured liquor amnii, obtained pure crystallized urea from that fluid.

Cholera.

M. VILFRAU presented a memoir by Dr. Pellarin which proved the contagiousness of cholera, from facts attending its importation into Finisterre from Brast. The author cited numerous other examples of the spread of the disease in hospitals concurrently with the reception of cholera patients where it had not before appeared.

Correspondence.

ON THE INFLUENCE OF FOOD IN THE DEVELOPMENT OF OXALATE OF LIME IN THE SYSTEM. BY C. B. ROSE, ESQ. F.R.C.S. [COMMUNICATED BY DR. GOLDING BIRD.]

To Dr. Golding Bird, F.R.S.

MY DEAR SIR,—The perusal of your valuable work on "Urinary Deposits," resuscitated my early taste for similar inquiries, and has led me to watch, with renewed industry, the changes that are constantly taking place in the secretion from the kidney, both in health and disease. Your "discovery of the comparative frequency of oxalate of lime in the urine in fine and well-defined octohedral crystals, and of the connection between the occurrence of this substance and the existence of certain definite ailments, all characterised by great nervous irritability,"* has induced me to pay attention more particularly to that form of abnormal secretion.

After examining what has been written by yourself and others on the deposit of oxalate of lime in the urine, since your observations first appeared, it has occurred to me that it would be a useful inquiry to ascertain how far the elimination of oxalate of lime by the kidney was the effect and a pathognomonic sign of a *certain definite ailment*, or how frequently it was the consequence of a form of dyspepsia merely, or in what degree it was dependent upon the ingestion of certain articles of diet; and I have therefore, for some length of time, undertaken and pursued this inquiry, but more especially with reference to the last point.

I have been in some degree led to question the opinion that a connection *invariably* obtained between the occurrence of oxalate of lime in the urine and the "existence of certain definite ailments," by a case that I will relate, in which the oxalate of lime continued to be eliminated by the kidneys for months after the patient was, to all appearance, in perfect health; and, further, from having ascertained that certain vegetables and fruits in daily dietetic use were capable of inducing or of leading to the production of a like deposit.†

* Urinary Deposits, 1st edit., p. 123.

† Since writing the above, I have met with the following observation of that profound philosopher, Dr. W. H. Wollaston, which makes manifest how greatly he was in advance of the chemistry of his time, as applicable to pathology and therapeutics. "With respect to the mulberry calculus, I fear that an intimate knowledge of its properties will leave but small prospect of relief from any solvent; but, by tracing the source of

CASE.—On July 25th, 1849, I was requested to see a lad, eleven years of age, who had been ill some weeks, with symptoms of dyspepsia: I found him looking sallow, so much so, indeed, that I at the moment thought he must be labouring under an attack of jaundice. His parents said that he had wasted considerably. All that I could learn respecting his state of health during the period of his indisposition was, that several months previously he was attacked with diarrhoea, and that he had been in a state of *malaise* ever since. At the present time his appearance was dull; his complexion was very peculiar; his skin looked dirty; indeed, his mother had repeatedly told him that he did not wash his face; his tongue was covered with a dirty-coloured fur; his abdomen was tumid, but his bowels were tolerably regular, and his faeces were quite of a healthy character; his urine possessed a dark amber colour, exhibited a slight mucous cloud, gave a considerable deposit of uric acid crystals, with a slight excess of urea; sp. gr. 1027. On examining it in the usual way for oxalate of lime, I met with an abundance of its crystals. I prescribed for him a saline diuretic mixture; an alternative, with a diaphoretic, at bed-time.

July 28.—My notes inform me that the lad looks briske in the face, and with a less dirty complexion: he passes nearly two pints of urine in the twenty-four hours, of not so dark an amber colour; sp. gr. 1025; has a large deposit of uric acid, as red sand; gives a considerable excess of urea, and shows an abundant crop of crystals of oxalate of lime; the urine also exhibits a highly acid reaction with litmus.—Prescribed five minims of Tincture of Sesquichloride of Iron, in Infusion of Quassia, three times a day.

August 2nd.—The patient is looking better; his complexion is less sallow, and he is more lively; his appetite is good; his body not now tumid; his bowels are regular, but his tongue continues furred; urine not so highly amber-coloured; sp. gr. 1027; abundance of uric acid crystals; urea in excess, but not quite so much oxalate of lime; a few large crystals were exhibited.—Ordered five minims of the Wine of Colchicum to each dose of the Chalybeate.

5th.—The lad appears more cheerful, and eats well; he never touches

the disease, we may entertain some hopes of preventing it. As the saccharine acid is known to be a natural product of a species of oxalis, it seems more probable that it is contained in some other vegetables, or their fruits, taken as aliments, than produced by the digestive powers, or secreted by any diseased action of the kidneys. The nutriment would therefore become a subject of minute inquiry, rather than any supposed defect of assimilation or secretion.—See Philomoph. Trans. (abridged), vol. xviii. p. 220, 1797.

cooked vegetables or fruits; he still retains a considerable degree of sallowness; his urine has the same amount of uric acid sediment, and excess of urea; oxalate of lime quite as abundant; dumb-bell crystals exhibited with the octohedral.—He is to continue taking the colchicum and chalybeate.

12th.—The lad's complexion is much better, and his health is altogether improved: urine paler, with less deposit of uric acid; sp. gr. 1030; much less oxalate of lime visible, and no excess of urea.—Discontinued my regular attendance.

October 25th.—I called on the lad, who says he is quite well: he eats, drinks, and sleeps well, and appears to have regained his usual spirits, as when in health; his complexion is clear. I procured some of his urine, and found it of a pale sherry colour, with a slight mucous sediment, and deposits but little uric acid; sp. gr. 1020. There is at this time a great abundance of oxalate of lime in both octohedral and dumb-bell crystals. He is taking no medicine.

April 23, 1850.—I examined the urine of the lad, who continues to enjoy good health: his night's and morning's urine amounts to 18 ounces; it is of a pale sherry colour, with slight uric acid sediment; sp. gr. 1025; it contains a considerable excess of urea, and an abundance of oxalate of lime, in minute crystals, without any of the dumb-bell form. This lad's daily diet is bread and butter and tea for breakfast, rarely milk; meat, with pudding or dumpling, for dinner, seldom any beer; bread and butter, with tea, again for supper. He eats occasionally, but not frequently, portions of raw turnip and carrot: he had not eaten either at the time of my last examining his urine, and he never, by any chance, eats cooked vegetables of any kind; he is not a greedy or voracious eater.

The peculiar feature of the above case, as you will observe, is the habitual elimination of oxalate of lime by the kidneys, the individual enjoying, in all other respects, perfect health. Can it then be said that the formation of oxalate of lime in the animal fluids is *invariably* a morbid condition?

A nearly similar immunity from morbid symptoms has been noticed by the late Dr. Prout and others. The former observed "that the peculiar symptoms attending the oxalate of lime diathesis vary exceedingly in degree as well as in kind. Sometimes they are so slight as to pass unnoticed by the patient; or at most are considered as dyspeptic derangements of no great importance"*. Dr. Shearman says "the appetite

* Stomach and Urinary Diseases. 3rd edition, page 62.

is often good, and it is almost impossible to get them (the patients) to acknowledge they have any derangement in their organs of digestion.* Mr. Stallard, also, when describing a case of irritable bladder, in which he detected "*large octohedra of oxalate of lime*" in the urine, says "the general health is exceedingly good; has no dyspepsia, and complains of neither depression of spirits, or any of the symptoms described by Dr. Golding Bird as the usual characteristics of the oxalate of lime diathesis."†

The fact of the continuance of apparent health during the elimination of oxalate of lime by the kidneys, as recognised by the above-named observers, and also brought to my own notice by the case related, has excited me to look for some other cause or source of this unnatural element than mal-assimilation from ill-digested food, through redundant quantity, or irreducible quality, or diminished power in the digestive organs.

Aware that the edible rhubarb invariably led to the appearance of oxalate of lime in the urine of persons partaking of it, without their experiencing any appreciable derangement of the digestive function, I thought it not improbable that other articles of diet, both vegetables and fruits, in common use might be productive of a like effect. I therefore determined on putting this idea to the test, by instituting a series of experiments; and wanting the opportunities which an hospital affords, I practised them upon myself—which circumstance, I considered, would certainly not render them less satisfactory: and I will now briefly recount these experiments to you, as minute detail would be more tedious than useful.

Having first assured myself that the rhubarb quickly and invariably led to the appearance of the crystals of oxalate of lime in my urine, the plan I adopted was to partake of the selected article at dinner, with my usual form of mutton or beef, &c., drinking water only; then, at the expiration of two and a half to three or more hours, before taking tea with sugar, I passed my urine, which was set aside till the following evening, and its lower stratum then examined, by warming, &c., and the use of the microscope, in the manner you have directed.

I made my first essay of *other aliments* on the *leek*;‡ and in November 1848, I ate

at dinner heartily of boiled leeks, and found an abundance of oxalate of lime in my urine, passed about three hours afterwards.

Jan. 10th and 11th, 1849.—I partook of leeks both days, but no trace of the oxalate was found in the urine passed on either of the days.

Feb. 5th. Ate leeks and no sweets; my urine, passed three hours afterwards, contained a great abundance of oxalate of lime, many crystals as large as if I had eaten rhubarb. The urine made immediately before dinner contained none.

*Onions.**—Partook largely of onion-sauce at dinner, and found in the urine passed three hours after, mixed with some made after taking tea, a few small crystals of oxalate of lime.

After eating, on two successive days, at dinner, largely of *Portugal* onions, my urine, passed from three to six hours after on each day, and put together, examined in the usual way, exhibited an abundance of oxalate of lime in both large and small crystals. These experiments were made in the autumn of 1848.

In September of 1849, after eating heartily of *common* onion-sauce with boiled rabbit, on trying my urine I could not find any oxalate of lime.

November.—I ate plentifully of *Portugal* onions boiled; in the urine passed three hours afterwards I found a very few small crystals of the oxalate: in this instance the urine was examined on the evening of the day on which the onions were eaten; there were none in the urine passed just before dinner. On allowing the urine for this experiment to stand for twenty-four hours before examination, it exhibited an abundance of oxalate of lime. The urine made in the morning *subsequent* to eating the onions furnished none of the oxalate.

Knowing an individual who ate almost daily two or three small raw onions, as an antidote to gravel, I procured some of his urine, made on the day he had eaten the onions, but I could not detect any oxalate of lime in it. There was a copious deposit of crystals of uric acid.

Turnips.—November 1848, I ate heartily of boiled turnips with my meat at dinner, and my urine afterwards gave an abundance of oxalate of lime.

February 1849.—Partook in moderation of boiled turnips with boiled beef; the urine passed three hours after gave an abundance of small crystals of the oxalate.

December 1849.—After eating heartily of boiled turnip, I examined my urine at the usual time, but discovered no oxalate of lime. Probably it ought to have stood

* On the Value of Urinary Diagnosis in the Detection and Treatment of Disease: *Provincial Med. and Surg. Journal*, page 529, 1845.

† LONDON MED. GAZ. vol. ii. page 118, 1846.

‡ Allium porrum. Pereira, speaking of the various species of Allium, says "their virtues are analogous to those of the onion and garlic;" and, on the authority of Fourcroy and Vauquelin, he states that the onion contains uncrystallisable sugar: it is, therefore, probable that the leek also contains sugar.

* Mr. John Quékett informs me that he has discovered crystals of oxalate of lime in the onion.

longer before examination. I occasionally tested the urine on the same evening that it was passed.

January 25th, 1850.—Ate heartily of boiled turnip with mutton; my urine afterwards gave an abundance of *large* and *small* crystals of the oxalate, whilst the urine passed just *before* dinner contained a very few *small* crystals only.

Parsnips.—April 1849: ate heartily of boiled parsnips, and no sweets; examined my urine, passed three hours afterwards, and found an abundance of the oxalate of lime.

February 15th, 1850.—Ate three small parsnips at dinner, and my urine afterwards gave a few *large* crystals of the oxalate.

18th.—I experienced symptoms of slight dyspepsia, and slept badly; my urine, passed immediately *before* dinner, exhibited a great abundance of *minute* crystals of the oxalate. I ate rather heartily of boiled parsnips, with boiled mutton; my urine afterwards gave fewer *small* crystals, with several *large* ones.

Carrot.^{*}—August 17th and 18th: I partook of boiled carrots each day at my dinner; on the 19th, in the evening, I examined the urine passed after dinner on the 18th, and found a few *small* crystals, with two *large*ish ones, of the oxalate.

September 18th.—Ate boiled carrots with my dinner, and a few crystals of oxalate of lime appeared in my urine.

February 1850.—Ate two large boiled carrots at dinner: my urine, passed three hours afterwards, gave a few *large*ish crystals of the oxalate; the urine passed immediately *before* dinner exhibited none.

The ingestion of *cauliflower* or *asparagus* has not, with me, given rise to the oxalate of lime in the urine. Of those vegetables, after the eating of which oxalate of lime is eliminated by the kidneys, I am inclined to believe that after rhubarb, leeks, Portugal onions, and turnips, lead to the production of the largest quantity. Parsnips, and then carrots, appear to produce a smaller quantity. It appears to me, also, that a greater number of *large* crystals are met with after those things have been eaten which in the highest degree tend to the production of oxalate of lime.

Apple.†—Apple pudding and tart, with or without sugar, taken at dinner, invariably gave an abundance of oxalate of lime in my urine, there not having been any in that secreted *before* dinner. Once, having eaten at dessert three raw London

pippins, I examined my urine passed from four to six hours afterwards, but found no trace of oxalate of lime. There was a large quantity of urate of ammonia; the specific gravity of the urine 1025.

Red currants.—I ate, three successive days, of currant pudding at my dinner. The fruit was very acid, requiring sugar. My urine, passed before taking tea, gave a great quantity of oxalate of lime. There was no excess of urea.*

Orange.—January 1850: after eating three oranges, my urine, passed before tea, gave an abundance of *large* and *small* crystals of the oxalate: the urine made just *before* dinner exhibited many *small* crystals; but those after taking the oranges were greatly increased in number and magnitude.

February.—I ate four oranges after a plain dinner, and an abundance of *large* crystals of the oxalate were found in my urine made afterwards. There were none in that passed *before* dinner.

Grapes.—March 1850: I ate at two o'clock a dinner of two mutton chops, potatoes, and Stilton cheese, and drank half a pint of table beer; examined my urine, passed at six P.M.: it contained an abundance of oxalate of lime; it was taken from the lower stratum of ten fluid-ounces of urine. At seven o'clock the same evening I ate nearly a pound of Malaga grapes. I examined the lower portion of five fluid-ounces of my urine passed three hours afterwards: it contained about the same proportion of the oxalate as in the first specimen; the only difference between the two was, that there were several *large* crystals in the last. I afterwards examined the lowest stratum of the urine (20 ounces) passed the following morning at eight o'clock: it exhibited a great abundance of *small* crystals of the oxalate, but no *large* ones. I was not aware of being dyspeptic at this time; still, I am certainly subject to slight derangements of the digestive function, and which will probably account for the following state of things:—

On April 4th of the present year, I examined my urine passed *before* dinner: it contained an abundance of oxalate of lime.

I examined the urine passed three and a half hours after my usual plain dinner: there appeared rather more oxalate, and a few *large* crystals.

I examined the urine passed on the following morning, and found in it a *great* abundance of *small* crystals of the oxalate.

I will now give you the results of exami-

* Pereira informs us, on the authority of Wackenroder, that the expressed juice of the carrot contains uncrystallisable sugar, with some starch and malic acid.

† Mr. Quekett has detected crystals of oxalate of lime in the apple.

* My urine usually contains a full proportion of urea, apparently an excess, not absolutely so; for as I commonly take but a small quantity of fluids, my urine is proportionably concentrated.

nations of the urine made after two or three instances of excess in my ingesta:—

May 7th, 1849.—I dined at an inn: partook of salmon, soup, roast beef, and potato, chicken, sweet pudding, boiled custard, and cheese; drank moderately of Bucoallas, sherry, and port; afterwards took coffee. Examined the next morning's urine, and found a great abundance of the oxalate of lime in it.

8th.—Dined to-day at home upon roast mutton, and ate rather largely of boiled cauliflower, but no sweets. My urine passed immediately before tea exhibited no crystals of the oxalate.

24th.—Dined at an inn: ate fish, soup, mutton, green goose, custard, lemon-cream, and cheese; drank Bucoallas and port; afterwards took tea. Examined the urine passed eight hours after dinner, and found a great abundance of crystals of uric acid, but none of the oxalate of lime.

About a week after the last date I dined with a party at my own table; partook of the usual routine of fish, soup, solid meat, and plum pudding; no pastry; and drank in moderation of sherry and port only. The urine of the following morning, sp. gr. 1022, contained my usual proportion of urea, no excess of uric acid, nor any oxalate of lime.

On reference to certain dates in this communication, it will be seen that the oxalate of lime was not always found in the urine made *after* dinner, although it had previously appeared in the secretion of that period under apparently similar circumstances: for instance, January 10th and 11th, 1849, after eating leeks; September 1849, after onions; and December 1849, on eating turnip. Might not the oxalate have been met with in the urine of the following morning, had it been examined? Dr. Shearman, in a paper from which I have before quoted, makes the following remark:—"It is commonly found that the oxalates are more plentiful in the urine after dinner, or in the urine chylif, than in the morning, or urine sanguinis; and, as the cause of the deposit is supposed to depend on mal-assimilation of the food in the stomach, this seems to be the most likely time to find it. But here (Case 1st) the deposit of oxalate of lime was never found in the urine after any meal, but was always most plentiful in the urine voided in the morning."*

Herein I have laid before you all the data I possess upon this subject, acquired from the experiments my leisure has enabled me to make: they are faithfully recorded, however imperfectly they may

have been prosecuted. I forbear to comment or reason upon them, being conscious of my incapacity for so doing, from my want of a sufficiently intimate knowledge of the metamorphoses and mutations constantly taking place in the proximate elements of the secretion of those scavengers of the animal body—the kidneys.

It will afford me much gratification, and amply repay me for the trouble I have taken, should the results of the above inquiry prove of any practical utility; and I cannot but think that their application in the treatment of cases of oxaluria will at least remind us of the importance of not being satisfied with a *general* direction, but prompt us to prescribe a *special* form of diet, as a necessary adjuvant to our therapeutics.

I am, my dear sir,

Very faithfully yours,

C. B. ROSE, F.R.C.S. &c.

Swaffham, June 27th, 1850.

EVIDENCE ON INTRAMURAL INTERMENTS. UNJUST ATTACK ON THE MEDICAL PROFESSION.

SIR,—Permit me to call your attention, and that of your readers, to the following unwarrantable statements, which I find in the evidence taken by the Board of Health on the Intramural Interment question.

A Mr. Sadler, an undertaker, states, that "it is known that surgeons take commissions" (on funerals); that "one of the evils of the present system is the taking of commissions"—reiterating the charge; "there are medical men who take commissions."

If words mean anything, these words distinctly imply that the taking commissions from undertakers is common among London medical men. The utter want of truth in such a charge, as applied to the profession generally, it is scarcely necessary to point out; but it will be made clearly apparent to any one who will take the trouble of inquiring among his professional brethren, as I have (somewhat superfluously) done. There will not be found an exception to the rule that every one will give an indignant denial to such a charge.

In all my life I had never before heard of such a thing; I therefore took the trouble to inquire of an undertaker, carrying on at this present time a very large business, and he assured me that neither he himself, nor any member of his trade with whom he had spoken on the subject, were aware of any such transactions until they met with Mr. Sadler's assertions; adding, at the same time, that no respectable

* Cases of Oxaluria, by Dr. E. J. Shearman: LONDON MEDICAL GAZETTE, New Series, vol. ii. page 408, 1846.

tradesman would venture so to insult a professional man.

So much for this assertion, that the taking of commissions by surgeons is "*known*." Even if Mr. Sadler has found some "surgeons" with so low an estimate of professional honour as to submit to such a degradation at his hands, still these exceptional instances afford no ground for affixing this stigma of disgrace upon the entire class; such transactions having been *unknown* and secret compacts. If Mr. Sadler had thought the proceeding honourable or professional, he would have given the names of those surgeons who have so lowered themselves and their profession.

Unless this disgraceful charge be substantiated by the proof alluded to—the publication of the names of the commission-takers—it may be fairly assumed that the whole statement is a mere unadulterated fiction, invented to remove censure from the real authors of exorbitant funeral expenses.

I should not have intruded upon your space by noticing so evidently false and calumnious a charge, had it not been dragged from its merited obscurity in the pages of a *blue book*, by a professional publication, the *Edinburgh Monthly Journal of Medical Science*, in which the London surgeons are represented as finding in this way a *solatium* for the results of unsuccessful operations! As a matter of course, I should expect to find this passage copied into the *variétés* of the continental journals. It is therefore needful to give it thus publicly an unqualified and direct denial.—I remain, sir,

Your obedient servant,

W. B. KESTVEN.

Upper Holloway, Aug. 12, 1850.

INFLUENCE OF VACCINATION ON AN EPI-
DEMIC OF SMALL-POX. BY DR. LOMBARD.

IN the department of L'Ariège, where small-pox has been prevalent for several years, and recently with much severity, a hundred and twenty vaccinations were practised in a period of six weeks. Of this number of infants not one was attacked with small-pox, while others died. Even where small-pox was developed to a certain extent, vaccination exerted a favourable influence. M. Lombard states that in those persons in whom variola appeared, and who had been vaccinated some time previously, the severity of the disease increased with the length of that interval. M. Lombard concludes in favour of re-vaccination.

—*Gazette Médicale*.

X

Medical Intelligence.

HONOURS CONFERRED ON MEDICAL MEN IN
THE ARMY AND NAVY.

THE Queen has been graciously pleased to make and ordain a Special Statute of the Most Hon. Order of the Bath for the purpose of authorizing the admission into the Military Divisions of the second and third classes of the said Most Hon. Order, of such Officers of the Commissariat and Medical Departments of the Army and Navy, and of the East India Company's Army and Navy, as by their meritorious services have already or may hereafter be deemed by Her Majesty to have deserved such distinction. Her Majesty has accordingly been pleased to give orders for appointing—

Sir James M'Grigor, Bart., Doctor of Medicine, Director-General of the Medical Department of the Army;

Sir William Burnett, Knight, Doctor of Medicine, Director-General of the Medical Department of the Navy; and

James Thomson, Esq., Inspector-General of Hospitals on the Bengal Establishment of the East India Company's Service, to be Ordinary Members of the Military Division of the Second Class, or *Knight Commanders* of the said Order.

And Her Majesty has further been pleased to give orders for appointing the following Officers to be Ordinary Members of the Military Division of the Third Class, or *Companions of the said Most Hon. Order*, viz.:

Duncan McArthur, Esq., Doctor of Medicine, retired Physician of the Fleet;

Sir John Webb, Knight, Director-General of the Medical Department of the Ordnance;

Sir James Robert Grant, Knight, Doctor of Medicine, Inspector-General of Hospitals;

John Gunning, Esq., Inspector-General of Hospitals;

John Robert Hume, Esq., Doctor of Medicine, Inspector-General of Hospitals;

Sir John Richardson, Knight, Doctor of Medicine, Inspector of Hospitals and Fleets;

Benjamin Fonseca Outram, Esq., Doctor of Medicine, retired Inspector of Hospitals and Fleets;

Robert Perkins Hillyar, Esq., retired Inspector of Hospitals and Fleets;

Sir George Magrath, Knight, Doctor of Medicine, retired Inspector of Hospitals and Fleets;

Sir John Liddell, Knight, Doctor of Medicine, Inspector of Hospitals and Fleets;

Henry Franklin, Esq., Inspector-General of Hospitals;

James French, Esq., Doctor of Medicine, Inspector-General of Hospitals;

Stephen Woolriche, Esq., Inspector-General of Hospitals;

Charles Renny, Esq., Superintending Surgeon on the Bengal Establishment of the East India Company's Service;

B. W. Macleod, Esq., Doctor of Medicine, Superintending Surgeon on the Bengal Establishment of the East India Company's Service;

John Wylie, Esq., Doctor of Medicine, Inspector-General of Hospitals on the Madras Establishment of the East India Company's Service; and

Charles Doyle Straker, Esq., Doctor of Medicine, Superintending Surgeon on the Bombay Establishment of the East India Company's Service.

THE ATMOPYRE, AND ITS PRACTICAL USE AS A SOURCE OF HEAT.

A VERY ingenious mode of burning a mixture of coal-gas and air has been recently suggested by Mr. D. O. Edwards, surgeon; the object of which is to economise heat, and produce, without waste of fuel, a maximum of heat at a minimum of expense. For this purpose Mr. Edwards employs a small cylinder of pipe-clay, varying in length from two to four inches, perforated with holes the fiftieth of an inch in diameter, in imitation of the gauze of Davy's safety-lamp. The cylinder has a circular opening at one end, which fits exactly upon the burner No. 3, commonly used by gas-fitters. Gas is introduced into the interior of the cylinder, with the air of which it becomes mixed, forming a kind of artificial fire-damp. This mixture is ignited on the *outside* of the vessel, and it burns entirely on the exterior of the earthenware, which is enveloped in a coat of pale blue flame. The clay cylinder soon becomes red hot, and presents the appearance of a solid red flame. All the heat of combustion is thus accumulated on the clay, and is thence diffused by radiation and connection.

A cylinder of this kind, called by Mr. Edwards "a hood," is heated to dull redness in the course of a minute; but an aggregate of "hoods" placed in juxtaposition, and enclosed in an earthenware case, are heated to an orange colour, and the case itself becomes bright red. The case is fire-proof, being made of two-thirds Stourbridge clay and one-third "frit," or Stourbridge bricks pulverized; and the hoods are best made of a proportion of one-third China clay to two-thirds common pipe clay: they are blackened by one-third the proportion of black oxide of copper. For all common purposes, two-thirds of

fresh pipe clay, with one-third of pipe clay already burnt, would be thoroughly efficient.

As to economy, Mr. Edwards tells us that each hood consumes five-sixths of a cubic foot of coal-gas per hour, and a battery of eight hoods (consuming about seven cubic feet of gas) is sufficient to warm a good-sized room measuring about 4000* cubic feet, at a daily expense of about sixpence.

The numerous perforations in each hood, amounting to 90 on the average, secure a due supply of oxygen and the perfect combustion of fuel, the products of which may be entirely carried out of the room by a pipe attached to the apparatus. In this way, also, the warming may be conjoined with ventilation. Too much air must not be admitted among the hoods, for this would have the effect of cooling and reducing the amount of heat. Coal gas requires about 12½ times its volume of air for complete combustion. Any quantity of air beyond this proportion, acts as a vehicle to carry off the heat, and prevents its accumulation to a high intensity.

An earthenware case enclosing a battery of twelve hoods, becomes heated to 400° or 500°, and thus forms a safe and constant repository of heat. This may be placed in an outer case of china, terra cotta, or common ware. The fresh air is brought from the outside of the house through a large pipe about six inches in diameter, which communicates, by means of a valvular iron plate, with the space contained between the two cases. The outer case being perforated at top and bottom would allow a current of air to traverse it,—to become heated by contact with the inner case, and thus diffuse an equable temperature through an apartment. Each corner of the room might have its warming apparatus; while the supply of air, and therefore of heat, might be controlled by a valve in the iron pipe which brings it from the outside.

•• There is nothing new in the principle of the atmopyre. Chemists have been for some years in the habit of burning coal-gas mixed with air by the aid of a metal cylinder having a wire gauge top. Mr. Edwards has suggested the use of clay for the purpose of retaining and diffusing the heat. So long as the products of combustion are carried out of the apartment, this mode of procuring warmth from gas is safe, cleanly, wholesome, and unobjectionable. The question of its relative economy has yet to be settled by a practical application of the atmopyre on a large scale.

It does not appear very clearly from the

* A room 20 feet long by 15 wide, and 13 feet high, would have nearly this capacity.

description, how the aggregate of hoods are arranged with respect to the *first* which should fit the gas-burner exactly. Mr. Edwards tells us they should be placed in "jurisdiction." Some additional information on this point is desirable, also where the hoods may be seen or procured.

It appears to us that the plan of warming suggested by Mr. Edwards will be found highly useful if a sufficient supply of gas at a cheap rate can be procured.

STATISTICS OF POISONING.

A RETURN has been published relative to the number of poisoning cases tried in the United Kingdom from 1839 to 1849 inclusive. The number of persons tried for this crime during the above period, at the Central Criminal Court, has been 33, of whom 18 were men and 15 women; 16 were tried for murder, of whom five were convicted, and 17 for attempt, of whom 10 were convicted. In the Home circuit 8 women were tried for murder by poison, three for attempt to murder, and two men as accessories before the fact. The number of convictions was three. In the Midland circuit 11 women were tried for administering poison, of whom four were convicted. The number of men tried was six, all of whom were acquitted. In the Norfolk circuit 22 cases of poisoning were tried, in 12 of which the prisoners were female. The number of convictions was nine. In the Northern circuit the number of males tried for poisoning was 15, and of females five, the number of convictions being 14. In the Oxford circuit 17 cases of poisoning were tried, in nine of which the prisoners were women. The number of convictions was three. In the Western circuit, in 12 out of 22 cases of poisoning tried, the prisoners were women, and conviction took place in ten. In the county of Durham there have been no trials for poisoning. In the county palatine of Lancaster, out of eight cases, seven were of women, and conviction was obtained in five. In the North Wales and Chester circuit the number of persons tried for poisoning was 16, of whom 11 were women. The number of convictions was five. In the South Wales circuit five prisoners were tried for poisoning, two of whom were women. Conviction was obtained in one case only, in which a man and woman were implicated.

In Scotland the total number of trials for poisoning, from 1839 to 1849, was 15, in ten of which the prisoners were women; and in seven, convictions were obtained. In Ireland 31 women and 25 men were tried for poisoning during the same period, and convictions were obtained in 14 cases. The largest number of cases occurred during the year 1849, the number of cases being

13 (seven men and six women), the average of the other years being 4·3, in the proportion of 1·8 men to 2·5 women.

THE ASIATIC CHOLERA.

Alexandria and Cairo, August 7.—Several cases of cholera have occurred both at Alexandria and at Cairo, and there is every probability of another visitation of the disease to this country. Letters from Alexandria of the 8th inst. announce that in that city there were eight cases on that day. At Cairo, where it had also broken out, the number of cases averaged from 30 to 35 daily. The Viceroy, who, considering his station and the influence he exercises over the masses, should show more firmness, took to flight from Cairo as soon as the cholera made its appearance there. His Highness would not even come to Alexandria, but ordered a steamer and a frigate, well provisioned with stores of every description, to meet him at Damietta, to which town he proceeded, down the Nile, and there embarked for Rhodes on the 6th. Some cases of cholera had also occurred at Suez among people who had arrived there from Cairo.

Cuba and the Hawaiian, July 27.—Much anxiety is felt, and a great degree of uncertainty exists, as to the progress of the cholera in the interior of the island, and as to its probable effects upon the next and subsequent crops. This disease has not yet become general, but it continues to spread in various directions, and its ravages in many cases have been appalling. On several estates the destruction of life among the negroes amounts to 25 and 30 per cent., and in some cases even 40 and 50 per cent. of the whole number, and the most efficient are generally the victims. Should the disease diffuse itself throughout the island, the loss of life among the slave operatives would in this ratio probably not be short of 100,000 to 125,000. Even at the present average rate of mortality on the estates, the estimates as to the ultimate destruction of life among the negroes, in the event of the epidemic becoming general throughout the interior, are not less than 70,000 or 80,000. The period of the year is, unfortunately, not favourable for checking the malady. The labourers carried off are an irreparable loss, because there is no source available at present from which they can be replaced.

Malta.—The cholera still pursues its deadly course. Many hope it is on the decline, but it is to be feared that it is only subsiding in places where its ravages have been most severe, to commence in places hitherto free. Within the last few days we have had cases more frequent in Valetta, though as yet not very numerous. The progress

of the disease is dreadfully rapid. On Thursday afternoon Captain M'Queen, of the Royal Artillery, who has but recently joined the garrison, was attacked. At half-past 2 A.M. on the following morning he breathed his last, at the early age of 27 years.

Schleswig.—It is reported that cases of cholera have occurred among the soldiers of the Danish garrison in Schleswig.

Brunswick.—The cholera has been making some fearful ravages in Brunswick since the month of June. According to official reports, as many as eight hundred persons have died.

London.—The latest returns of the Registrar-General show that there is no appearance of an outbreak of malignant cholera in London. The deaths from this disease last week, when the total mortality was considerably below the decennial weekly average, amounted to only eight; and of these, six occurred among children.

Diarrhoea infantum is still very prevalent and fatal. Out of 139 deaths from diarrhoea, there were 118 among children under fifteen years of age.

THE MALIGNANT CHOLERA OF 1832 AND 1849 COMPARED.

WITH reference to the epidemic of 1832-3 we must take the statement as we find it made to the Privy Council, from which it appears that in London the attacks were 14,144, and the deaths 6,729; the population of London then being 1,681,641. From data given in the appendix, it is estimated that in 1848-9 the attacks were about 30,000, and the deaths 14,601; the population at that time being 2,206,076; so that in the last, as compared with the former epidemic, the deaths were more numerous than the attacks, while the attacks were more than double;—or, to state the result more precisely, in the epidemic of 1832-3, one person died in every 250 of the inhabitants, or 4 per cent.; whereas, in 1848-9, one person died in every 151 of the inhabitants, or 66 per cent.: the mortality, therefore, in 1832-3 was about two-fifths less than in 1849, which is the same as to say that in proportion to the population, about 5,800 more persons perished of this epidemic in London in 1849 than in 1832.

In England and Wales in 1832-3, the attacks are stated to have been 71,600, and the deaths 16,487.—*Board of Health Return.*

THE TENDENCY OF CHOLERA TO RE-VISIT THE SAME HOUSES AND STREETS.

As was anticipated and predicted, cholera, during its recent visitation, returned to the same countries, and the same cities and

towns, and even the same streets, houses, and rooms, which it ravaged in 1832. It is true that many places have been attacked in the recent, which escaped in the former epidemic; but very few indeed that suffered then have escaped now. In some instances it has reappeared on the very spot in which it first broke out sixteen years ago. The first case that occurred in the town of Leith in 1848 took place in the same house, and within a few feet of the very spot whence the epidemic of 1832 commenced its course. On its reappearance in the town of Pollokshaws, it snatched its first victim from the same room and the very bed in which it broke out in 1832. Its first appearance in Bermondsey was close to the same ditch in which the earliest fatal cases occurred in 1832. This return to its former haunts has been observed in several other places, and the experience abroad has been similar. At Groningen, in Holland, the disease in 1832 attacked in the better part of the city only two houses, and the epidemic broke out in these two identical houses in the visitation of 1848.

CONTAGIOUSNESS OF CHOLERA.

L'UNION MEDICALE, in one of its leading articles, deprecates the declaration of the contagiousness of cholera by the Academy of Medicine, on the ground of public alarm. Many sittings of the Academy have been devoted to the discussion of the question, and the result is that the proofs of contagion outweigh those of non-contagion. The editor observes, however, that this is the last question that should have been raised; that the attention of the Academy would have been better occupied by the discussion of the various modes of treatment which have been advanced, and the selection of some one plan which should receive public confidence.

We do not concur in this opinion of our valued contemporary: we regard the question of the mode of propagation of this disease, of the very first importance in reference to preventive measures. We know from sad experience that the disease is more amenable to prevention than to cure; and this, in so fearful a scourge, is a most acceptable mitigating feature, and one not to be met with to the same extent in many other epidemic diseases. X

THE MAHA MURREE, OR INDIAN PLAGUE.

THE last Indian mail brings the intelligence that the Maha Murree, or certain death, has again broken out in the hills of Gurhwal and Kamaon. This disease, which has all the appearance and symptoms of the plague of Turkey, is so infectious, or believed to be so by the hill tribes, that it used to be the custom to taboo a village

in which the disease had shown itself, to draw a cordon around it, beyond which, if any of the unhappy residents of the infected place dared to creep out, he was shot like a mad dog. The hills in which this infection almost always shows itself are those at the foot of the great snowy ranges; it disappears as it approaches the outer hills towards the plains; such as the Landour and Gagur Ranges. In Gurhwal and Northern Kemaon it takes a most virulent form, and the visitors of Nynee Tal and Almorah should hesitate in making the usual visits to the snowy ranges whilst this plague is said to be raging in the intermediate country, particularly British Garhwal. The Maha Murree is believed to be highly infectious: it commences with most violent fever, which is soon followed by swellings in the arm-pits and in other parts of the body; it destroys the infected in twenty-four hours generally, though there are some instances where the sufferer has lingered a few hours more. It is supposed that not one in a hundred of those attacked recovers.

MORTALITY ON THE AFRICAN STATION.

FROM a return made up to the year 1845, which is the last we have seen, it appears that the annual ratio of mortality in the African squadron for the period of twenty-one years then terminating, was 58 per thousand; that in the West Indies being 18, in the East Indies 15, and on home stations 9.

THE GENERAL BOARD OF HEALTH.

THE Queen has been pleased to appoint Thomas Southwood Smith, M.D., to be a member of the General Board of Health.

The General Board of Health have appointed Mr. Charles Macaulay, formerly clinical clerk to Sir Benjamin Brodie, and Lecturer on Medical Jurisprudence at St. George's Hospital, to be Assistant-Secretary to the Board.

ON ACUTE POISONING WITH PREPARATIONS OF GOLD, AND ITS TREATMENT. BY DR. HANNON.

THE salts of gold, more particularly the chloride, are, according to Dr. Hannon, among the most dangerous of the metallic poisons.

These salts, administered in very small doses, promote the activity of the digestive functions. A slight increase in the dose excites irritation of the mucous membrane. Administered in large doses, the vascular and nervous systems are excited; painful priapism, headache, delirium, and severe gastro-enteritis ensue: death rapidly follows, unless an antidote be speedily given. The antidote *per excellence* is proto-sulphate of iron. A chloride of iron is formed,

and the gold is precipitated in a metallic state extremely divided, and perfectly inert. Vomiting is then to be promoted, and inflammation or other symptoms treated as they may arise.—*La Presse Méd.* x

UNIVERSITY OF LONDON.—FIRST EXAMINATION FOR THE DEGREE OF M.B., 1850.

First Division.

T. F. Hardwich, King's College.
F. W. Headland, King's College.
T. K. Hornidge, St. George's Hospital.
J. Lister, University College.
F. W. Pavy, Guy's Hospital.
T. M. Rooke, Guy's Hospital.
H. Simpson, University College.
A. D. Smith, King's College.
J. H. Trouncer, University College.
T. B. Washbourn, Guy's Hospital.
J. D. Weaver, University College.

Second Division.

W. Brown, King's College.
P. L. Burchell, Westminster Hospital.
R. H. Courtensay, Richmond Hosp. Dublin.
E. Jay, University College.
W. E. Masfen, King's College.
B. Rice, St. Bartholomew's Hospital.
B. Shillitoe, University College.
N. H. Stevens, St. Bartholomew's Hospital.
E. H. W. Swete, Bristol, and King's Coll.
C. H. Tovey, Guy's Hospital.
J. W. Walker, St. Bartholomew's Hospital.
A. L. Williams, University College.

THE HONOUR OF KNIGHTHOOD CONFERRED ON DR. OUTRAM.

THE Queen has been pleased to direct letters patent to be passed under the Great Seal, granting the dignity of a Knight of the United Kingdom of Great Britain and Ireland unto Benjamin Fonseca Outram, of Hanover Square, in the county of Middlesex, Doctor of Medicine, Retired Inspector of Hospitals and Fleets, and Companion of the Most Honourable Order of the Bath.

THE CLASSICAL EXAMINATION AT THE ROYAL COLLEGE OF SURGEONS.

THE Court of Examiners of the Royal College of Surgeons have just announced that in future candidates for the Fellowship of the College may present themselves for the examinations in the Greek, Latin, and French languages, and in the elements of mathematics, at any period after having attained the age of eighteen years.

OBITUARY.

DR. TWITCHELL, U.S.

DIED, in Keene, N.H., on Sunday, May 26, Amos Twitchell, M.D., aged about 70

years, for more than forty years one of the most eminent physicians and surgeons in New England. A great number of physicians in the United States cherish a remembrance of him as their highly esteemed preceptor and friend. Dr. Twitchell graduated at Dartmouth College in 1802, studied medicine with the late Dr. Nathan Smith, at that time and for many subsequent years a professor in that college, and obtained a medical degree in 1805. He soon after settled in Keene, where he ever afterwards resided, and in a very short time rose to eminence in his profession. He has received repeated proposals to accept of a professorial chair, which he has always declined. He has often been elected president of the New Hampshire State Medical Society, and was an associate of the Philadelphia College of Physicians. He was a prominent member of the National Convention which adopted the constitution of the National Medical Association, took an active part in its first organization, and he evinced a lively interest in its objects and its success.

ROBERT CLARKE EDWARDS, ESQ., M.D.

On the 19th inst., at his residence, No. 90, Oxford Terrace, Hyde Park, Robert Clarke Edwards, Esq., M.D., aged 37.

DR. WILLIAM OGILVIE PORTER.

On Thursday, the 15th inst., in Portland Square, Bristol, Dr. William Ogilvie Porter, aged 76.

JAMES SNELL, ESQ.

On the 6th ult., at Kingstown, St. Vincent's, West Indies, James Snell, Esq., surgeon, in his 55th year. He was a man of great and varied talents, energy of character and purpose, and of great zeal and probity in the faithful discharge of duty, in whatsoever he undertook.

Selections from Journals.

ON THE EMPLOYMENT OF FORCED FLEXION FOR ARRESTING HÆMORRHAGE IN WOUNDS OF THE PALMAR ARCH. BY E. DURWELL, GUEBWILLER (HAUT RHIN).

M. DURWELL prefaces a case of wound of the palmar arch by some general observations on the modes of treatment which have been advised in this accident, and points out certain known objections to each. The following abstract presents the chief features of the case, and M. Durwell's subsequent observations:—

A woman fell from a ladder, having a bottle in her hand. The bottle was broken, and fragments pierced the palm. M. Dur-

well, on arriving at the poor woman's cottage, found the palmar arch wounded; and he had no means of securing the artery. While controlling the hæmorrhage by pressure on the brachial artery, the following sentence in M. Malgaigne's "Anatomie Chirurgicale" occurred to his mind:—"The only points at which obliteration of an artery can be obtained by position alone, without the aid of external compression, are at the bend of the arm and knee—a fact which is of great importance in reference to the arrest of hæmorrhage." Acting upon this statement, M. Durwell immediately bent the arm on the forearm at an acute angle; the hæmorrhage was instantly arrested. Advantage was taken of the circumstance to effect a definite cure. The arm was retained in its flexed position by bandages, so that the pulsation of the radial artery was completely intercepted. The wound of the hand was treated as an ordinary wound, and, for the sake of precaution, compresses were laid over the course of the arteries of the forearm. The cure progressed favourably. On the third day, as the patient complained of the restraint of the posture, the arm was slightly extended, and it was noticed that a small portion of florid thin blood oozed from the wound. The arm was restored to its flexed position, and in a short time the vessels and the external wound had perfectly healed.

In this manner a wound, usually regarded as of a very serious character, was safely and speedily cured by a proceeding as simple and unobjectionable as has ever been proposed. It must be attended with success when employed in analogous cases, and, from its simplicity, should be tried in the first instance in every case. M. Durwell observes justly, that it would be rash to make a more general application of a single fact, as there may doubtless occur complications, rendering its employment impossible or unadvisable; but, at the same time, he adds that this principle may be found, by further experiment, to be useful on other occasions, for the obliteration of the popliteal and humeral arteries. This principle he lays down in the proposition—"that in most arterial lesions of the forearm and leg, prolonged forcible flexion supersedes the ligature of the vessels."—*L'Union Médicale*. X

* * We find that the pulsation at the wrist is entirely stopped by firm flexure of the arm at the elbow-joint: hence hæmorrhage from the ulnar and radial arteries may be thus controlled.

PUERPERAL PERITONITIS.—DR. REESE'S NOTES OF HOSPITAL PRACTICE.

A SUDDEN irruption of this fever has appeared in the lying-in wards several diffi-

sent times, the epidemic character of which has been in every instance promptly arrested by abruptly changing the apartments to another floor of the house, having a different exposure to the external air, and in which a due ventilation could be secured. The wards in which the fever appeared were meanwhile thoroughly cleansed and purified by whitewashing, &c.; and not until thoroughly aired, and renewed by a change of furniture and bedding, have they been again occupied. Two or three weeks have been found a sufficient time to vacate the wards under such circumstances. The medical treatment has been various, and, in rather more than half of our cases, unsuccessful. The opium practice, when exclusively relied on, has uniformly failed. In no case has recovery taken place without bleeding; and yet, in several well-remembered examples, no advantage was derived from either venesection or leeching, although both had been liberally and judiciously employed. The same may be said of the mercurial treatment, with and without opium, and bloodletting, although all these remedies were used in every example of recovery; yet, in some instances, they all failed. The most numerous instances of success, however, were in those patients who, during the epidemic, were treated as follows:—Immediately after delivery, a combination of ten grains of calomel with ten of camphor was given. On the appearance of the characteristic rigor, when this occurred, or when without it other symptoms indicated an attack, the same dose was repeated, with two grains of opium. When pain and tenderness became distressing, and in the onset of the febrile excitement, a full bleeding at the arm was resorted to, the patient being placed in an erect position, and approaching deliquium being secured. If but little relief to the pain and tenderness followed this remedy, or these symptoms speedily returned, twenty or more leeches were applied to the abdomen; and, meanwhile, calomel and opium, in doses usually of from five to ten grains of the former, and one to two grains of the latter, were repeated every three or four hours, and in urgent cases more frequently. In these circumstances, a large blister plaster, covering the abdomen, and dressed with mercurial ointment on its removal, was found signally beneficial in several examples. The pytialism, when it occurred under this treatment, was but slight, and in no instance severe. The patients who recovered under this treatment used ice and iced water freely, all their drinks being cooled with ice. The extent to which bleeding was carried was various; two or three full venesections, besides leeching, having been sometimes called for during the first twenty-four

hours, and with uniform advantage, as the results proved. So also the calomel was varied in dose, and in the aggregate quantity, according to circumstances. Small, and even very minute doses, often repeated, were relied on in several cases, but every one of these proved fatal. So also scruple doses, with and without opium and venesection, were tried, but with less favourable results than the course above mentioned.—*Dr. Reese, in Amer. Journ. of Med. Sciences.*

BIRTHS & DEATHS IN THE METROPOLIS During the Week ending Saturday, Aug. 17.

BIRTHS.		DEATHS.	
Males....	699	Males....	441
Females..	674	Females..	433
1363		874	

CAUSES OF DEATH.

ALL CAUSES	894
SPECIFIED CAUSES	871
1. Zymotic (or Epidemic, Endemic, Contagious) Diseases....	256
<i>Sporadic Diseases, viz.—</i>	
1. Dropsy, Cancer, &c.	53
2. Brain, Spinal Marrow, Nerves, and Senses	106
4. Heart and Bloodvessels.....	23
5. Lungs and organs of Respiration	69
6. Stomach, Liver, &c.	51
7. Diseases of the Kidneys, &c.	12
8. Childbirth, Diseases of Uterus, &c.	8
9. Rheumatism, Diseases of Bones, Joints, &c.	7
10. Skin.....	1
11. Old Age.....	40
12. Sudden Deaths.....	0
13. Violence, Privation, Cold, &c.....	18

The following is a selection of the numbers of Deaths from the most important special causes:

Small-pox.....	10	Convulsions.....	42
Measles.....	11	Bronchitis.....	25
Scarlatina.....	24	Pneumonia.....	26
Whooping-cough.....	13	Phthisis.....	114
Diarrhoea.....	139	Lungs.....	5
Cholera.....	8	Teething.....	8
Typhus.....	27	Stomach.....	1
Dropsy.....	17	Liver.....	9
Hydrocephalus.....	26	Childbirth.....	6
Apoplexy.....	19	Uterus.....	1
Paralysis.....	18		

REMARKS.—The total number of deaths was 178 below the average mortality of the 33d week of ten previous years.

BOOKS & PERIODICALS RECEIVED.

(The List will be given in our next No.)

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer	29.76
Thermometer	62.6
Self-registering do.	Max. 90° Min. 46°
* From 13 observations daily. * Sun.	

RAIN, in inches, 0.86.—Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was 1° above the mean of the month.

NOTICES TO CORRESPONDENTS.

Communications have been received from Dr. Trayer and Dr. Tilt. These will have early insertion.

Mr. Newnam's request shall be attended to. The St. Bartholomew's Hospital Report next week.

RECEIVED.—The Glasgow Examiner.—The Glasgow Mail.—The Carlisle Journal.

Lectures.

LECTURES
ON THE
MEDICAL JURISPRUDENCE OF
INSANITY.

*Delivered in the Medical School of King's
College, Aberdeen.*

BY ROBERT JAMIESON, M.D.
Lecturer on Medical Jurisprudence in the
University.

LECTURE III.

General description of the insane state (continued). (a) The moral condition of lunatics (concluded).—Their selfishness, suspiciousness, timidity, and apathetic listlessness.—(b) The intellectual condition of lunatics—manifested in deranged voluntary power over the succession of thoughts, in delusion, imbecility, incoherence, and extravagant conduct—disturbed relation of will to thought. Delusions—their origin in violent mental impressions—their hold on the belief—their resistance to interfering doubts—their absorption of the attention—most distinctly defined in monomania—concealed delusion—delusions an insufficient test of unsoundness—divided into high and low. Subjects of exciting delusions—their insufficiency to produce happiness. Subjects of depressing delusions—Phobias—Panic—Hypochondriacal monomania—Nemomania, &c.—relative frequency of high and low delusions—modifications by sex, temperament, education, and bodily disease.

IN my last discourse I had commenced a general description of the insane state with the consideration of the moral condition of the insane. I described the peculiarities of that emotional derangement which is ordinarily a fore-warning of diseased intellect, and, in depicting the features of the fully developed disorder, alluded at some length to the morbid predominance of the selfish feeling, which, owing to the loss or depraved state of the sympathetic or benevolent affections, is so constantly observable in the insane. The consequences of this mental obliquity are for ever prominent in their language and behavior, and it leads besides to the unmasking of the evil passions of human nature, which, owing to the impaired voluntary control accompanying the disease, too frequently start up in ugly activity. In saying that the insane are

morally selfish, I use a term of very wide significance, which, as it were, epitomizes their moral condition, leaving very little to be added.

Suspicion is another feature which is pretty constant in the emotional aspect of the insane. Mistrust is said to be a natural characteristic of feeble minds, and to be the lot of those whose understandings have been but little cultivated; and it would also seem to be as natural a concomitant of mental disease. "The heart of the insane," says Esquirol, "cherishes no feeling but mistrust. This symptom increases with the progress of the disease, and augments in strength as the intellectual faculties are weakened. Yet notwithstanding their mistrust," continues he, "the insane have no foresight; they have no care nor inquietude for the future, but an extreme jealousy of every thing present."

This suspiciousness of mind, this constant imagining of evil, is exhibited in a multitude of delusions. Cabals and conspiracies are suspected to be formed against them by those whose good intentions there seems least occasion to doubt. Food is carefully inspected, lest it should be poisoned; to every unusual action in those around them a bad motive is attributed; and the over-heard common-places of ordinary conversation are twisted and tortured into an evil meaning towards their welfare. A female lunatic persisted in living solely on a vegetable diet during her illness, and no persuasion nor temptation could lead her to alter her resolution. After her recovery, I found that the reason of this conduct was, that she suspected the officials of the asylum in which she was placed to be in the habit of murdering the incurables, and victimizing the establishment with their bodies.

There are some cases in which the whole unsoundness seems to consist in unreasonable suspiciousness: it forms not an uncommon variety of monomania in advanced life. An old lady makes herself exceedingly miserable, by the baseless delusion that she is plundered and cheated by servants, neighbours, and indeed any one with whom she has had any dealings. Within doors, no number of locks and no ingenuity of fastenings are sufficient to give ease to her mind; every box and drawer is not only locked, but padlocked to boot; and when these are opened by herself, and every article is found in the exact relative position in which she had placed it, she has still the belief that her repositories have been entered, and her property changed for similar things of a less valuable description.

It is with the utmost difficulty that one

can satisfy a lunatic of the sincerity of words and intentions, and a request he looks upon as a snare. The superintendent of an hospital for the insane wished, for a special and very harmless purpose, to have the autographs of his patients. He found that he had undertaken a very difficult task in attempting to get them to comply with his proposal. He could persuade very few to subscribe their name without a great amount of trouble. Some signed hesitatingly; some repented when the deed was half done, or threw down the pen after they had taken it in hand; one signed his name upside down; another effaced his as soon as he had it completed; and many refused to have anything to do with such a dangerous proposal. The document was a very curious one in many respects, but not the least interesting feature in it was the evidence it contained of that suspiciousness of disposition on which we have been commenting.

So much suspicion is not compatible with a high degree of courage, and you will accordingly observe that an unnatural degree of *timidity* is one of the defects of the unsound mind. Patients in a state of maniacal fury may be found to encounter, blindly, any sort of danger, but all other kinds of insane persons are very easily alarmed. Those of them who are characterized as dangerous lunatics are more malignant than ferocious. The ferocity which, in times not long past, led to the imposition of chains and fetters on the insane, was the lie of ignorant, timid, and lazy keepers, or the result sometimes of their barbarity. You will find neither whips, chains, nor danger, within the walls of any of our British asylums.

An *apathetic listlessness* is another prevalent mood in the insane, though one of a negative kind, having its origin in a general anæsthetic condition of the senses, combined with an enfeebled memory and abstracted or decayed attention. Unless in maniacal cases, in which there is a morbid degree of activity, the desire of action is generally lessened, often entirely lost. The sound body cannot endure the tedium vite of idleness, neither can the healthy mind brook want of occupation. A man must have some occupation to withdraw him from himself, or, says Pascal, he is necessarily unhappy. Now the insane are almost constantly occupied with themselves, to the manifest increase of their misery. The desire of employment is very difficult to be excited in them; they will not exert themselves either to be useful or to be amused; and curiosity, one of the marks of an active and healthy mind,

is, as in some of the lower savage races, very much in abeyance.

One hears a good deal of balls and parties in lunatic asylums, and we have wondrous paragraphs in newspapers, mere puffs by vain superintendents or silly directors, describing how extra-sane and supremely happy all the proceedings were, or if any slip were, how pleasantly cured. Now having seen not a few of such things in various places, I can assure you that no ball in a lunatic asylum was ever worthy of the name; as how could it be, with the absence of such necessary elements as health, humour, wit, activity and contentment? In making this remark, I by no means intend to reflect in an unworthy spirit upon such attempts; they are most highly to be praised, if judiciously managed and not injudiciously blazoned. Such is the listlessness of the great body of lunatics, that to succeed to get them either to occupy or to amuse themselves is the hardest employment of those who have them in charge, and the one for which the highest commendation is deserved. Ennui is not a pain felt by an unsound mind. The monomaniac's delusion is his constant thought and his sole thinking; his feelings are of one complexion; he is insensible to the language which would arise from an unvaried feeling in a healthy mind. Ennui is a hopeful complaint in the insane, for they are in a position, says Esquirol, which is adapted to render it like an active sensation that will usefully react upon their thoughts and affections.

The intellectual condition of lunatics.—In the analysis of the human mind, two things are observed to constitute mental consciousness, viz.—thought and will. The first represents that unfeeling train of ideas which have their origin in an accidental series of external impressions on the senses, or which, if not ab externo, are connected together by certain laws of association. They consist of instinctive inclinations, emotional feelings, intellectual perceptions, and primitive rational intuitions. The second (will) represents the consciousness which we have of a certain amount of power in controlling or affecting the succession of thoughts. This voluntary faculty does not create ideas, it merely selects, detains, and operates upon, any particular idea of the many that crowd upon the mind. It is by this relation of our will to thought, that thought becomes thinking; desire, action; and abstract mind, personality. The agency of the will is apparent in such processes as attention, judgment, and recollection, which are, in truth, voluntary contemplation, alternate and retrospective; but, indeed, the volun-

very faculty is the essence of all intellectual operations whatever.

Now, in the various forms of intellectual unsoundness, both these principles seem in every case to be affected, though it would be more strictly correct to say, that it is the relation of the latter to the former that is disturbed. The suppression of ideas is more rapid than natural in maniacal excitement, more slow than natural in melancholia (monomania with depression), and uncontrolled by its natural laws in dementia. The voluntary power over strains of thought is defective in all cases. In mania it seems overpowered by the force and rapidity of the current; in melancholia, enfeebled by the one vast and gloomy image that has dammed up the whole channel of thought; and in dementia, permanently impaired.

Such a condition of the mind may occasion diseased manifestations of any or all of the intellectual processes, and it would not be difficult to select examples of disordered sensation, perception, attention, memory, conception, and so forth, occurring in lunatics: but the defect which is most characteristic of insanity is, as I have formerly stated, an inability to exercise the faculty of comparison, so as to evolve judgment upon one or more subjects, that incapacity being a direct consequence of impaired voluntary power over thought. In addition to such other evidence of disease as misinterpreted sensations and conceptions, rivetted or distracted attention, and wayward and irresponsible memory, there is this distinct impracticability of judgment before there can exist an insane delusion.

When the intellectual signs of madness become apparent, the premonitory stage is past; the disease is fully developed. The morbid intellect of insanity is displayed in delusion, inexplicable conduct, mental feebleness, and incoherence of language,—pathognomonic features which I shall now successively discuss.

Delusions are very generally but not universally to be observed in lunatics. They are, as you will recollect, illusions and hallucinations, governing the conduct, despite the will, despite the reason. They are idealities accepted as realities. They are errors of perception consequent upon the disordered sensations and disordered fancies, to which a diseased state of the psycho-physical relation renders the mind liable.

Violent mental impressions are the chief originators of delusion. Take the following example, which I borrow from the plentiful store of Dr. Conolly's experience. A clergyman opened a letter that was handed to him and swallowed the seal. A

friend standing beside him said, jestingly, that it might seal up his bowels. This made a startling impression on the clergyman's mind; the notion of his bowels being sealed up was a horrible one, and spite of himself, kept forcible possession of his thoughts, engrossing his attention so violently that his will became unable to dispel the conception. The attention could not be brought to bear upon any other subject, and if removed for a little, constantly returned to the disagreeable idea, until at length the conception acquired all the force of an actual sensation, and came to be believed in as a reality. The power of instituting an efficient comparison on this point was lost. No quantity of purgative medicine, operating however naturally, could prove to him that his intestinal canal was not actually closed up. There was no judgment in the matter, and he became a monomaniac with this particular hypochondriacal delusion. Here, then, you observe, first, a diseased intensity of conception; secondly, rivetted attention; thirdly, diminution of the voluntary power over the thoughts; and lastly, abeyance of judgment and insanity.

A morbid impression thus completely established—converted into a delusion—has a stronger hold of the belief than any impression that an actual sensation could convey. A lunatic believes in his delusion more firmly than ever he held by the truth of any real object of sense. The possibility of being deceived he cannot admit; he will sooner admit all other accredited realities to be deceptive. Those senses which act soundly enough have no power to correct the error. Sir Walter Scott relates the case of a poor lunatic in the Edinburgh Infirmary, who fancied that he was living in great state and splendour, in a mansion of his own, his only unhappiness being, that all the dainties with which his table was supplied had the taste of porridge. The palate acted an honest part, but its appeal to the judgment was quite ineffective. Any degree of doubt, which may arise in an insane person's mind regarding the reality of his fancies, is of a peculiar sort, and exerts no influence, or only one of the most transient and feeble kind. "I had a species of doubt," says a recovered maniac, describing what were his feelings, "I had a species of doubt; but no one who has not been deranged can understand how dreadfully true a lunatic's insane imaginations appear to him, how slight his sane doubts." A patient of Dr. Conolly's was the subject of delusions which he occasionally suspected to be delusive, and yet he could not deliver himself from them. He supposed that he had entered into pecuniary engagements with a friend,

which would occasion his total ruin. His words were, "I know I have not, I could make oath that I have not, and yet I cannot get rid of the impression that I have; and I am continually haunted with the dread of coming to the workhouse." This idea had led his thoughts towards suicide. He had tried in vain, and frequently, to get rid of the delusive impression by the exercise of reason, and felt conscious of the want of some help. "I think," he would add, "that I must get my friend to write me a letter, assuring me that I have not made this ruinous agreement with him; perhaps that may convince me." "But," says Dr. Conolly, "in these cases, until the morbid state is removed, conviction seems inadmissible by the mind: a man will send for his banker's-book, see that he has a large balance in his favour, and persist in saying that he is not worth a farthing."

The delusion of a lunatic is not an occasional but a constant error of mental action. "It is the dominant idea in his mind," says Dr. Pagan; "it is not a subject which occupies no more of his attention than its interest would seem to claim. He does not merely think about it erroneously, but it is *ever* present to his mind; talking, he speaks of it; silent, he thinks of it; sleeping, he dreams of it. No other subject has a proportional share of his attention; he has no sympathy for other men's interests."

Monomania is the form of insanity in which delusion is most distinctly defined. In the other varieties it is frequently difficult to point out any fixed governing idea. The maniacal are in a state of delirious excitement, in which everything wears a strange aspect, and the judgment cannot be brought to bear efficiently on any one idea. Their delusions are constantly varying with surrounding circumstances, and have no marked persistence: an insane notion arises, disappears, and is replaced by some other idea equally unsound, and possibly inconsistent with its immediate predecessor.

In monomania there is one particular, fixed, and dominant delusion, or there are a set of relative subjects on which the patient always manifests his derangement. If he think himself a king, for example, he may be comparatively rational on all matters unless those which connect themselves with the idea of his royal condition. But even in monomania, a delusion absolutely and persistently single is a rarity; for, by and by, the intellectual blemish is well seen to be surrounded by other marks of unsoundness, no longer latent. It is not, as ordinarily stated, that one delusion produces more; that one maggot generates another; the single intellectual error extending its

influence like a morbid virus. I have already told you that partial insanity as opposed to general is not a true phrase: it is only the exhibition of unsoundness that is limited. The single delusion is but the first manifestation of a mind as wholly unsound, though less intensely, as that of the bewildered maniac. However, be this as it may, true it is, that, when you have a single delusion, the chances are that you will have many more evidences of disease. There are few monomaniacs in whom the mind acts with vigour and soundness on subjects even widely removed from that of their delusion.

An insane person sometimes seeks to conceal the existence of delusion. I believe I may add that he never succeeds. There are such things as concealment of the nature of the delusion; but that some delusion exists in the mind is always in some measure expressed in habits and conduct. The attempt at concealment is to be witnessed only in cunning monomaniacs, who have become aware that they are deemed insane. Monomania is, however, the very form of insanity which gives rise to judicial difficulties, and in which it is of the greatest importance that the existence and character of the dominant delusion should be clearly displayed. I have more than once known such a lunatic to conceal the cloven-foot so successfully as to impose upon a most competent and persevering examiner, who was ignorant of the particular point on which the power of rational discussion was impossible. The insane often appreciate the motives which ought to lead them to hide their aberration, and are well aware of the subjects on which it is necessary to exercise concealment, in order to pass muster as sound in mind, but for all this they seldom or never succeed in concealing their delusion when they are directly questioned regarding it. The most that a lunatic desirous of passing as sane can in general effect is not of himself to introduce the subject on which his judgment is alienated; but when he is expressly catechised upon it he either refuses to answer any queries regarding it, which is to him a very great effort, or he certainly stumbles in his replies, and makes evident the unsoundness of his understanding. An insane lady had so far imposed upon a philanthropic visitor of the asylum in which she was confined, as to lead to a private and influential representation to the sheriff that she was unnecessarily and unjustly deprived of her freedom. The sheriff accordingly called unexpectedly at the hospital, and had a private audience in order to satisfy himself of her condition. She answered every question in an rational style, so to afford no apparent grounds on which her liberation should be refused. Upon consulting the case-book,

however, he found that she was said to entertain the belief that she was the Duchess of Wellington, or at times the Baroness Rothschild. Having obtained the key to her mystery he held a second conversation with her, when, in spite of her anxiety to impose upon one whom she knew to have the power of terminating her confinement, she exhibited such evident insanity, that he left her apartment holding up his hands in amazement. She would not resist a direct interrogatory upon her rank of Duchess.

Common as delusions are amongst the insane, they are not prominent, nor possibly even existent, in every case of the disorder, and therefore the absence of delusion is no test of soundness. Delusion is merely one indication that the disease has extended to the intellectual faculties, and is therefore no test of mere imbecility, of moral insanity, or of the first stage of many cases both of monomania and mania.

It is usual to divide the delusions of the insane into *high* and *low*, or *exalted* and *depressing*, according to the mode in which they affect the patient's self-esteem. What is commonly called a depressing delusion may be combined with both mental and corporeal excitement, and a high delusion may exist with much apathy of mind and debility of body.

Most of the high delusions of the insane consist of ideas of imaginary rank and wealth; of over-estimation of mental and bodily powers, and of imaginations of a less sublimary kind, leading the subjects of them to consider themselves as inspired by Heaven. However exalted the insane idea may be; and however much calculated to render the possessed of it independent of the influence of immediate and surrounding circumstances, it is very seldom a source of happiness and comfort. Such high notions seem always to cast a shadow on the mind; there is a hollowness in the happiness such as interferes with the exalted fancies of feverish dreaming, and a frequent recurrence of moods of depression, which are little consistent with the professed state of beatitude. As a class, lunatics are persons experiencing a great amount of misery: the feeling of pleasure is rare amongst them; laughter is almost unknown, at least as the natural language of joyous emotions; and it is only among some of the imbecile that unhappiness is either expressed or seen in the countenance or the conduct.

The low delusions are of more various kind, and require more special description. In this list, prospective feelings of dread, retrospective and present suspicions, hypochondriacal miseries, and terrors of the future state of existence, form the most remarkable groups. The dread of being killed by their enemies, or of suffering death

by the hands of justice for some form of fancied guilt, is a common species of delusion amongst the insane (*thanasophobia*), and one which it is well to be aware is perfectly compatible with the desire of killing themselves, and which is indeed very usually associated with the propensity to self-destruction. Many are overshadowed by some impending and gigantic evil, vague, but dimly real and irresistible, which they cannot describe. Every object wears a threatening aspect; they feel terrors unaccountable; their hearts are sunk within them; tears are scarcely ever from their eyes; and their whole occupation is to watch for the bursting of the dark cloud that is to destroy them for ever (*panophobia*). Some live in the constant dread of poison mixed with their food, administered instead of drugs, placed upon their clothes, hidden in their beds, and poured into their ears, or administered per anum during sleep. Others labour under the fear of future want, and, with no ground for anxiety, believe that they are on the high road to destitution and beggary. Their past circumstances seem like an experience of purple and fine linen when compared with their present condition; and the future they regard as a vision of sackcloth and starvation: such misfortune has been brought about by enemies who have plundered and cheated them, or has come upon them, by the vengeance of heaven, as a punishment for sins. Domestic jealousy is not an uncommon character of the insane delusion, particularly amongst males. Men are also more subject than females to another imagination which is a frequent accompaniment of lunacy, — the conviction that they are the victims of a widely-extended conspiracy. Every one with whom they come in contact is sure to become concerned in the great plot which is hatching against them. Macnaughten, the lunatic who assassinated Mr. Drummond, was a monomaniac of this class. For many years he had imagined himself an object of persecution, first to the Jesuits, then to the whole body of Roman Catholics; these were latterly joined by the Police, and finally, he believed, by the whole Conservative party. There is a numerous class of the insane in whom strange bodily sensations give rise to the notion that they are the subjects of strange pathological conditions, or of actual physical metamorphosis (*hypochondriacal monomania*). The sexual organs are very frequently the subject of hypochondriacal delusions among the insane. Obstructions of the intestinal canal, displacement of viscera, the sensation of living animals within the cavities, and various monstrosities of external appearance, are also common imaginations in this class. Depressing delusions connected with

religious subjects are very frequent, particularly amongst the female insane. Such persons are beyond the power of salvation, are under the curse of the Deity, have committed the unpardonable sin against the Holy Ghost, have directly bartered their soul to the evil one for some worldly pleasure or advantage, or are actually already in the state of eternal torment. Witchcraft, and various forms of unseen agency, are other subjects on which the insane often manifest their lack of judgment. They have been permanently injured by the incantations and evil prayers of others; their thoughts, words, and actions, are not the effects of their own individual will, but are the devices of external machinations, or are the doings of evil spirits around them, or within them. Now and then a patient will be found who has the painful fancy that he is actually possessed by the Devil, that his soul has been taken from him, and that Satan himself has taken possession of his frame (*demonomania*).

Delusions are liable to be modified by such circumstances as sex, age, temperament, education, and bodily disorder. Depressing fancies are much more common than those which are called exciting. In 287 instances of monomania, I found that the low delusions amounted to 168; the high delusions to 76; and those which were of an indifferent order to 43. High delusions were more frequent in the male than in the female sex. The only temperament in which they relatively predominated over those of a low type was the sanguine: in all the others, particularly in the lymphatic or deteriorated sanguine, depressing imaginations were the more numerous.* High delusions have a tendency to be transformed into those of a depressing character. Education modifies the nature, but more strikingly the expression, of insane convictions. An intelligent person, for instance, will not be found styling himself "the fourth person of the Trinity;" nor will an educated female write her name "Margaret Rex." The sensations which compel an ignorant lunatic to express his belief that he has been bewitched, would, in one above superstition, lead to the notion that he had been brought under the influence of some kind of magnetic or mesmeric agency. Bodily disorder may, there is some reason to think, produce a modifying effect. Diseases of the lungs, or of the encephalon, are

* In 244 patients in whom the modifying influence of temperament was observed, the following ratios existed:—

	High delusions per cent.	Low delusions per cent.
Aggregate cases	31	69
Nervous temperament ..	30	70
Bilious " ..	26	74
Sanguine " ..	60	40
Lymphatic " ..	8	92

more likely to be conjoined with delusions of a high cast, than those of the kidneys or heart are. Almost all cases of insanity which are threatening apoplexy or paralysis, are associated with high delusions. In those cases, also, which are complicated with the peculiar paralysis of the insane,—an insidious general palsy,—exciting fancies, are nearly universally observable. Such persons, while they are scarcely intelligible, from defective articulation, while they are passing their urine involuntarily, and staggering with extreme difficulty from one chair to another, still have a shattered physiognomy of happiness. They are in a paradise of excitement, leading armies over Alps, or dispensing the riches of India. When death is making daily approaches upon them, they are in a state of miraculous health; and when they are in his very clutches, they are in the arms of victory, or the glories of Solomon's temple.

CASE OF ANEURISM OPENING INTO THE TRACHEA. BY DR. MINOT.

THE patient was a female, thirty-six years of age, who had had dropsy for fifteen years, and diseased heart for the last four. Five weeks ago, there was noted a hoarse cough, dyspnoea, wheezing, and at nights orthopnoea but without expectoration or palpitation. These symptoms increasing, she was suddenly seized with extreme dyspnoea, the inspiration being easy, but the expiration laboured and rattling; pulse 160, and very feeble; whole chest resonant on percussion, but with sonorous and sibilant rales in every part. From this attack she revived, after a copious expectoration of clear, viscid fluid; but soon had a second. The respiration continued somewhat rattling, and always had a peculiar tubular sound, which could be heard at a distance from the bed; she complained, also, constantly of a sense of oppression in the trachea. Nine days before her death there came on pleuro-pneumonia; but this seemed to be subsiding, when, after a slight fit of coughing, a torrent of blood poured from the mouth and nose, and she died instantly.

The specimen being shown by Dr. M., there is seen to be some ill-defined dilatation of the arch of the aorta, with disease of the parietes, but nothing that can be called an aneurism; upon the inner surface of the artery at this part there is a deep ulcer, four or five lines in diameter, and this had burst into the trachea just above its bifurcation; a red, fleshy little mass projecting into this last at the seat of perforation. The bronchi were full of coagulated blood, and there were also found pneumonia, pulmonary emphysema, old pericardial adhesions, and scirrhus.—*American Journal of the Medical Sciences*, July 1850.

Original Communications.

ON SOME OF THE
MORE PRACTICAL POINTS
CONNECTED WITH THE
TREATMENT OF DEFORMITIES.BY EDWARD F. LONSDALE,
Assistant-Surgeon to the Royal Orthopaedic
Hospital.

(Continued from p. 191, vol. xlv.)

*Of the Treatment of Talipes Varus—
T. Valgus—T. Equinus—T. Calcaneus—
and T. Calcaneo Valgus.*

In my last paper I considered the treatment of talipes equinus and equino varus, in both of which it is necessary to divide the tendo-achillis; and in the latter sometimes the anterior and posterior tibial tendons, and, when very rigid and much shortened, the plantar fascia as well. I also described the mechanical means by which the foot was to be brought into its natural position, and of keeping it so afterwards.

There is a point I wish to refer to, before entering upon the treatment of talipes varus, which I omitted to mention when speaking of the division of tendons generally.—namely, a peculiarity that exists in the mode in which the tendon separates where it is found necessary to redivide it some time after the union of the first division. It will be found that it never separates with the decided “jerk” or “snap” that it does on the first division, nor is the same interspace left between the two ends. So little appearance is there of any division having taken place, that one unaware of the peculiarity I have mentioned would suppose such not to be the case, and would reintroduce the knife to make one or more attempts to ensure success: so constantly is this peculiarity met with, that it may be considered as a diagnostic mark of a previous operation having been performed.* A second operation may be required in cases where the patients neglect to attend to the after-treatment, or where the mechanical means are not properly and

efficiently applied to remove the deformity at a sufficiently early period to prevent the rigidity of the tendon returning before the deformity is removed.

The explanation of this peculiarity I have no doubt is, that after a tendon has been divided, not only do the ends themselves become joined together, but adhesions of the sheath to the adjacent portions, as well as to the surrounding cellular tissue, also take place, and which remain permanent after all thickening has subsided, and leave no appearance on dissection observable to the eye.

Of Talipes Varus.

Talipes varus is the true form of club-foot, of which all the other varieties may be said to be modifications.

Nature of the deformity.—Inversion of the foot, varying in degree from the slightest pointing of the toes inwards, to the extreme degree of inversion, where the anterior half of the foot brings the toes upwards, so as to be placed within a very short distance of the inner edge of the tibia. The os calcis is raised in an unnatural degree by the contraction of the gastrocnemius and soleus muscles, or by the original shortening of the muscle or tendon, or both, “in utero.” The other bones of the tarsus also lose their natural relative position to each other: thus the astragalus becomes displaced downwards and outwards in a position that the whole pressure of the weight of the body in walking falls principally upon this bone. This is made apparent in cases of long standing,—that is to say, where the patient has walked much, by the thick pad of fat and adipose tissue which nature forms as a soft cushion to protect the skin and parts beneath it from injurious pressure. The smaller tarsal bones, as well as the metatarsal bones, become also compressed, so as to diminish the transverse arch of the foot, the os cuboides and metatarsal bone of the little toe taking part with the astragalus in less severe cases in bearing the weight of the body. In very severe cases, as already stated, the foot is so much turned upwards and inwards, that the pressure falls only or principally upon the astragalus.

* I have found the above to be the invariable rule since I heard the observation made by my colleague, Mr. Tamplin. The tendon never separates at the first operation, no matter how many years after, the second may be performed.

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spasm of the one set of muscles over the other, or else to paralysis of the muscles on the anterior part of the leg, so leaving the foot to the action of those muscles of the posterior part of the leg, which, without being spasmodic, may, from want of any opposition, gradually draw the foot upwards and inwards.

The degree of severity existing in the congenital varus when the child is born is found to vary much: thus in some cases the foot may be said to be simply inverted, without any other alteration of its shape, the arch of the foot, either in its long or transverse diameter, not being shortened, and consequently unattended with any alteration in the relative position of the tarsal bones, the only alteration being that the astragalus is slightly drawn from its contact with the end of the tibia, and the os calcis being drawn slightly above its natural level: in these cases there is but little rigidity or contraction of the muscles, and the position of the foot admits of being easily redressed to a certain point by mechanical means only. In the severest forms of infantile varus, the foot becomes almost as much displaced as in those adult cases where the person has borne the weight of the body upon the foot for some years, causing the bones to be wedged together in every direction; the only difference being that in the infantile varus the transverse arch of the foot is not diminished, owing to the child not having yet walked. The tendons may be equally rigid in proportion to the child's age, and take an equally active part in keeping up the deformity. I stated in a former paper that in the non-congenital cases of varus, when dependent on paralysis, the feet never become so much deformed, owing to the condition of the limb never allowing the weight of the body to tell with a degree of pressure sufficient to compress the bones together, as occurs in the congenital adult cases where the limb is otherwise healthy. For the same reason you never meet with the thick pad or cushion on the inferior edge of the foot in the former, as in the latter cases.

Of the treatment of Talipes Varus.—

The first thing to be considered is the acting cause which immediately opposes the return of the foot to its natural position: and this will be found to exist in the permanent contraction (for such it may be said to have become) of those

muscles whose natural action is to turn the foot into the position in which it is found to be placed. The three principal tendons, then, that have to be divided, are the tendo-achillis, the posterior and anterior tibial tendons, and in some cases the tendon of the long extensor of the great toe, as also the long common flexor of the toes. The division of this latter tendon is rarely required, but may be in very severe cases, as well as in the paralytic cases of varus, where the toes become flexed in an extreme degree, owing to the paralysis of the extensors. The plantar fascia very often requires to be divided, owing to its shortening and extreme rigidity opposing any attempt to elongate the foot in its long diameter. Its division is often required in infantile as well as in more advanced cases of varus. In the former cases, however, the shortening of the fascia does not become apparent till the treatment has sufficiently advanced to enable the foot to be flexed in its natural position; it will then be found that the toes cannot be brought sufficiently upwards, at the same time that the arch of the foot is more prominent than natural, a condition that can be easily ascertained to depend upon the plantar fascia, by pressing the finger forcibly into the sole of the foot at the same time that the fascia is put upon the stretch, when a firm hard cord will be felt. I should recommend the division of the plantar fascia in all cases, whether infantile or adult, where it appears to offer the slightest resistance to the elongation of the foot, it being, I believe, one of the most unyielding parts of the body, and much more so than the proper ligamentous structure.

The alteration in the shape of the bones is little or none, which is a matter of surprise, when it is considered how much the relative position of the individual bones to one another is changed, as well as their points of contact bringing them under the pressure of the weight of the body in a direction quite different to that they would have to bear in the natural condition of the foot.*

* There is a very interesting specimen in the museum of St. Bartholomew's Hospital of a skeleton with double club-feet, showing very well the altered position of the bones. The os naviculare is pushed completely from its articular junction with the astragalus, and rests on its inner edge. The os cuneoides is also displaced from its connection with os calcis, resting also more on its inner than anterior edge. (Abstract)

The only bone, I believe, which is liable to become affected is the astragalus;—absorption may take place, causing a diminution in its size, more particularly of its anterior portion; produced no doubt by the constant pressure upon it in adult cases. The inner malleolus of the tibia will also be found in some cases diminished in size, caused no doubt by the constant pressure against it, either originally preventing its growth to its natural size, or causing its diminution afterwards.* This will of course weaken the joint, by depriving it of the support it naturally possesses in its perfect state, and be a cause of the return of the deformity, if the ankle-joint be not properly supported by mechanical means, when the patient begins to bear his weight upon the limb after the foot has been brought into position.

The ligaments take an active part in keeping the bones in the wedged position in which they are found in adult cases of varus, rendering it one of the most difficult parts of the treatment to get the foot flat, by the opposition they make to the unfolding of the arch: and until this be done the cure cannot be considered satisfactory; for when the patient begins to walk, he will be seen to tread more upon the outer than inner edge of the foot, and will gradually turn the foot inwards, and after a while cause a return of the deformity. The most difficult bones to redress are the os cuboides and the metatarsal bone of the little toe. It must be the ligaments that oppose the replacement of the bones, for only one other cause could,—viz., anchylosis, and this I have never seen yet, nor do I think it likely to occur.

In all cases of varus, whether slight or severe, *the foot must be brought completely out of its state of inversion, so that the great toe may be in a straight line with the tibia, and the sole of the foot be rendered perfectly flat*, before any attempt at flexion of the ankle-joint be made. If this be not attended to, the deformity will never be removed, for two reasons—in the first place, the joint cannot be flexed to an extent sufficient

to allow of the heel being brought completely to the ground; and in the second, no after attempts to flatten the arch of the foot will be successful.

I shall now describe more in detail the various steps in the treatment of varus, and I shall take a severe form in the adult, in illustration.

It is impossible to give a *decided opinion* as to the length of time it may take to treat the severe form of congenital varus that is met with in the adult; for, as I have already stated, the great difficulty is, to unfold the arch of the foot, to bring the tarsal and metatarsal bones out of their wedged into their natural position, and the amount of resistance that may be offered cannot be ascertained till the treatment has made a certain advance: as a general rule, the older the patient the longer the case will take to treat: there may be a difference, however, in cases of the same age, which may be referable to the severity of the form of varus. There is also a difference in infants, some cases being much more amenable to treatment than others. As far as my own experience has gone, I should say the average limits of time it takes to treat cases of confirmed congenital varus in adults, and much after the age of puberty (and by *treatment*, I mean not the *partial* removal of the deformity by which the patient is enabled to put the sole of the foot to the ground, the foot itself still being twisted, but the *complete* restoration of the shape of the foot, in which all trace of deformity is removed), is from eight to twelve months, and in some cases more. In infants from two to four months old, the time required will be about the same number of months, either two and four, according to the degree of severity of the deformity; in some even less, when the varus is but slight, and in others a longer time, when very rigid. As a general rule, an opinion may be pronounced as to the case being one of an obstinate nature by the appearance the foot presents, as well as by the degree of rigidity it offers to the hand when attempts are made to move it towards its natural position: some will be found to be much more yielding than others, and will therefore offer less resistance. The appearance of the dorsum of the foot will indicate the extent to which the bones are wedged inwards towards the sole, by being more prominent, and

tion has also taken place of the anterior and outer parts of the astragalus, and os calcis. The other bones are of their natural size and shape.

† I have a case under my care now, of congenital varus in an adult, where there is a complete absence of the inner malleolus, there being no trace of any projection now that the foot is brought into its natural position.

presenting an irregular and knotty surface, instead of the smooth one the dorsum of the foot naturally possesses. This difference is observable in infants also; in some cases the tarsal bones project from the dorsum of the foot much more than in others, and are always a longer time under treatment.

Of the division of the tendons in varus.

—The first stage in the treatment of talipes varus is that occupied by bringing the foot outwards and downwards from its inverted position till it is in the state of extreme extension, the great toe being in a line with the inner margin of the tibia. To admit of this being done, the tendon of the posterior and anterior tibial muscles must be divided, and in extreme cases, the flexor communis digitorum, and the extensor proprius pollicis. The mode of dividing these tendons I have already described in a previous paper. When the plantar fascia takes part in producing the deformity, by shortening the foot, it must be divided also. I believe that in very severe and rigid cases advantage will be gained by trying to bring the foot outwards by mechanical means as much as possible before the tendons are divided, in order to advance the foot a stage, the object of which is, that the most obstinate rigidity offered by the bones and ligaments will be reached at an earlier period after the tendons are divided, and before union of their divided ends can have become sufficiently firm to act a second time as an opposing cause, which I am inclined to think may be sometimes the case, with these tendons, as well as with the tendo-achillis (which latter tendon is not divided in the first stage of the treatment to this account)—viz., that by the time the foot were in a position to be flexed it would have become so firmly united as to require redivision. In infants, in most cases, the tendo-achillis may be divided at the same time with the other tendons, for the foot may be everted before the union has become sufficiently strong to oppose the flexion of the foot upwards; but here, also, should the form of varus be more than ordinarily rigid, I should recommend its division at a second operation, as in the adult, as it will save after trouble, as well as the necessity of a redivision of the tendon.

The mechanical treatment required in

the first stage of varus.—After waiting four or five days in infants, and a week in adult cases, to allow the wounds to heal, and the union of the tendons to have commenced, abduction of the foot may be commenced in the following manner:—The leg must be evenly rolled from the toes upwards, to the knee. A long straight wooden splint, extending half way up the thigh, and for about eight or nine inches below the ankle, and of a width corresponding to the width of thigh and leg (that is to say, the upper part is to be as wide as the thigh, and gradually diminishing to the thickness of the ankle below), is to be placed outside the limb. The object in shaping the splint in this manner is, that it is less liable to slip from its position than if it were made of one uniform width. A firm pad is placed beneath the splint, being made of much greater thickness opposite the outer ankle, in order to throw the splint well off from the foot, to enable the straps that pass round the toes and instep to act with greater advantage. This pad should not extend below the outer ankle to avoid pressure against the edge of the foot itself. The splint is fixed to the thigh and leg by five or six broad webbing straps placed at intervals, the lowest one being low down, close to the instep: an inside splint is sometimes of use also, attached by back straps to the outer one, when there is a difficulty in preventing the outer splint slipping round the front of the limb. The lower end of the outer splint is notched in three or four places, or may have slits made in it to pass two or three straps round and through. These straps are made to act upon the foot in the following manner: the lowest one passes round the ball of the great toe; the one immediately above it round the instep; the third round the bend of the ankle and over the heel behind. Before these straps are tightened the foot is to be everted as much as the rigidity of the parts will admit of, when the straps are to be fastened by means of the buckle to retain the foot in this position. The straps are to be tightened by degrees so as to bring the foot gradually more and more outwards every second or third day, accordingly as the patient may be able to bear the pressure, care being taken that irritation of the skin be not produced either on the outer ankle or over the ball of the great toe, and if

such be the case, all the straps must be loosened till the skin recovers itself.

As soon as the foot has become everted from its original deformed position, sufficiently to be in a straight line with the leg, examination must be made to ascertain the condition of the tarsal and metatarsal bones; and if the cuboid and metatarsal bones of the little toe be found projecting towards the sole of the foot, so contracting the transverse arch, means must be taken to press them upwards towards their natural position. These means are by placing pads beneath the sole and on the dorsum of the foot in such a position that they may press the outer edge of the foot upwards and the inner edge downwards. This is best done by first placing the foot in the Scarpa's shoe. This, as before stated, is often one of the most tedious parts of the treatment, for it must be persevered in till the sole of the foot be rendered quite flat, requiring frequent alteration in the position of the pads, and will often tax the patience and ingenuity of the surgeon.*

In infantile, or cases more advanced, but still under the age of puberty, these screw pads are not so frequently required; for there has not been that degree of pressure made upon the bones of the feet by the superincumbent weight of the body to fix the bones in the wedged position which I have just been describing.

Of the second stage of the treatment of varus.—Presuming the foot to have been brought into a straight line with the leg, and the wedged condition of the arch of the foot having been sufficiently unfolded, so that the sole of the foot is rendered flat, the foot is now in a condition to allow of the second stage of the treatment to be proceeded with, which is that of flexing the ankle-joint and bringing the heel down. In order to do this, the tendo-achillis has to be divided, which operation I have already described in a previous paper. At the end of five, six, or seven days, according to the age of the patient, the limb may be placed in the Scarpa's shoe, taking

care that the straight line of the foot and leg be still preserved; the ankle-joint is now to be flexed every second or third day, by turning the screw at the ankle, each time very gradually. As a general rule in adult cases, this flexion of the ankle will occupy about six weeks or two months, and becomes in most cases the shortest stage of the treatment, though unforeseen circumstances may occur to interfere with this part of the treatment, so as to allow the union of the tendo-achillis to have become so rigid as to prevent the heel being brought down, and to require a second division of the tendon before the cure is completed.

Of the third stage of the treatment of varus.—The foot having been now brought into position by means of the Scarpa's shoe or other mechanical means the surgeon may please to employ, the next thing to be done is to fix it in such a manner that the patient may begin to make attempts to bear his weight upon it in walking, and this is to be effected by means of a boot and iron so made, that the foot is prevented being turned inwards and the heel being drawn upwards. The principal points to attend to, and which I have already described in a previous paper, are, to see that the boot is large enough and wide enough to prevent the toes being cramped. The part of the boot opposite to the bend of the ankle should fit sufficiently tightly to keep the heel well down. Further support is given to the ankle by means of a double iron, one on each side of the leg, extending up to the knee, with joints opposite the ankle made to lock at their posterior part, and so prevent the heel from being drawn upwards. A broad ankle strap is also required, passing round the outer ankle and inner iron. This boot must be worn in bed as well as in the day-time, the first month or six weeks, to guard against any recontraction of the tendo-achillis. Another important point to attend to, is to work the ankle-joint frequently in a state of flexion; either by the patient himself placing the foot flat upon the ground and bending the knee forwards, or by a second person taking hold of his foot and forcibly flexing it upwards. This practice should be followed daily to overcome the rigidity which naturally exists, as well as to overcome any tendency there may be for recontraction of the tendons.

* There have been two or three cases lately in the Orthopedic Hospital, under the care of my colleague, Mr. Tappin, in which the difficulty of unfolding the arch has been immense, and which have required great pains and trouble to bring the feet into the flat condition, causing the cases to occupy a length of time only known to those who have great experience in the treatment of these cases.

In infantile varus, the same powerful means are not required to bring the foot into position, though the same rules hold good as in adult cases—viz., that the foot should not be flexed till it be brought in a straight line with the leg. A small Scarpa's shoe may be employed, or other mechanical contrivance which gains the desired end. A boot with a very light thin iron may be required in some infantile cases as well as in adult, where much rigidity remains or tendency to recontraction of the tendo-achillis. Weakness of the knee-joint often exists in these cases of infantile varus, which is evidenced by the leg rolling inwards, causing the foot to turn in the same direction, and giving the appearance of the foot still being deformed. This is to be obviated by carrying the iron above the knee, and attaching it to the thigh by means of a broad leather strap, and if the inversion be extreme the iron should be carried still higher up to the hip and attached to the pelvis. It is to be worn till the child gains sufficient power over the muscles to prevent the knee rotating inwards.

Of Talipes Valgus.

Nature of the deformity.—Eversion of the foot, with flattening of the arch, causing the foot to be much wider than natural, owing to the tarsal and metatarsal bones being spread out to the utmost in severe cases, instead of being wedged together as in the cases of varus just described. The inner edge of the foot becomes rounded, and projecting inwards; the outer edge appears much widened, and almost inclined to turn upwards; the os calcis appears longer than natural, owing to the flattening of the foot bringing the tibia and fibula more forwards and inwards. This condition of the foot is produced entirely by the want of power in the ligaments to keep the bones in their natural position. The opposite set of muscles is now in action to that in the talipes varus—viz. those muscles that turn the foot outwards and upwards,—these are the peronei and long extensors of the toes. Their tension is more marked than the tibialis posticus and anticus, owing to their position admitting of more displacement of the tendons, and therefore making them more prominent, and more easily felt. The tendo-achillis is not concerned in the simple valgus, although the natural facility of bending the ankle-

joint does not perfectly exist; this is owing, however, to the altered position of the tibia, by the astragalus being pushed so much forwards and outwards. Talipes valgus may be congenital: it is, however, more frequently noncongenital, and produced most commonly by carrying heavy weights, and being constantly on the feet.

Of the Treatment of Talipes Valgus.

If there be the least rigidity about the joint, combined with marked resistance of the tendons, mechanical treatment only will not succeed in removing the deformity. In slight cases, however, where these are not tense, and the deformity of comparatively short standing, there will be no occasion to divide the tendons: the following treatment will then be found sufficient to effect the cure. If the flattening of the foot be only slight, a piece of cork placed on the sole, in the inside of the foot, shaped in a manner to press principally beneath the astragalus and cuneiform bones, the object being to support the arch of the foot at the part where it is most inclined to sink. The cork should not be made of the full thickness at first, otherwise it will make painful pressure; it should be thickened by degrees, until the arch of the foot is pressed well upwards. If the patient be a young person, the cork must be worn till he have done growing, and care should be taken never to bear the weight of the body on the foot without it; for when the construction of the arch of the foot is considered, it will easily be conceived how little pressure will soon displace the bones again, and stretch the ligaments.

In more confirmed cases, but still not sufficiently rigid to require the division of the tendons, more powerful pressure must be made by means of a firm pad made to press against the inside and under part of the arch. To do this, the patient must be confined to the bed or sofa.

Of the division of the tendons for the treatment of talipes valgus.—The tendons requiring to be divided in talipes valgus are, both the long and short peronei, and the extensor communis digitorum with the peroneus brevis. The two former tendons always require division; the latter, only in rigid cases. I am not sure, however, that the majority would not be always benefited by the

common extensor being divided: it frees the ankle joint, and allows of the anterior half of the foot being brought down, while the arch of it is pressed upwards: it *loosens* the bones, if I may use the word. The difficulty experienced in dividing the two peronei is, that the more anterior of the two—viz. the peroneus longus—most frequently rides over the margin of the fibula, and is found placed upon the bone, instead of behind it. In adults the tendon can be easily felt; but in infants, where the tendons are very small, and the child fat, it is by no means easy to do so. Where the tendon is found to be riding on the bone, as just described, it is to be divided by passing the sharp-pointed knife rather obliquely, and in a position more anterior, in fact over the bone instead of behind it: so much of the tendon as lies upon the bone may be divided, when the point of the knife is to be dipped a little deeper to include the other tendon, if it be not raised to a level sufficient to be included in the division of the first one. In infants or young children the same precaution is to be taken, only the operation becomes more difficult from the greater difficulty of defining the margin of the tendon. In adults the division of the tendon is evidenced by an audible snap, and a marked interspace remaining. In infants the yielding of the foot with a slight jerk is felt more by the assistant than by the operator, and no perceptible interspace between the ends of the tendons remains afterwards.

The extensor communis is easily divided, by passing the knife beneath the tendons at the bend of the ankle-joint, just before they split to diverge towards the toes; one stroke of the knife then generally divides them all at once. Were the knife passed lower down on the instep, it would have to be pushed a long way beneath the skin to include all the tendons. Compresses and a few turns of bandage to confine them are placed over the punctures, and a splint, padded, is applied in front and to the inside of the ankle-joint for a week, to allow the wound to heal.

Of the Mechanical Treatment of Talipes Equinus.

A thick oval pad must be placed along the inner and under part of the sole of the foot in a position to press the astragalus and navicular bones

upwards. While this is being done, the toes and outer edge of the foot must be brought downwards and inwards; in fact, in a direction as if an attempt were being made to produce *varus*. This object is gained by having the pad attached to a strong spring which extends to and beyond the great toe, and is curved from it; the other end of the spring is fixed to a splint of the width and length of the legs, along the inner edge of which it is placed, and extends as low as the ankle. This splint is well padded, and firmly fixed to the leg by means of bandages and straps. The foot is then to be fixed to the spring by pressing the latter towards the foot, and the pad, of course, with it against the arch; and then passing two or three straps round the toes and free end of the spring by means of small buttons on which the strap hooks. These straps are to be gradually tightened till the arch is returned to its proper shape.

The arch of the foot being redressed, it has to be supported, as I have already described, by means of the raised cork sole within the boot. If the ankle-joint be weak, or disposed to fall inwards, the boot must have an iron attached to it, extending from the sole up to the knee, being placed on the outer side, and standing some way off from the ankle, round which a broad strap passes, which supports the joint, and brings it outwards towards the iron.

Of the Treatment of Talipes Equinus Valgus.

Nature of the deformity—Eversion of the foot, flattening of the arch, and elevation of the heel or locking at right angles. It is more frequently non-congenital, and very often paralytic, the muscles on the anterior part of the leg having lost all power of flexing the ankle-joint. The extensors of the knee are also frequently paralysed at the same time, causing the patient to throw the leg out, having no control over it.

In the paralytic cases the only tendon requiring to be divided is the tendo-achillis, and the points to attend are to take great care that the uniting medium be not too quickly stretched, otherwise the tendon will be weakened to an extent that its power over the heel will be lost, and the os calcis will drop, converting the case into one of *talipes calcaneus*, and so leaving the patient worse off than before. A fortnight should be

allowed to elapse in these paralytic cases before extension be made upon the divided tendons, and then it must be done very gradually, indeed.

When the heel has been brought down, and the arch redressed, the same plan must be adopted with regard to the boot as just described: the joint, however, had better be supported by means of a double iron with a stop-joint, one on each side, and a very broad ankle-strap passed round the inside of the ankle, and round the outer iron. If the knee be paralysed or weak, the iron should pass above the knee, and have a support behind the joint as well. In young children it is a very good plan to have the whole limb fitted into a leather case, closely surrounding the thigh and upper half of the leg, fastened by straps and buckles. The iron in the boot is also attached to it, and fixes the ankle joint in the same way as before.

In the non-paralytic cases there is, of course, no occasion for the iron to extend above the knee, and there must be a strong lock at the ankle-joint of the iron to prevent the heel being brought beyond the right angle.

Of the Treatment of Talipes Calcaneus.

Nature of the deformity.—It may be congenital or non-congenital; if the former, I believe it depends upon position "in utero," although the muscles in the anterior part of the joint may appear to be more rigidly contracted than natural. I believe they have only become so by having accommodated themselves during their growth to the position of the foot. There is no paralysis of the muscles connected with the tendo-achillis in these congenital cases.

In the non-congenital cases there is always paralysis, causing the heel to drop; and in extreme cases, so much so, that the os calcis is almost brought into a straight line downwards with the bones of the leg. The plantar fascia in most of these paralytic cases is contracted to an extreme degree, causing the arch of the foot to be apparently increased by the hollow that exists beneath it. It will be found, however, to be principally caused by the dropping of the os calcis, and its approximation to the toes; for the plantar fascia having lost all resistance to keep it on the stretch, by the loss of power in the tendo-achillis to pull it backwards, remains in its original shortened condi-

tion if the attack have commenced when the child was young, and the bones small, and does not grow with the rest of the foot.

In congenital cases of *talipes calcaneus*, if very rigid or unyielding, the extensor communis digitorum must be divided by passing the point of a small knife beneath it, just at the bend of the ankle, and extension be made after four or five days by placing a small padded tin splint on the anterior part of the leg and instep, and so gradually bringing the foot down to its natural position.

The non-congenital cases of *talipes calcaneus* constitute one of the most unsatisfactory class of deformities the surgeon has to treat; there is, perhaps, only one more so—namely, the *calcaneo valgus*; for the principal cause of the deformity being the loss of power from complete paralysis of the large and powerful muscles of the calf of the leg, which generally become so much atrophied as to preclude all chance of recovery, leaves the foot in a much more helpless condition than in the other kinds of deformities I have hitherto described. I believe all that can be done is, to endeavour to get the heel into better position, in those cases where it points vertically downwards, by dividing the plantar fascia freely, and then applying pressure on the instep in such a manner that the os calcis may be pushed upwards and backwards. I should recommend the division of the deep muscles, if necessary, at their point of attachment to the os calcis; for they may, in cases of long standing, take part also in opposing the replacement of the os calcis: partial improvement may generally be gained, but not complete.

As soon as the os calcis has been brought into position as much as circumstances will admit of, means have to be taken to fix the foot in such a manner that it may be kept flat upon the ground when the patient walks; and this can only be done by means of a double iron attached to the boot, with a "lock" at the ankle joint placed on the anterior part of the hinge, so as to prevent the heel from falling downwards; a broad ankle strap should also pass round the inner ankle and outer iron, as there is always a tendency to valgus in these cases, if it be not decided. The boot should also be made of good length, and fit the instep well, so as to press the heel firmly downwards and backwards.

by these means the patient may be made to walk better, and with comparative comfort.

Of the Treatment of Talipes Calcaneovalgus.

Nature of the deformity.—Dropping of the heel, and eversion of the foot, the most unsatisfactory of all deformities of the feet to treat. In most of the cases there may be said to be paralysis of all the muscles of the lower extremity below the hip, accompanied with extreme atrophy of the whole limb. All that can be done (and which is rarely required) in the way of operation is the division of the plantar fascia, to enable the heel to be brought more into the horizontal position.

The mechanical treatment required for these cases is to confine the whole limb, either by means of an iron extending the whole length, from the foot up to the hip, with proper straps and knee caps, or else place the whole limb in a case of thick leather, with irons attached to the boot. The iron must be double below the knee in both cases, and have a broad ankle strap passed round the outer iron to keep the ankle joint from falling inwards. In these cases the flexors and extensors of the hip are seldom, if ever, paralysed, so that the patient is enabled to bear his weight upon the limb when the knee and ankle-joint are artificially fixed, and walks with the aid of the hip-joint only.

In my next paper I shall consider some of the more practical points connected with contractions of the larger joints.

[To be continued.]

EXPENSES OF CHOLERA IN IRELAND IN 1849-50.

THE entire number of persons attacked with cholera in Ireland was 55,141. Of these no fewer than 8309, or more than one-seventh, were Dublin patients. There were in the

North Union	3787
South Union	4522
Total in Dublin	8309

The expenditure created by this visitation was altogether £41,102, of which the Dublin portion was only £2561, or about a twelfth.

PULMONARY DISEASES IN TROPICAL CLIMATES.

AND THEIR TREATMENT.

BY R. H. A. HUNTER, M.D., Surgeon.

Pulmonary diseases.—The next subject we shall glance at will be the class of pulmonary diseases; not because of their superior importance; but because they have been little noticed in works on tropical diseases, though certainly they are neither deficient in variety nor interest. Consumption, as we have elsewhere shown (*Transactions of the Bombay Medical and Physical Society*, No. 4), is a disease of moist exposed localities, without reference either to latitude, temperature, or elevation; and so occurs equally in tropical as in temperate climates. Thus it is found at its minimum in the dry sheltered climates of the East Indies, whilst in the West India Islands it attains nearly to its maximum. From inattention to this, very grave mistakes have been committed. Persons have frequently been sent to the West Indies on account of consumption, though this is about as reasonable (according to the above views at least) as to send those with chronic rheumatism or pleurisy to the frozen regions. It is sometimes objected, it is true, that Madeira is a good climate for the consumptive, but Madeira is not within the range of the trade winds, neither is it in "the variables," as they are called, but between these in the mildest region of the world. Madeira, too, is at no great distance (to the eastward) from the dry burning deserts of Africa, whilst from the west it is completely sheltered by the mountains; only the lower south-eastern face of the island being inhabited, by invalids at least; whereas the West India islands are far in the strength of "the trades," after these have swept nearly across the whole western ocean. So true it is, with respect to this disease at least,—*"Nam fere ventus ubique, a Mediterraneis regionibus veniens salubris—a mari, gravis est."* In the West India islands the ratio of mortality from consumption is certainly not less than one per cent, whereas in central India it is scarcely two-tenths. Neither can it be

always prudent to send the consumptive even to the southern shores of England, except the summer be decidedly dry; or perhaps, after all, some of the low inland districts might be a better locality, as Clifton or Bath. Indeed, it is to the sheltered localities of these last, rather than to the waters, we would ascribe their salubrity.

But the best place for the consumptive we know, and now within the reach of the wealthy at least, in thirty-five days, is Poona, under the Presidency of Bombay, as it also possesses the advantage of being as healthy as England in most other respects. Madras, too, is a good climate for the consumptive, and Bangalore (another excellent climate); but Bombay, Cannanore, and the Malabar coast generally, had better be avoided, at all events in the rainy season. In Bombay, indeed, one way and another, we met with more of consumption than in all the rest of India put together twice over.* It is there particularly frequent, too, among the "half castes," and those of European extraction generally, who have been brought up in India, insomuch that on our losing one of this class in the female (regimental) hospital, the late Archdeacon Jefferies mentioned he had never known one of the girls brought up in the Bycullah Orphan School reach the age of thirty, and chiefly from this cause. The regiments or detachments from the interior, also, soon lose their consumptive cases in Bombay, whether arriving as invalids or in the ordinary way of relief, and the same at Cannanore, and particularly during the moist stormy winds of the south-west monsoon.

If consumption be confirmed, however, any change only adds to the discomfort, and may hasten the event: nor would it be advisable to go to India otherwise than *viâ* Egypt, which is itself, I should say, a good climate for the consumptive about Cairo, not Alexandria, though, it is true, we have the authority of Celsus, that the latter is preferable to Italy—"ideoque aptissime Alexandriam ex Italia itur"—(I have myself only been once through this way). Many have the disease first set a-going during the voyage by the way of the Cape of Good Hope,† and it

is equally pernicious from the opposite direction. On one occasion, with invalids, we lost all our consumptive cases (three) between Bombay and the Cape, though none of them were supposed, previously to embarkation, to have been in a hopeless state. In fact, they were all embarked under other diseases.

Bronchitis.—This is another by no means infrequent disease in tropical climates, and, though seldom fatal, has in the higher localities in the East Indies a great tendency to run into a chronic or asthmatic form. From Poona particularly, to the east of the Great Western Ghats, we had to invalid several on this account, but in the low countries or coast have never known it have this termination. On the contrary, those from the interior have been immediately relieved, and many of them rendered fit again for service, in the low countries at least. Bronchitis also occasionally complicates the fevers in India, imparting to them a typhoid character, and adding greatly to the danger. Also in the West Indies; but here bronchitis, instead of assuming the asthmatic form, tends rather to consumption, becoming localized in one or other of the subclavian regions. It is to this may be attributed the mistake Colonel Tulloch has fallen into with respect to the frequency and mortality from pneumonia in the West Indies: this we sufficiently proved by a careful perusal of the records on the spot.

Hæmoptysis.—In the East Indies this occurs far more frequently than can be accounted for on the presumption of some latent tuberculous disease. Besides, it is more generally found to proceed from one or other of the lower lobes; but so seldom do such cases even eventually prove fatal in India, that it is difficult to say, perhaps, what their true nature is. We except, of course, those connected with phthisis, aneurism, or with disease of the heart. One we remember, however, in the lower left lobe, which, after two or three attacks, proved fatal, with all the symptoms of phthisis. In this there was a large regular cavity resembling the empty cyst of an abscess, without any other marked pulmonary disease. And another ultimately died away from head-quarters, who had been frequently under treatment for some years previously. Per-

* See a few cases, &c., "Bombay Transactions (Medical and Physiological)," No. 1.

† There are two rainy seasons, or zones, passed through this way.

haps there are two varieties, the one nearly allied to the first stage of pneumonia, which is immediately relieved by the hæmorrhage, the other to pulmonary apoplexy. In three cases of sudden death we found the pleura burst up, and the left cavity of the chest filled with blood. One of these, however, was originally a case of acute bronchitis, which as a pneumonia had been treated on the "heroic principle." Getting up in the night, he fell dead very unexpectedly, when on examination it was found the blood had escaped from a superficial gangrenous spot, at the bottom of the sulcus, between the two lobes. The gangrene was inferred chiefly from the colour of the membrane, but it was very limited, and in depth could not have exceeded a quarter of an inch. The two pleural surfaces between the lobes were partially adhering by very recent lymph, and the lower lobe was engorged with frothy serum. In the West Indies, also, hæmoptysis is frequent and copious: so copious, indeed, that in two cases we met with of phthisis it more resembled a pulmonary apoplexy. One of these, which had been in hospital for some months previously, without any alarming symptoms, proved fatal fifteen days after. The right lung was engorged with blood, as well as thickly studded throughout with large white tubercles, only a few of which had become softened. The other was a similar case, but he left with his regiment (19th Foot) in the hope that a sea voyage might prove beneficial.

There was a curious case we might notice here, which occurred at Poona in 1835, though not immediately connected with the above. It was that of a fistulous scrofulous abscess in the left axilla. The subject of it was a recruit, or at least recently arrived from England, of a rather gross habit of body. During the visit one morning, the discharge having ceased, he was suddenly seized with the greatest alarm and fear of impending dissolution. "*My breath is stopped!*" Nor was the least murmur audible on that side by the stethoscope, though the resonance remained perfect. The sensation and signs were exactly as if something had fallen into the left bronchus. Warm fomentations were applied to the axilla, under the impression some revulsion had taken place (there seemed to be scarcely time to

think), and as soon as the discharge returned, which was almost immediately, he was relieved. He had a similar attack some days afterwards, but eventually recovered, and for several years was an effective soldier; nor do I remember what became of him. I may here state, that regiment (the 2d, or Queen's Royal) numbered 854, exclusive of officers, on the day I joined Headquarters in Colaba, Bombay, and when I left them at Deesa, about twelve years after, not fifty of these remained.

Treatment.—On this subject a very few words will suffice. As regards consumption, indeed, not many cases comparatively, we have seen, last over six months. In bronchitis, however, we have been particularly fortunate, and not only so in the East and West Indies, but also in England; attributable, as we suppose, in a great measure at least, to the "*nonnima diligentia*." The pernicious effects of the too common routine of active treatment, we have witnessed certainly in many diseases, and if we except occasionally where there was no disease at all, in none more conspicuously than in bronchitis. A moderate venesection may be beneficial in many cases at the commencement, and cupping in moderation in the course of the disease; but time is the grand remedy; to gain time if possible, without exhausting the vital powers, and of all things to be sparing of blood. Bronchitis tends to death by suffocation or by apoplexy, not to any morbid alteration of structure incompatible with life, like pneumonia, hepatitis, or dysentery; to recovery, by a bland secretion from the air tubes, the work of time only; for though you may bleed to death, you cannot resolve the disease till then. After a moderate venesection perhaps, in recent cases, we exhibit an emetic of ipecacuan, adding a little of the tartarized antimony, and afterwards give the ipecacuan in smaller doses, with calomel or hydrargyrum cum creta (say gr. v. of each) alternately, every two hours or so, with some saline diaphoretic, to which is added either the vinum ipecacuanhæ vel antimonii. Whenever the breathing becomes much oppressed, we prefer trusting to stimulating pediluvia, mustard poultices or blisters to the chest, and say pulv. ipecac. gr. xx., whether it may act as an emetic or not. Nor would we be *busy* with the lancet or cupping-glasses, except dry cupping

perhaps, even under the most unfavorable appearances. (We remember a case of this kind some years ago, where these means were very strongly urged. The reply, however, was, "Never fear, he will be diligent in the morning" and sure enough in the morning it was his answer to the letter, "I'm illigant, I"). Afterwards, when the secretion is becoming copious and loose, or in the more chronic stages, the carbonate of ammonia, with myrrh, bitters with tinct. colchici, quinine, &c. &c.

In some old chronic cases we tried the pil. hyd. tart. ant., and ext. conii, as mentioned in one of the numbers of the *Medical Gazette* of 1842, or beginning of 1843 (our constant moving about prevents us carrying many books with us); but, like other remedies, we found it scotched, did not cure, for the cases soon returned upon our hands.

Many become extremely liable to this disease on any exposure to cold; often, indeed, without any assignable cause. In such cases, only change of climate is to be depended upon: and here Celsus's suggestion would be more appropriate perhaps—"Sic ut densius quam id est, ex quo discedit, aeger," though cold salt water sponging may reduce the susceptibility.

In hæmoptysis, unconnected with phthisis, moderate venesection and cupping, calomel, antimony, and opium, and a succession of small blisters; but the cure is seldom perfect, in the hæmoptote engorgements at least, or permanent, or the normal respiration restored.

Bristol, 8th August, 1850.

ARTESIAN SALT SPRING.

ACCORDING to Dr. Granville, a new Artesian salt-spring has just been opened at Kissingen, in Bavaria. The well is 1878 feet in depth; and the water, at the rate of 100 cubic feet per minute, is projected to the height of 52 feet above the surface. Its temperature is 66°; and it is said to contain 8.25 per cent. of pure chloride of sodium separable by evaporation. It issues from a bed of rock-salt, which has been penetrated to the depth of 138 feet. The rock-salt stratum is supposed to be 1000 feet in thickness.

MEDICAL GAZETTE.

FRIDAY, AUGUST 30, 1850.

WE beg to direct the attention of our readers to the report of another trial for manslaughter by poisoning with *Lobelia inflata*. The northern counties have within the last few years acquired a disgraceful notoriety from the numerous cases of poisoning by lobelia which have occurred in that part of England: and, if we are to judge from the verdict in the case which we now report, it appears highly probable that this benevolent method of destroying one's neighbour will continue henceforth unchecked. A trial by jury for such a crime, in a county in which the lobelia quackery is so extensively diffused that even a verdict of *Not guilty* is received in open court with a "murmur of satisfaction," is surely a mockery. Justice cannot be done. It is time that the *venue* should be changed,—in other words, that a jury should be selected from a county not yet infected with this form of vegetable quackery. For our part, we do not see why the Attorney-General should not, on every future charge of this kind in Cumberland, exercise the power of entering at once a *nolle prosequi*.

The facts are much the same in all the cases.—A friend, with good intentions, administers an active poison without having any experience of its properties, dose, or uses. He gives an *ad libitum* quantity to a fellow-workman just as ignorant as himself, and both have a dreamy idea that the vegetable poison is a "cure for all diseases," that you cannot take too much of it, and the more violently it operates "upwards or downwards" the stronger is the proof that a sufficient quantity has not been taken. If the man dies, it is quite certain that the drug could not have killed him; but that a fit of indigestion or

spasm, for which lobelia is the very best remedy, must have carried him off! There is another curious feature in these cases; this is, that they are sure to meet with a defender in Mr. Sergeant Wilkins. The learned counsel appears to be the Attorney-General of the disciples of Mr. Coffin; and it would be as great a phenomenon to find a Cumberland jury returning a verdict of *Guilty* as it would be to find a case in which Mr. Sergeant Wilkins was not retained for the defence.

Our readers will perceive, from a report of this trial elsewhere inserted,* that there were two simple questions for the jury to decide—1. Did the deceased die from the effects of lobelia? 2. Was it administered ignorantly and unskillfully by the prisoner?

The counsel for the defence, and the learned judge himself, who is described as having been favourably impressed towards the prisoner, made a great point of the admission that there was no imputation on the prisoner's "motives." If this means anything, it is that the prisoner did not wilfully administer the poisonous drug; and the answer to a suggestion of this kind, which an ignorant jury were likely to interpret as a very favourable circumstance to the prisoner, is, that had there been any imputation on the prisoner's "motives," he would probably have been arraigned on a charge of murder, and not of manslaughter. The good-natured judge and the cunning counsel, therefore, concurred in placing before the jury a fact likely to influence their verdict, although, strictly speaking, it had no thing to do with the charge. Manslaughter by poison presumes an absence of criminal motive.

A kind-hearted friend may administer to a person an ounce of bitter-almond water for the purpose of allaying spasms. He may have heard that it will do good

—have administered it, with this view, and his feelings may have been "of the very best and most friendly kind;" but still his friend may die; and the legal question, as we understand it, for making out a charge of manslaughter, is whether death was really caused by the ignorant and unskillful administration of the drug, not whether the man had a good or bad motive in administering it. This principle, would operate on the amount of punishment, not on the nature of the crime. These kind-hearted and good-natured friends, who persist in gratuitously poisoning you out of sheer benevolence, should be taught that if they will prescribe drugs, of the doses and properties of which they know nothing, they must incur the chance of a conviction for manslaughter. This trial for poisoning by lobelia shows us that the death of a fellow-creature, although unquestionably proved to be the result of ignorance and unskillfulness, is not treated as manslaughter by a Cumberland jury; and their verdict of *Not guilty* gives rise to "a murmur of satisfaction" in Court.

We for the present pass over the sophistry and complete ignorance of medical science, rendered all the worse by the assumption of knowledge, displayed in the defence. Our business is to consider whether there was evidence to show that the man did or did not die from the effects of lobelia. The prisoner admitted the administration of the drug, and this was also admitted by his counsel; although the latter took such liberties with the common sense of the jury, as to assert that the green vegetable matter found in the stomach, with *signs of irritation beneath it*, might have been potatoes, greens, or onions, which it does not appear that the prisoner prescribed or the deceased swallowed. The prisoner's counsel disputed Mr. Wilson's evidence respecting the presence of lobelia in the body of the deceased; and while he at one time complained of

the non-production of the vegetable powder found in the stomach, he at another time admitted "that the administration of lobelia was the very best thing that could be done for the deceased," as it would by its emetic properties have the effect of removing from the stomach the putrescent vegetable matter lodged there, and, being antispasmodic, it would cure the spasms!

The quantity of lobelia administered by the prisoner is unknown. The evidence shows that the deceased took two doses from a basin on the Wednesday evening; and it is highly probable that the "dark mixture" sent by the prisoner on Thursday contained the same drug. It is not pretended that the officious friend prescribed anything but lobelia in the treatment of the deceased. Previously to taking the mixture on Wednesday evening at ten o'clock, with the exception of slight heartburn, we learn that he had been in good health. Soon after taking the mixture, he was seized with violent vomiting, which continued throughout his illness, if we except a short remission on Thursday afternoon. The vomited matter is described as of a green or lightish green colour. Death took place in forty-four hours. The stomach and intestines were found inflamed; and, *covering the inflamed parts*, was a quantity of green vegetable matter, recognised by smell and other properties to be identical with powdered lobelia—the substance which prisoner admitted he had given to the deceased on Wednesday evening, shortly before the violent and fatal illness commenced.

The deceased appears to have sunk under the incessant vomiting and the exhaustion thereby produced, as well as from the specific action of the poison. There was no morbid appearance of any kind, irrespective of that caused by the vegetable irritant, to account for the incessant vomiting; and there was certainly no disease of the stomach to ac-

count for it. It is, therefore, we think, incontrovertibly proved by the general and medical evidence, that the deceased did die from the effects of lobelia. There was no other cause in the body to account for death.

The other question is—Was the drug administered *ignorantly and unskilfully*? The prisoner was a foundryman,—had no experience in medicine or knowledge of drugs. He had probably heard of the good effects alleged by a certain class of quacks to be produced by lobelia, and without any knowledge of the properties, or mode of action of this medicine, he gave evidently a very large and poisonous dose to the deceased, probably with the intention of benefiting him. We say that the dose must have been very large; for, without entering into the question of how many doses had been given during the deceased's illness, the mixture of the powder in a basin by the prisoner, the fact that the deceased vomited a greenish-coloured matter for forty hours, and that some of the vegetable powder was afterwards found in the stomach and bowels on inspection, are abundant medical proofs that a large and poisonous quantity of the drug had been taken in one or more doses by the deceased during the forty-eight hours which his illness lasted. These facts clearly establish, medically speaking, that an active medicine must have been administered in a poisonous dose with ignorance and unskilfulness, but probably with the very best and most friendly intentions. If these facts do not establish *ignorance or unskilfulness*, what are the proofs which the law requires? On the other hand, if in this case the verdict were proper, and the murmur of satisfaction with which it was received justifiable, it follows either that the deceased did not die from the effects of lobelia, or that his death was an accidental result of its scientific and skilful administration!

There is an old saying, and a very just one—*Judex damnatur cum nocens absolvitur*; and we are inclined to regard the acquittal in this case as a result of the imperfect charge on the part of the judge.

The speech of Mr. Sergeant Wilkins, embracing the wildest and most untenable propositions in medicine, was allowed to have its influence with an ignorant jury. The learned Sergeant, who is a sort of renegade from the profession, invariably commences, what the newspapers call his "energetic" defence, with an attack on medical men and medical science. When practitioners are dragged into a Court of Law in these cases to give evidence for small fees, and under a serious loss of practice, the learned Sergeant charitably assumes that the *only* motive which actuates them, is one of persecution arising from the interference of unlicensed persons with their profits or their prejudices! Because a blacksmith in Germany (Priessnitz) had introduced hydropathy, *therefore* the Cumberland people were to poison each other with lobelia! He was told by all the medical witnesses that *they* had not employed lobelia, and did not employ it, because they considered it unsafe as a medicine, and because there were other remedies at hand which were safer and more effectual. They had not tried his client's experiments with the drug,—i. e., of giving it in large and poisonous doses from a basin, and therefore they knew nothing about it! "An attempt was made to convict the prisoner of felony on the evidence of men who knew no more about lobelia than any one of the jury." The learned Sergeant therefore thinks that the only medical witness whose evidence will be worth listening to at a trial of this kind, is a practitioner who has incurred the risk of transportation by faintly poisoning half a dozen of his patients with lobelia. No experience short of this will satisfy him. Lobelia

is largely used in America, but only by quacks in dangerous doses: yet "why," said the learned Counsel, "did they not hear of its being productive of mischief?" This would be a fair question, if the learned Sergeant and the Cumberland jury were of the medical profession, and in the habit of perusing weekly the American medical and other journals; but as this is not the case, we do not see that the occurrence of mischief from the use of this drug is disproved, merely because the learned Counsel and the jury have not heard of the fatal cases.

The attempt of Mr. Sergeant Wilkins to account for the death of the deceased from natural causes, is a very fair specimen of the mode in which the facts of medical science are sometimes allowed to be abused and perverted in a Court of Law. Six weeks before swallowing the fatal dose of lobelia, the man had cramp, the result of wind and flatulency: this, it was argued, arose from *impure* gases evolved as a result of fermentation and indigestion. Hence it was inferred that the food in this man's stomach became decomposed and putrescent, and accounted for his sudden death! As persons had died from the effects of constipation produced by overloading the stomach, so it was contended the facts in this case were such as to induce the jury to believe that this man had died from a similar cause! The symptoms were then referred to the sour beer, although they had actually commenced twenty-four hours before any beer was swallowed, and continued to be of the same character throughout the illness. The learned Counsel contended that if the drug produced the vomiting described in the evidence, none ought to have been left in the stomach. This was a question of fact; and although the statement of Counsel is quite opposed to medical experience, it was allowed by the judge to go to the

as an unquestioned truth! The last medical hypothesis broached by the learned Sergeant was to the effect that the absence of purging might have produced all the symptoms seen, i. e. the incessant vomiting of 'green matter' and the patches of inflammation found in the stomach and bowels after death!

This is only a faint abstract of a few of the points urged in defence by the learned Counsel for the prisoner. Our readers, will, we are sure, agree with us that if he had assigned the death of the man to poisonous fungi suddenly generated in the stomach by fermentation, he would have been quite as near to the truth, and would thus have been able to explain in a few words, to the satisfaction of the jury, the cause of the green colour of the vomited matter, as well as the nature of the vegetable powder found in the stomach after death!

As to the charge of the learned judge, we regret to have to express our opinion; that if the report be correct, it could not in any way assist the jury through the mass of medical ignorance and sophistry which had been placed before them in the defence. Thus they were told that the prisoner's feelings towards the deceased were of a "friendly" kind. There was then a dry exposition of the law: they were requested to satisfy themselves that the deceased had died from lobelia, but how, or in what way, they were to become thus satisfied, the learned judge altogether neglected to point out to them. The man died either from lobelia, or from some one of the numerous causes suggested by the counsel for the defence—i. e. flatulency of six weeks' standing, indigestion, fermentation, or constipation of two days' standing! Not the slightest attempt was made to show how the medical evidence disproved these absurdly assigned causes of death. The jury were left to make a conjecture. They were told

that lobelia was not much known in England; that when administered in large doses, and under improper circumstances, the effects were such as many persons were unable to endure.* Even this good-natured way of speaking of lobelia as poison, and as a dangerous and unsafe medicine, was entirely neutralized by the additional remark, that "many drugs, even in the most common use, were, when given in large doses, and under improper circumstances, most deadly poisons." The jury were thus led to associate lobelia with colocynth or rhubarb: and this pharmaceutical association with the "flatulent" theory of Mr Sergeant Wilkins still ringing in their ears, will fully account for the verdict of acquittal, and the murmur of satisfaction heard in Court.

We must express our astonishment that a member of the judicial bench should, with such evidence before him, have left it to the jury whether the deceased died from *indigestion*, or the effects of lobelia! There was not one particle of medical evidence to show that indigestion was even a probable cause of death. Neither the symptoms nor appearances in the body could admit of any explanation on such an hypothesis. It was a mere random suggestion, thrown out by the prisoner's counsel, and adopted as probable by the learned judge, although the medical facts of the case were altogether opposed to it.

When all had thus terminated in favour of the lobelia quackery, the learned judge remarked, "It was to be hoped that all parties of insufficient education and skill would for the future be cautious how they dealt with such drugs as this." Either the verdict was

* This is a truism of wonderful force. It is only equalled by that attributed to M. Boyer, in which he sternly declared his conviction that "to die is a religious duty which every human being owes to his Creator."

ntrue, or such advice on the part of the judge was wholly misplaced. The jury, by their verdict, did not admit that there was *insufficient education and skill* on the part of the prisoner; and any one acting like him as a prescriber of lobelia in Cumberland, needs not to attend to the grave caution of the Bench; for the jury must have thought that it had only those qualities possessed by drugs in common use, or they would not have acquitted the prisoner. Their verdict plainly implies that it requires no medical skill or knowledge to prescribe: any blacksmith or pedlar may safely take up this line of practice; and if a person happens to die after having swallowed a large dose, all that is required is to refer his death to fermentation, or to some form of indigestion!

Trials so conducted appear to us to be a mockery of justice. It would be a great saving of unnecessary expense if the County in any future case declined to prosecute; and after the imputation of improper motives, which, without any censure or remark from the judicial Bench, was thus allowed to be unjustly cast on members of the medical profession, we think it would be only fair if the Cumberland practitioners permitted these "lobelia poisonings" to continue unchecked. In the course of time the fatal cases of "indigestion" would become so numerous as to excite the suspicion of Government, and thus lead to the adoption of some better means of repressing the crime of poisoning by the ignorant and unskilful use of noxious drugs, than appear to be now available in the criminal law.*

* The subjoined case furnishes an additional example of the good-natured way in which these friendly instances of poisoning are disposed of. It was tried in the Crown Court at Liverpool, on the 30th inst. by the learned judge who tried the lobelia case.

"CROWN COURT.—(Before Mr. Justice WIGHTMAN.)

"Robert Lees was charged with the manslaughter of an infant named Elizabeth Corrie, by negligently and unskilfully administering to her an over dose of laudanum.

"It appeared that on Sunday, the 4th of August

Metebos.

The Diseases of the Breast, and their Treatment. By JOHN BIRKETT, F.R.C.S.E., Assistant Surgeon to Guy's Hospital, 8vo. pp. 264. London: Longman and Co. 1850.

THE Jacksonian Prize for the year 1848 was awarded by the Council of the College of Surgeons to Mr. Birkett's essay on Diseases of the Breast. That the award was just, and the essay in every respect worthy the distinction, its reproduction in the present treatise will sufficiently attest. Every known form of disease to which the breast is obnoxious is here accurately described under all its varied aspects and conditions, its history, symptoms, progress, treatment, and termination. The con-

last, the wife of John Corrie went with another woman to a public-house in Dale Street for a glass of ale, carrying with her her infant, which was seven months old. There she saw the prisoner, who observed to her that her child was not well. She said it had been suffering for some days from a bowel complaint; and the prisoner then said he would give her a prescription for the child which would relieve it, if she liked. She assented to this, and the prisoner dictated a prescription, which a young woman present wrote down. '3 oz. of chalk mixture, 1 oz. of gum arabic, and 2 drachms of laudanum: two tea-spoonful to be given as a dose.' This Mrs. Corrie got made up at a druggist's shop, and administered one dose to the child, which, according to the evidence of the medical witnesses, would contain 14 drops of laudanum. The result was that the child became drowsy, and died eight days afterwards. In the opinion of the surgeon who made a post-mortem examination of the child, it had died from the effects of a narcotic poison.

"The prisoner, in defence, alleged that he had never told Mrs. Corrie to put laudanum in the prescription, but 'rhea'; that he had no intention to injure the child; and that he had dispensed medicine for twelve years, and had never committed a mistake.

"His LORDSHIP, in summing up, said the prisoner appeared only to have acted from motives of kindness; but if they were of opinion that he had acted with negligence, want of skill, presumption, and ignorance, they ought to find him guilty. The child had also died at a longer than the ordinary period after the administration of a narcotic poison; and it was therefore for them to consider whether or no the dose administered had caused its death.

"The jury found the prisoner Not Guilty."

This case adds to the difficulty of comprehending what can be legally intended by the unskilful and ignorant administration of powerful medicines. If the accused had prescribed two drachms of Scheele's prussic acid instead of tincture of opium, we presume that, as he had only acted from motives of kindness, there would have been no ignorance or unskilfulness, and that he would have been equally discharged of all legal responsibility for the death!

sideration of these is preceded by a short anatomical description of the healthy gland, and a copious bibliography.

The diseases of the breast are arranged under the following periods by the author.

"I.—*Before* PUBERTY.

"II.—*During* THE ESTABLISHMENT OF PUBERTY.

"III.—*After* THE ESTABLISHMENT OF PUBERTY.

"1. During pregnancy, the puerperal period, and lactation.

"2. At any period or age after puberty."

Few diseases of any importance occur to the breast during the first or second periods; but the author recites from the Medical and Physical Journal an interesting case, of unprecedented extent, of inflammation of the breast, spreading to the whole anterior surface of the body, in an infant about a month old.

In the third period, or that of adult female life, the number of diseases to which the breast is exposed necessarily becomes considerably augmented. Of these the most frequent is inflammation, *mazotitis* (a. Ma^gde), as it is termed by the author. This is divided by Mr. Birkett into,

A. Inflammation, and its results, in the cutis and subcutaneous tissues, covering,

1. The nipple; 2. The areola; 3. The gland.

B. Inflammation, and its results in the tissues behind the gland.

C. Inflammation, and its results in the tissues within the investing fibro-cellular envelope, or fascia of the gland:

1. Of the lobes or gland tissue; 2. Of the uniting tissue.

Mr. Birkett states that he is "quite prepared to deny the existence of true erectile tissue in the nipple." This organ consists, according to the author, almost entirely of yellow or contractile fibre, the advantage of which is, that it arrests the flow of milk when the ducts are distended with that secretion. This tissue exists almost alone at about a quarter of an inch from the apex of the nipple.

"When the nipple is placed in the infant's mouth, the warmth of this organ is sufficient to relax this contractile tissue, as we see in other parts, and the milk readily escapes. Again, that condition of the nipple in which it is said to be in a state

of erection is nothing more than either a peculiar contracted condition of this fibrous tissue, or the result of distension of the lactiferous tubes."

This statement, it appears to us, is not entirely consistent with the following.

"But when the infant is not at the breast, then, by the contractile power of this fibrous tissue, the nipple is retracted, and resumes its quiescent state." (p. 35.)

It seems to involve a contradiction, that the same tissue should be the means both of the erect and of the quiescent state of the same organ. At the same time that the author denies the existence of erectile tissue in the nipple, he admits what is virtually the fact of erection, when he says that

"to enable the infant to suck with facility, the nipple must be elongated; and this condition is effected by the distended state of the tubes." (p. 35.)

To our apprehension, this temporary distension is as veritable an erection as if it had been caused by a flow of blood to special vessels. That Mr. Birkett's view of the cause of what we should still be disposed to regard as an erect state of the nipple—viz., the distension of the lactiferous tubes, is correct, is confirmed by the fact which we have noticed to obtain generally, that where the nipple is defective, there is also a deficient supply or total absence of the secretion of milk.

The remaining forms of inflammation in the breast, the acute and chronic abscess, are all forms of disease with which most practitioners are conversant.

Diseases depending upon the development of cysts and intra-cystic growths come next in order: of these new growths the author observes, various writers have offered explanations, although none have entered very minutely into their organization. They are arranged by Mr. Birkett as follows:—

I. Cysts depending upon dilatation and a morbid condition of the lactiferous ducts or acini.

II. Cysts produced by a peculiar action in the fibro-cellular envelope of the gland-tissue, and the consequence of a morbid state of the function of nutrition.

The existence and formation of the first of these forms of disease are traced by the author with accuracy. The author at the same time explains them by reference to a diagram.

Mr. Birkett thus describes the formation of a disease of the fatger ducts near the nipple, presenting a cystiform character:—

"In the first place an excess of secretion takes place in the duct, which, from some cause or other—malformation of the nipple, or obstruction of the duct by pressure—does not flow away spontaneously, although perhaps it may be made to come out. The fluid becoming absorbed, the more solid material, the epithelium, remains behind, leaving a coherent mass of more or less solidity. This body may cause irritation; an excited action is induced, blastema is effused, and nucleated cells, which attain a degree of fibrillation, are formed: hence the appearance of organized growths found in the ducts." (p. 68.)

This form of disease may be extensively complicated, but remains distinct from carcinomatous disease, although the two may co-exist. The author also points out, with reference to these new growths and their contents, that we may find in them the traces of abnormal nutrition and secretion, and of an abnormal development of gland tissue, also of an arrest of development at certain stages through which we know that all perfectly-formed tissues have progressed. "It is important," as the author remarks, in the consideration of morbid actions, to determine clearly whether or not the morbid product be a disease *in* or *of* the tissue in which it is found.

We quote the author's conclusions in reference to these tumors:—

1. That certain collections of fluid take place in the areolar tissue of the mammary gland.

2. That a cyst or closed cavity is formed, and lined with tessellated or hexagonal epithelium.

3. That the physiological relations of this fluid may be regarded as differing from ordinary serum, and that it may be considered as superabundant effusion of blastema.

4. That the intracystic growths, being developed within the sphere of nutrition of the mammary gland, present more or less resemblance to the gland tissue, both to the unassisted eye as well as when examined with more minuteness.

5. That from the observations above made, these growths may be regarded as an imperfectly-developed gland tissue; and

6. That their tissue presents no resemblance to that of the carcinoma, and that there exists no evidence to prove that it has the power of contaminating other tissues, or of being generated in any other organ belonging to the body.

Hypertrophy of the mammae forms the subject next under consideration.

Under the head, "Diseases of innervation," the author treats of—

1. *Neurodynia*, or a painful affection of the organ generally.

2. *Lebular chronic induration* of the normal tissue.

3. *Lobular imperfect hypertrophy*: a new growth.

Among the various subjects which follow, we notice—Atrophy, Tubercle, Enchondroma, Osseous growths, Hydatids, &c. The diseases of Lactation, Morbid conditions of the Milk, Diseases of the gland in the male, and Carcinoma. Besides these, several other still rarer forms of disease, or morbid conditions of the gland, are noticed by the author. In short, as we have said, every known form of disease will be found to have received notice in this treatise, with all its varied aspects and conditions, its history, symptoms, course, and treatment. A selection of cases illustrates or proves the existence of each form of disease, and a number of well-executed coloured engravings also afford clear ideas of the diseases spoken of. The drawings have been executed by the author himself, and are drawn on stone by Messrs. Hurst and Tupper.

To say that this work stands alone, and forms the most complete treatise on the diseases of the Breast that has appeared since Sir A. Cooper's great work, is not saying too much, as every scientific practitioner who examines it will admit.

TEMPERATURE OF THE EARTH.

In the deepest mine in Ireland, according to Professor Oldham, the increase of temperature below the level of no variation amounted to only 1° for every 85 feet. The mine has been recently deepened from 800 to 1200 feet; but the results of new observations on the temperature are the same, although in another shaft, at a distance of only 800 yards, the increase is 1° for every 58 feet. In England, and on the Continent, the average increase of temperature is 1° for every 46 feet of depth.

Proceedings of Societies.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Edinburgh; August 1.

Is Sulphite of Lead a Poison? Remarks on the Lead process for refining Sugar, and the probable danger arising from the use of Sugar thus refined.

In the Chemical Section of the British Association a question of some importance in relation to public health has been discussed: it is, whether *sulphite of lead* does or does not possess injurious effects by operating as a slow poison on the human system?

Dr. Scoffern read a paper on the *Sugar Produce of the South of Spain*, in which he stated that by the application of his patent process of refining sugar by the use of subacetate of lead, a considerable saving was effected in the amount of labour, in the cost of working, and in the more abundant produce from cane-juice of good crystallizable sugar.

It is stated that the salt of lead which the syrup may happen to dissolve in using the subacetate as a defecator, is separated by passing sulphurous acid gas into the syrup; so that, it is assumed, the whole of the surplus lead is converted to *sulphite of lead*, which is quite insoluble, and, as it is alleged, is entirely separated by filtration. An objection to this process is, that by some accident the whole of the lead may not be separated from the sugar, so that the crystallized loaf or the uncrystallized treacle might contain a small quantity, not of itself sufficient to do harm, except when the sugar or treacle has been used for a very long period of time. In this case, it is presumed, chronic or slow poisoning by lead may result, and be indicated by emaciation, loss of appetite, colic, paralysis, and other well-known effects caused by the slow and insidious introduction of the preparations of lead into the system. Dr. Scoffern, the patentee of this ingenious process, treats this as a groundless fear "which practice demonstrates to be entirely without foundation; for not only is the sulphite of lead most easily removed, but even were it to remain, no injury could supervene, inasmuch as this agent is as *harmless as chalk*." This involves a question of *fact*, and a question of *opinion*. The question of *fact* is, Does any lead, either as sulphite or in any other form, remain in the sugar or treacle as a result of

the new process? and the question of *opinion* is,—Is sulphite of lead—assuming this to be the form in which lead exists in the sugar or treacle—an inert substance, and as harmless as chalk?

On Wednesday, August 14, the subject was referred to in the House of Commons, and the Chancellor of the Exchequer then made the following brief statement.

"THE CHANCELLOR of the EXCHEQUER said that an inquiry had been made into the subject, it having been represented that by the process in question, a quantity of lead must remain in the sugar refined, and be productive, of course, of very deleterious effects. Three chemists of eminence had analysed the sugar so refined, and found that no lead remained in the sugar; but they were of opinion that considerable lead did remain in the treacle, the result of the refining. The inventor, however, who was supported by several chemists, disputed this opinion, and, as the best test he could give of his conviction to the contrary, offered that he and his family would eat any amount of treacle that might be reasonably proposed for their consumption. (A laugh.) However, the clearest way would be for the papers on the subject which he held in his hand to be laid before the House, and he would at once move that they be printed accordingly.—Ordered."

From the report of the analysis we entertain no doubt that the *fact* is settled in the affirmative,—that lead, as sulphite, or in some other form, remains in the sugar in small proportion, and in the treacle in comparatively large proportion, as prepared by this patent process. It matters not whether this has arisen from some imperfection in the process,—a bad filter, a filter breaking,—careless manipulation, or some other error in carrying out the advice of the patentee,—lead exists in the product. This fact rests upon an elaborate analysis made by Messrs. Graham, Thomson, and Hoffman, the three chemists referred to by the Chancellor of the Exchequer. Whatever caused the accidental introduction of lead in the specimens analysed by them, which are said to have been fairly taken, might, of course again operate at some future time; hence the public would never be with certainty secured against the purchase of sugar or treacle contaminated with lead. As a kind of answer to this view, it has been asserted that all varieties of sugar and treacle commonly sold contain more or less lead, owing to the employment of leaden vats or pipes, and iron cones painted with white lead, &c. This is confirmed by the results of the analyses of the three chemists above mentioned; but the quantity of lead found by them in sugar and treacle prepared by the ordinary

process was much less than in that prepared by the lead process. Still it would appear that all sugar is more or less poisoned with lead: and, if we understand the argument rightly, it is contended that the patent process is innocent because ordinary sugar contains lead, and we do not hear of any injurious consequences from its use. We are inclined, however, to place the question on this issue:—Ordinary sugar and treacle, like the patent sugar and treacle, either contain a noxious quantity of lead, or they do not. If ordinary sugar contains as much lead as the patent sugar, it is time for Government to interfere and prevent the use of lead in its manufacture; if it contains a much smaller, — i. e., an infinitesimal proportion, the argument is *inutil ad rem*; it then becomes a mere question of degree. At the same time, we know of no proportion of lead in any kind of sugar which can be said to be *innocuous*; it is in all cases a prejudicial impurity; and the discovery made by the chemists should therefore rather excite the Government to prevent its introduction into ordinary sugar, than to sanction its use on a still larger scale in the new manufacture of sugar. We have, however, heard it seriously contended that, because there is some portion of lead in common sugar, we ought to throw no obstacle in the way of employing the salts of this metal in the preparation of this universal article of food. Such an argument simply amounts to this. The English public are already poisoned to a certain extent by the accidental introduction of lead into sugar. Why should they object to be poisoned to the same, or, as it is alleged, to a greater extent, by the actual employment of the salts of lead in the sugar manufacture?

This, however, may appear to be begging the question, and here we fall back upon the matter of *opinion*:—Is sulphite of lead capable of acting as a slow poison, like carbonate of lead, or is it as harmless as chalk?

This was the question brought before the Chemical Section of the British Association. We take from the *Athenæum* report the following extract:—

Observations on the Sulphite of Lead. By
DR. GREGORY.

"Dr. Gregory stated that he had made experiments on the sulphite of lead formed in this process. He admitted that an infinitely small proportion might still remain in the sugar, but that he considered it quite innocuous. He had indeed fed rabbits and dogs with food which had been united with this sulphite of lead, and the result was that they thrived amazingly, showing no symptoms of any of the known effects of

lead. Dr. Gregory also remarked that, in testing sugar for lead with the hydro-sulphuret of ammonia, iron was often mistaken for the former metal.

Dr. CHRISTISON contended that we had no evidence that sulphite of lead was innocuous. It was true that in cases of poisoning by carbonate of lead sulphuric acid was administered to convert it into the comparatively insoluble sulphate; but this was a case widely different from the slow accumulation of lead in the system. Dr. CHRISTISON adduced some examples of exceedingly small doses of lead being taken in water for more than *twelve months* before its evil effects became apparent. He therefore thought it yet remained to be proved that the sulphite of lead was without action on the system, since we know nothing of the influence of the solvents it meets with in the system, or of the influences of vital action. Rabbits, he was prepared to say, should be entirely rejected in these inquiries, since he had found that they were not affected by many poisons. Dogs and cats were the only animals which could, from their internal structure, be regarded as the representatives of the human system in these investigations."

We must express our astonishment, that a man holding Dr. Gregory's position in the University of Edinburgh could treat such an important question as this, affecting as it does the health of millions, in such a loose and careless manner. Dr. Gregory is known as a chemist,—certainly not as a physiologist or a toxicologist,—and this is the only reasonable excuse we can find for the inconclusive nature of his experiments with the sulphite of lead. His chemical objection, or rather insinuation, against the results of the analysis of sugar,—namely, that iron may be mistaken for lead by the use of hydro-sulphuret of ammonia, cannot be considered applicable to the results of Messrs. Graham, Thomson, and Hoffman. They obtained the lead in the form of sulphate, and took those precautions which men of their experience and standing would unquestionably take, to prevent the occurrence of such a fallacy as that suggested by Dr. Gregory.

Dr. Gregory gives no explanation *why* he considers the sulphite of lead innocuous. As Dr. CHRISTISON very properly stated in reply, there is *no evidence* to support this view. The special effects of chronic lead-poisoning are undoubtedly traceable to the *oxide* of the metal, and not to the acid with which it happens to be combined. It is true that, like the sulphate of lead, the sulphite is insoluble in water; but insolubility in water cannot be received as any proof that a

substance exerts no poisonous action on the human body. Calomel and white precipitate are just as insoluble in water as sulphite of lead; and the same remark applies to Scheele's green and carbonate of lead: but it would be contrary to daily experience to assert that calomel, Scheele's green, and carbonate of lead, were therefore innocuous! The fact is, the human stomach, with its acid and mucous secretions, presents a solvent differing from water, and an absorbing surface of great extent and activity. The absorption of a mineral poison cannot, therefore, be regulated by the mere solvent action of water upon it. This is proved by the rapid absorption of powdered arsenic, and other mineral poisons not easily dissolved by water. There is no other reason, that we are aware of, for asserting that sulphite of lead is innocuous, than that derivable from the fact that we have had as yet no experience of its action on the human body.

The recent researches of M. Bernard* on the phenomena of absorption, show that substances insoluble in water are susceptible of absorption, and that the old doctrine—"corpora non agunt nisi soluta sint," is not founded on fact. Vermilion and Prussian blue, used in tattooing, have been detected in the neighbouring glands. It would be impossible to explain the action of a small dose of calomel on the liver, except on the supposition that it must become absorbed by the mucous membrane. Mercury has been found in the liver, and in all the soft organs of persons to whom calomel has been administered. How has this most insoluble substance been conveyed there, if not by absorption? Until experiments show the contrary, it is reasonable to assume that sulphite of lead is similarly absorbed. If absorbed, has it been proved that the sulphurous acid is so complete an antidote to the poisonous properties of oxide of lead, that the sulphite thus introduced into the circulation cannot act on the system like other salts containing oxide of lead? We do not find that Dr. Gregory has performed any experiments on the absorption or non-absorption of this substance, although such an inquiry was highly important before any inference was drawn by him respecting the innocuousness of the salt. The quantity absorbed might, it is true, be too small for detection, unless the administration had been continued for a very long period of time. If, however, lead had been detected in the tissues of the dogs and rabbits to which he gave the sulphite, he would have had great difficulty in persuading physiologists that the ab-

sorption of this substance might go on without in any way affecting health, or producing, after some months, the usual consequences of lead-poisoning.

It is very likely that a dessert-spoonful or a table-spoonful of sulphite of lead might be swallowed by an adult without acting as a poison or causing death; but this proves nothing with regard to the case under consideration. Carbonate of lead swallowed at once in a large dose is equally innocuous; but this substance taken day by day diffused in water, in the proportion of only from 1-60,000th to 1-100,000th part by weight, produces all the effects of chronic lead-poisoning. As Dr. Christison observes, it may take twelve months to produce these dangerous consequences, but in the end they will appear. How has Dr. Gregory met a case of this kind?—"He has fed rabbits and dogs with food which had been united with sulphite of lead, and the result was that they thrived amazingly, showing no symptoms of any of the known effects of lead." His conclusion, therefore, is, of course, that sugar containing sulphite of lead, as he says, in infinitely small proportion, though without giving any numerical results, may, with the treacle, be used by the public with the certainty that after any period of time they will not suffer from any of the effects of lead-poisoning!

Dr. Gregory's experiments, if they prove anything, have the unfortunate tendency to prove too much. He has actually discovered in the sulphate of lead a "chemical fattener." The animals fed with it not only showed no symptoms of lead-poisoning, but *throve amazingly!* The attention of the framers of poor-law dietaries might, therefore, according to this curious result, be profitably turned to this branch of industry. We can hold out no hope, however, that there will be any saving in the article of food by mixing it with sulphite of lead. The experiments are inconclusive and unsatisfactory. They throw no light upon the truth of the case, and have a direct tendency to mislead those who cannot perceive the numerous fallacies which they involve. It is not stated by Dr. Gregory how many dogs and rabbits were made the subjects of these experiments—what quantity of sulphite of lead was given at a dose—whether this sulphite of lead was or was not in the same impalpable state of division in which it exists in the patent sugar, or of suspension and probable intimate combination in which it exists in patent treacle—for how long a time the sulphite was given, and the nature of the food with which it was united. All these are facts required to test the correctness of conclusions which, if erroneous, may merely have

* L'Union Médicale, Septembre 1849.

the effect of spreading an epidemic colic throughout the British empire. We therefore quite agree with Dr. Christisen, that it yet remains to be proved that sulphite of lead is without action on the system; and the onus of proof lies with those who assert that it is innocuous, since *oxide of lead* is known to be poisonous, and there is nothing to show that its combination with sulphurous acid *renders it inert*. A few dogs, cats, and rabbits, are not the proper objects of experiment for solving a question upon which the health of a nation may depend. The actual effect produced by administering powdered carbonate of lead united with food to dogs and rabbits has not, so far as we know, been tried. It appears to us that, before drawing so sweeping a conclusion from the negative results obtained from sulphite of lead, Dr. Gregory should have tried the carbonate in the same doses and under the same circumstances, as a preliminary experiment. Negative results might have been here obtained, and in this case it would have been proved that the action of small doses of a known poison on animals is influenced by too many circumstances to allow of any fair comparison with their effects on man. It might have been shown that carbonate of lead given to these animals not only did not produce the known effects of lead, but that the animals while taking it thrived amazingly. In fact, it would probably have been thereby clearly established that carbonate of lead is quite *innocuous*; and that the accounts published of individuals suffering from drinking water containing only 1-70000th part of its weight of this salt, are fabulous tales.

It is assumed by Dr. Gregory that sulphite of lead is the only form of lead-salt contained in the patent products. In the treacle, as we infer from the report, it is mixed with acetate of lime, and is in an intimate state of combination. Dr. Gregory's sulphite, we presume, was prepared and given under widely different circumstances.

It has been stated that the patent sugar and treacle may be, and have been, taken with impunity for some time by adults and children without ill effects. This is possible; but still even such a result, if free from fallacy, does not exclude danger to the public in general by the use of an article of food which is liable, when well prepared, owing to unavoidable accidents, to contain a small quantity of lead, and when ill prepared to contain a dangerous quantity. How are the public to be guarded against a noxious impregnation, when the quantity of lead present, even under skilful hands, is liable to great variation? As to the negative results obtained from the consumption of

this patent sugar by ten or twelve persons for several months, they prove nothing conclusively. In the case of the ex-Royal Family of France, poisoned by the impregnation of the water of Claremont with lead, only 13 out of 30 persons suffered from the effects. This shows that not more than *one in three* may be affected by lead-poison in a given time—i. e., seven months. Had the 25 who escaped out of the 30 been the only inmates, it might have been "considered" that water containing one grain of lead in a gallon was "quite innocuous," and might be consumed without risk. Some persons are undoubtedly more susceptible of the effects of lead than others: a few may suffer in three or four months; others, as Dr. Christison states, not until after twelve months. The public health, however, must be protected, even although not more than one person in twenty might sustain injury from the use of the patent sugar or treacle, and although it might require many months to produce serious effects on the human constitution. It is a case in which we believe it to be impossible to obtain any fair criterion of safety except at very great and unnecessary risk.

THE PROVINCIAL MEDICAL AND SURGICAL ASSOCIATION.

THE eighteenth anniversary meeting of this Association took place at Hull on the 7th and 8th of August, and was very fully attended by medical practitioners from all parts of the kingdom. Dr. Horner, of Hull, took the chair which was vacated by Sir Charles Hastings. An appropriate address was delivered by the President, and the Report of the Council on the various subjects which have occupied the Association during the past year was then read. Several of these, with the reports thereon, have already found a place in our pages. From the financial statement it appears that the affairs of the Association are in a very prosperous condition.

The statement of accounts is as follows:—

Balance in hand	£	16	2	6
Receipt	.	1616	4	2
Total	.	1632	6	8
Expenditure	.	1522	8	2
Balance in hand	.	109	18	6

We are glad to find by this report that a committee has been appointed to draw up a code of *medical ethics* for the government of the Association, which we believe now numbers upwards of 1500 members.

Brighton has been fixed upon for the place of meeting in 1861, and the anniversary address will be delivered by Dr. King.

Hospital and Infirmary Reports.

ST. BARTHOLOMEW'S HOSPITAL.

Two Cases of Cancer of the Penis—Amputation.

O. HAWKINS, a stable-man, *æt.* 46, in the habit of drinking beer and gin freely: has had syphilis several times, but not within the last few years; has never experienced any difficulty in withdrawing the foreskin; about nine months ago discovered a sore on the lining of the prepuce, and showed it to a surgeon, who administered mercury and salivated him: the sore, however, continued to spread; and he left off the medicine, and did not seek any further advice until July 12, 1850, when he came to St. Bartholomew's Hospital, and was admitted under Mr. Lloyd. A large irregular ulcer, with thick edges, covered here and there with small florid granulations, and with a foul thin discharge, occupied the whole extremity of the penis; the glans was entirely destroyed; the remainder of the penis was hard and swelled, and the skin was red and thick. The glands in the groin were not enlarged. There was a varicocele on each side. The hardness of the penis extended so far back that there was some difficulty in determining whether all the disease, which was decidedly malignant, could be removed: this, however, was considered practicable, and the only means of stopping its progress, and was therefore recommended to the patient.

Aug. 6th.—Chloroform was administered, and Mr. Lloyd divided the integuments around the root of the penis, so as to separate it from the scrotum and pubis, and then, drawing the organ forwards, divided it just in front of the symphysis. The entire diseased structure was removed. A few arteries were tied, and a gum elastic catheter was passed into the bladder, and secured from slipping out.

19th.—The patient is going on well; the wound is suppurating and granulating kindly; the catheter remains in the bladder.

John Bradford, *æt.* 69, a cook, residing in London, has had a sore on the penis for nine years, which commenced on the lining of the prepuce, and gradually spread to the glans and orifice of the urethra, and occasioned a great deal of pain in making water; has always had phymosis.

Aug. 9th.—Was admitted into St. Bartholomew's Hospital, under Mr. Skey: the prepuce could not be drawn back, and was therefore divided, and an ulcerated surface, from which sprung an irregular shaped

nodulated diseased mass, having the appearance of an accumulation of warts, with a thin foul discharge, was exposed on the extremity of the penis in the place of the glans which had been destroyed. Several glands in each groin were hard, and slightly enlarged. As the disease extended very little beyond the glans penis, and the remainder of the organ appeared sound, and as its character was decidedly malignant, amputation was advised and consented to. **17th.**—The patient was brought under the influence of chloroform, and Mr. Skey divided the penis, about its middle, with one stroke of the knife: a few vessels were secured, and he was sent to bed.

Removal of the Head of the Femur.

Ann Sugg, *æt.* 13, fell over a skipping-rope about three years ago, and received some slight contusions about her left hip; inflammation of the joint followed: she was unable to use the limb, and suffered a great deal of pain in the knee. About a year after the accident she was able to limp about, but could only get the toes of her left foot to the ground, as she was unable to extend the knee or hip joints. In a few months abscesses formed around the hip, and burst; and for the last seven months some of them have remained open.

June 6th, 1850.—Admitted into St. Bartholomew's Hospital in an extremely emaciated condition, and nearly worn out with suffering. The left femur was dislocated on the dorsum lili, the limb shortened, and the leg and thigh flexed: there was a large ulcerated surface over the trochanter major, through which the bone threatened to protrude, with burrowing sinuses in the neighbourhood discharging pus freely. After being in the hospital a few weeks the child gained a little flesh, but she has lately fallen off again, not being able to stand the pain and continued discharge of matter from the wound. It was considered that removing the head of the bone would give the patient the best chance of recovery; and the operation was performed on August 17th, by Mr. Skey, the patient being under the influence of chloroform. As the end of the bone was only thinly covered with granulations, a very little cutting sufficed to expose it; this being accomplished, the limb was carried inwards, and the bone divided with the saw just below the great trochanter. The granulations bled freely on being cut, but the hemorrhage soon ceased, and no ligatures were required. The integuments were too firmly adherent to the parts beneath to admit of being drawn together; the wound was therefore left open.

The acetabulum was found to have been enlarged by absorption, and was extended

in a direction upwards and backwards, as if an attempt had been made by nature to form a new joint in this direction. The head of the fetus had been entirely absorbed; a portion of the neck remained, which, with the great trochanter, was the part removed: on dividing this with the saw it was found to be soft, and composed of vascular cancellous structure, with a very thin outer layer of compact bone.

Excision of a Malignant Tumor from the Thigh.

O. T., *et. 69*, a commercial traveller, with a florid complexion, singularly formed head, simple expression and manners, has always enjoyed good health, and been in the habit of taking strong walking exercise, states that three months ago he first experienced a painful sensation in the front of his right thigh, and soon afterwards discovered a lump there the size of a cob-nut, which has gradually increased.

He was admitted under Mr. Stanley on July 30th. A tolerably firm tumor about the size of an orange, over which the integuments readily moved, tender on pressure, and sometimes so painful as to prevent his sleeping, was embedded in the muscles on the front, and one-third from the top of the right thigh. The glands in the groin were not enlarged. Its removal was readily consented to.

August 10.—Mr. Stanley exposed the tumor by a vertical incision: it was situated in the rectus muscle, from which it readily separated, and was quickly removed. On examination, the tumor was found to be of a malignant character.

19th.—The wound is granulating, and the patient going on well.

Excision of a Chronic Mammary Tumor.

M. G., *et. 26*, a healthy unmarried girl from the country, states that five months ago she received a blow on the inside of her right breast, which gave her a little pain at the time, and was soon forgotten. Three weeks after this, she accidentally discovered a small lump, not quite so large as a nutmeg, in the same situation, which has gradually increased in size, but has not been at all painful.

August 3rd.—Was admitted under Mr. Stanley. A tumor the size of a small orange, firm, but not hard, and very moveable, was situated immediately beneath the skin and on the inner side of the mammary gland, which was quite healthy: none of the neighbouring glands were enlarged, and the tumor had every appearance of being non-malignant. The patient, however, was anxious to get rid of it; and on Aug. 10th its extirpation was effected with the greatest ease while she was under the influence of

chloroform. The wound was closed, and is now (Aug. 19) granulating and healing fast. The mammary gland was uninjured. The tumor was one of those which have been described as chronic mammary or pancreatic, as their structure somewhat resembles that of the pancreas.

PROPOSED EXHIBITION OF ETHER VAPOUR TO THE DYING.

OUR new contemporary, the *New York Medical Gazette* (July 18), in commenting on Dr. Warren's valedictory address, condemns his proposition to administer ether vapour to the dying for the purpose of easing the pangs of death.

"The agony of death," observes Dr. Warren, "is a fair subject for the influence of ether. This supposed agony, in the greater number of cases, perhaps, is a creature of the imagination, conjured up by fear. Nature, like a kind mother, has presented the phenomena of death with such accompaniments as she thinks necessary to deter us from resorting to it by our own act. But while the entrance to the dark passage appears frightful, as we pass on it becomes less and less distressing, till it terminates gently in insensibility. Some instances there are in which there is no real suffering. In these we have an invaluable friend in ether. And when the mortal strife is so far advanced as to leave no hope nor chance of recovery, the duty of a physician is to interpose the means by which we may be softly and agreeably conducted through the final moment. This practice I adopted many years before ether was introduced into surgery, and I have followed it lately, though with great caution, and never without the clear knowledge and full assent of friends."

On this our contemporary remarks:—"God forbid that any physician should ever assume to know infallibly that 'there is no hope or chance of recovery' in any case; for as learned and experienced men as Dr. Warren himself have often been mistaken on this point. And should the discovery of ether lead to the general or frequent use of this 'means of conducting' those who are supposed to be dying 'through the final moment,' it were better for the next generation that the discoverers had never been born. No 'knowledge, or full assent of friends,' in our opinion, can justify such a practice."

. We must also strongly express our disapprobation of the extraordinary proposition made by Dr. Warren. If the plan were adopted it would give rise to the greatest criminal abuse. It is the law in this country, that whatever accelerates death causes death, and a practitioner thus using ether vapour might find himself subjected to a charge of manslaughter.

Medical Trials and Inquests.**CUMBERLAND ASSIZES.**

Carlisle, August 3.

Before Mr. Justice Wickham.**Trial on a charge of Manslaughter for Poisoning by Lobelia Inflata—Acquittal of the accused.**

Wm. TAIT was charged with the manslaughter of William Hewitt, at Parton, on the 19th of June last, by administering to him a certain quantity of lobelia inflata, mixed with water. Prisoner pleaded Not Guilty.

Mr. JAMES and Mr. HODGSON appeared for the prosecution, and Mr. Sergeant WILKINS for the defence. Witnesses, at the request of Mr. Sergeant WILKINS, were ordered to leave the court.

It appeared, from the statement for the prosecution, that prisoner and deceased were fellow-workmen at an iron-foundry. Deceased was 25 years of age, and a man of good health. About five weeks before this occurrence he had had medical attendance for a slight gastric disorder of a temporary kind. From this he speedily recovered, and was engaged at his work as a founder until Wednesday, the 19th of June. He returned home at eight o'clock in the evening, and, it seems, complained of heartburn and indigestion. About ten o'clock that evening, the prisoner was seen mixing something in a cup, which he said was *lobelia*. The deceased was at the time in bed. The prisoner gave this mixture twice to deceased, and in about half an hour it produced vomiting. The vomiting continued, more or less, from that time, until the deceased died on Friday afternoon; but there seems to have been a remission of the symptoms on Thursday afternoon, so that deceased was able to get up and go out. The deceased had no convulsions or fits of any kind, and retained his senses until the last. The colour of the matter which he vomited was green. His stomach was much swollen. On Friday morning, as the deceased was very unwell, the prisoner was sent for. He came, and gave deceased some more medicine, the nature of which was unknown. Deceased subsequently had some small beer, which had been fetched from a neighbour's. It appeared that he had had some beer on Thursday, which a witness described as having a sharpish taste. He also had more medicine from the prisoner on that day. In fact, it appears that he had been

dosed with medicine by the prisoner from Wednesday night until Friday morning, when he was first seen by Mr. Hilton, a medical practitioner. We subjoin the medical evidence:—

John Bateman Wilson examined by Mr. HODGSON.—I am a member of the College of Surgeons, and practise at Whitehaven. I attended deceased about six weeks before his death. At that time he had a slight stomach complaint which is generally called spasm, produced by flatulency and wind on the stomach. I prescribed for him on that occasion, and he was relieved in the course of half an hour. I saw him again on the following morning, and he was so much better that I did not visit him again. He appeared to be a healthy man. On the morning of Friday, the 22nd of June, deceased's brother came to me, requesting me either to visit him or send him something. I sent him the same medicine which I had previously administered to him,—namely, two doses of calomel, the one to be given three hours after the other if he did not feel relieved. From what his brother said, I considered he was suffering from his former complaint. I got a friend to go that morning at eight o'clock to see him, and I went myself at three o'clock in the afternoon. When I saw him he was in a state of prostration. He was conscious, and recognised me, and held out his hand to grasp mine. From his being in such a reduced state, and from being two miles from my residence, I ordered a little brandy and water, for the purpose of supporting him as much as I could, although I knew it would be of no use, as he was then in what medical men call the sinking or the dying state. He was dead when I returned. There was no message sent me, and I returned between six and seven o'clock to make further inquiry, and he was dead. From the symptoms attending his death I considered he died from the effects of a poisonous substance. I form that opinion from his having been perfectly well on Wednesday, before he had something given to him, from the fact of death occurring so rapidly, and from what his relatives told me. I visited him on Monday, the 17th June, on account of an accident which happened to his finger. He appeared then in general good health. He made no complaint, nor did he look ill. After his death I thought that further inquiry should be instituted. I made a *post-mortem* of the deceased. Mr. Thompson assisted me on that occasion, and there were present two other gentlemen. The external examination of the body demonstrated a muscular young man. On removing the body from the bed to the table a quantity of fluid issued from the mouth

containing *vegetable matter*, which had a very sickly odour. A portion of the fluid I retained for further examination. On opening the chest we found the heart healthy. The lungs were congested at the posterior part. There was nothing else remarkable about them; but, on opening the abdominal cavity, we saw, through the coats of the stomach, a collection of a dark substance, which we likewise observed in various parts of the bowels. It was a *dark greenish powder*. His stomach I removed for more minute examination, and a portion of the bowels as well. In another portion of the bowels I found small patches of *this vegetable powder, with redness under these patches*. The liver was pale, and what we call flabby or soft. The gall-bladder contained a quantity of dark and thick bile. The spleen and pancreas presented no unnatural appearance. The kidneys looked paler than usual, but in what we call the lower portion, and in each of them, two drops of matter were found. On examination of the brain there was found some congestion, but not more fluid than usual in the ventricles. The stomach and a portion of the bowels I took care of and locked by. At a later part of the evening, with the other gentlemen, I examined the stomach. On opening it, the same disagreeable sickly smell issued from it. The stomach was *generally inflamed*; and there were *patches of vegetable matter, and under these there was a vivid redness. A portion of the bowels was likewise found red*. There was also the same smell, though not so distinct, as in the stomach. I applied a test with respect to this vegetable matter. I used nitric acid, which produced a reddish-brown colour, and sulphuric acid, which produced a dark brownish appearance. These two together go to show there was the presence of vegetable matter. They have been recommended as tests for lobelia, but they are tests for other vegetable matter, and I do not consider them alone sufficient tests for lobelia. From the tests applied, and taking all the circumstances into consideration, I consider that *deceased died from the effects of a poisonous irritant, causing inflammation of the stomach*. I have seen a powder called lobelia, but have not been in the habit of using it. It has a peculiar smell. The smell which issued from the stomach was that of lobelia. I purchased lobelia that evening, and then I found that it had the same smell as that which proceeded from the stomach and mouth of deceased. I never was in the prisoner's company except before the coroner and magistrates. Cross-examined by Mr. Sergeant Whelan.—I do not recollect that I ever administered lobelia in my life. I know it is a native of

America. It is used extensively there in dangerous doses. Its principal use is as an emetic. I may put it under the same class of smells as that of new-mown hay. It was about three weeks previous to the 21st of June that deceased injured his finger, and after I attended him for his stomach complaint. Supposing the finger continued to suppurate and exhibit proud flesh up till the time of his last illness, it would not indicate an impure state of constitution. Supposing that the finger was injured a month before, and continued to make proud flesh, and no healing process had begun at the time of death,—I say again, as I value my reputation, that it would not argue an improper state of the fluids. The healing process generally commences in about a week or ten days, but this case did well. The coat of the stomach is irritable in its nature. Spasm is created by an inordinate extension or contraction of the stomach. Spasm is the result rather than the cause. Want of proper action in the system will produce wind and flatulency. Indigestion will cause a want of action in the system. I never knew a case of indigestion where the food remained in the stomach until it was in a putrescent state. You may call it putrescent sometimes, when it is a while in the stomach without being digested. I never saw or read of a case where food remaining on a weakened stomach has produced all the irritating consequences of vegetable poison. I have known a person to die of Asiatic cholera. I have known two fatal cases of British cholera, but never had any of my own. When persons die of British cholera, it is produced by irritation of the stomach and bowels. Death from such a cause is sometimes sudden. Supposing I had known nothing of lobelia, and had not known that deceased had taken it, I would have attributed his death to some direct irritant applied to the stomach. Supposing the stomach to have been for a long time unemptied, and the food remaining in it till it was putrescent, that would not have been an irritant directly applied to the stomach, for there would have been purging with it. The very fact of constipation of the bowels would not act on the stomach and prevent purging: inveterate constipation, however, would. I do not know that deceased had no motion in his bowels for nearly three days. He had a motion on Wednesday. I know this by questioning his brother when before the magistrates. The circumstance of the gall-bladder being full of dark thick bile indicated that: here had been some obstruction to the complete flow of bile from the gall-bladder. In some individuals, it is the reverse, and the bile not

spread as it should be, irritation of the most dreadful kind will be produced; but I have not seen or heard of irritation of that kind producing death. I found some pus in the kidneys. The kidneys were healthy. There was nothing in the kidneys but that pus: it indicated inflammation. Irritation will produce inflammation, which will lead to the formation or deposit of pus in the kidneys. When Thomas Hewitt came to me he told me his brother's stomach was swelled in the same way that it was when he was at Whitehaven. *By Mr. Hodgson*—It is my decided opinion that death arose from nothing else but this vegetable matter which had produced inflammation. *By His Lordship*—I have never administered lobelia myself. I have, however, seen it administered, but never in doses exceeding from ten to twenty drops when in the form of tincture. It has been used in cases of asthma, for the purpose of producing vomiting and affording relief to the chest. *By Mr. Sergeant Wilkins*—I know that Taylor recommends eighteen to twenty grains as a dose.

William Richard Hilton, examined by Mr. Hodgson—I am a surgeon at Whitehaven. I attended deceased about half-past nine o'clock on the morning of the 21st of June. When I got there he was complaining of a burning pain. There was a good deal of cramp in the stomach and severe vomiting. He vomited very frequently while I was there. *What he vomited appeared to be a greenish brown vegetable powder.* In consequence of what he told me of his symptoms, I prescribed soda-water to be taken immediately to allay the vomiting; I ordered an injection; I prescribed also some pills the effect of which was to relieve vomiting, from whatever cause it proceeded. I was present at the post-mortem examination. *[Mr. Sergeant Wilkins said that it would not be necessary for Mr. Hodgson to go into the post-mortem examination with the witness. He then proceeded with the cross-examination of Mr. Hilton:]*—The case of the deceased was not attended by all the symptoms of violent cholera: there was no purging in this case at all. I did not administer the injection and blister, but I believe they were administered. After the administering of the injection I did not see the contents of deceased's bowels. I would not call lobelia a pure narcotic; it is a narcotico-irritant poison. Lobelia is a medicine very much used in the United States of America: it is used as an emetic there. I know that it is given there in larger doses than a scruple: it is given in dangerous doses. Twenty drops of tincture will contain about two grains of lobelia. Supposing I had visited a patient, and found

him labouring under the influence of lobelia, I would have given him an emetic if he had taken a large quantity. Dyspepsia means disordered digestion. If a stomach labour under the duties it has to perform for some time, it will be certainly weakened in its powers. The stomach is not as irritable as the pupil of the eye. Without comparison, it is exceedingly irritable. I have been ten years studying and two years in practice.

John Thomson, examined by Mr. Hodgson—I am a Member of the College of Surgeons, and practise at Whitehaven. I was present at the post-mortem examination of deceased. I have frequently prescribed lobelia. *Cross-examined by his Lordship*—In large doses, I should call lobelia a narcotico-irritant poison. It is used as medicine in small doses; in cases of asthma it is used as an anti-spasmodic; it is also given as an emetic. From what I observed at the post-mortem examination, I think deceased died from the effects of an irritant vegetable poison. *By Mr. Sergeant Wilkins*—Lobelia has been used here for some years in small doses: its nature is not so well understood as that of other medicines. I know there are some very eminent medical men in America: I have heard that lobelia is commonly used there in large doses. I found, at the post-mortem examination, that the internal coat of the stomach was a good deal inflamed; in this case, the inflammation was not produced by the deranged state of the stomach. Supposing the stomach to have contained food for some time in an impure state, inflammation would have been produced, but not such as in this case, where it was in patches. I have got none of the stomach now: by this time it will be decomposed. I did not think it was necessary to preserve any of the vegetable matter. *By Mr. Hodgson*—Lobelia is a most dangerous medicine. *By his Lordship*—It has a peculiar smell. I have felt the smell of what is sold in the shops as lobelia, and the smell which issued from the stomach of the deceased was the same.

Richard William Broster, examined by Mr. Hodgson—I am a surgeon at Whitehaven, and attended the post-mortem examination of William Hewitt. From the symptoms I observed on that occasion, I consider deceased died from inflammation of the stomach. I would consider it was caused by what was found in the stomach. I believe it was lobelia. I never had anything to do with lobelia myself. I consider lobelia to be an irritant: I would designate it a narcotico-irritant. I consider it a dangerous medicine.

Mr. Sergeant Wilkins then addressed the jury on behalf of the prisoner. He

said it was a fortunate thing for the prisoner, who was as respectable and respected a young man as existed in this country, that he was not to be tried by members of a profession who were always aroused by anything which threatened to interfere with their profits or their prejudices. It was a matter of history, and he said it advisedly, and begged the attention of the jury to the fact, that the medical world had done more to impede the progress of medical science than all the rest of the world put together. It was a remarkable fact that the greatest men which the medical world had ever produced had, in their day and generation, been persecuted and scandalised when they dared to stand out from the beaten track of the profession. They had been told that Tait was simply a man who wrought in a foundry, as if that were to be urged as a reproach against him. It was a notorious fact that the man who had made the greatest discovery of the age in medical science was a blacksmith in Germany—the author of *hydropathy* was Priesnitz, and he hoped to see the day when *hydropathy* would be fully carried out. They had it in evidence that this medicine, called *lobelia*, was making rapid progress in America. There, where they had first-rate medical skill, it was becoming general and universal in its application. And who were the men to call it a dangerous medicine—who were the men to impugn its effects? In this evidence, which it was shocking and revolting to contemplate, an attempt was made to convict the prisoner of felony on the evidence of men who knew no more about *lobelia* than any one of the jury. The gentleman who had been last examined had never used it in his life. One had seen it administered by others, but had never administered it himself; and the first gentleman (Mr. Wilson) said he was not likely to run the risk of poisoning his fellow men by using it. Not one of them had described it. What had they learned from these four medical gentlemen that they did not know before? They had said that *lobelia* was used in dangerous doses in America. How did they know that? They had never tried the experiment. The medical doings in America were as well known in this country as were the legal, commercial, and philosophical doings of America; and why, therefore, had they never heard of this before? When a man prescribed four grains of calomel he did it on the authority of experience—he did it on the authority of some cases stated

in the medical world. Almost everything done in the medical world was based on precedent: the men did not think for themselves; they learned the uses of the various medicines from their books: their pharmacopœias told them that the use of such and such medicines were proper in such and such cases, and they shaped their course accordingly. And what had this man done more? There was his authority. He said—"I fetch Mr. Wilson, the man who institutes this prosecution, and Mr. Wilson himself tells you that in America it is largely used, and that in what he calls dangerous doses." If it was attended with such very great danger in America, why did they not hear of its being productive of mischief? Let them, however, take a short history of the case itself. Let them look at the position of the prisoner and deceased. They were on friendly terms with each other. Deceased had gone to the prisoner—the prisoner had not obtruded himself on deceased. The father, brother, and sister, said that he became sick within half an hour after the prisoner first gave him the *lobelia*, and that he continued so up till the time of his death. That could not be if they believed the father possessed the feelings of a father. If he had thought that the prisoner had given him anything wrong, why did he send for him again? The prisoner had been most attentive to him—had been with him on one occasion for three hours—had gone when sent for during the night, and not a farthing was he to get for his trouble. If he had asked the medical men separately what they would have given deceased in the circumstances in which he was placed, he laid his life to a shilling that every one of them would have given a different prescription. It would be best for society if post-mortem examinations were more frequently held; and he took it for granted that if some of these gentlemen were subject to post-mortem examinations, the result would not be very much to their credit. Six weeks before this death happened deceased had cramp, as it was called, in the stomach, which produced those spasms the medical men said were the result of wind and flatulency. But what produced wind and flatulency? If indigestible food were allowed to remain on the stomach, fermentation ensued, and impure gases arose, and the stomach became subject to spasms or cramp, and that was not unfrequently followed by death. He took it on him to say, that on a perfect stomach wind never existed. Deceased had an imperfect stomach, and he said that his food

* It is making "a rapid progress," but not in the sense implied by the learned Serjeant. The "progress" is in the hands of quacks, to wit in reducing the number of the population.—Ed. Gaz.

putrescent; and hence the comparative sudden death of this man. A man's food would often be his poison if his digestive organs were not in a proper and healthy condition. A lobster would poison a man—not that the lobster itself was poison, but that, remaining on the stomach undigested, it would in a short time be productive of all the consequences of poison, from the *supere and putrescent state to which it must become reduced*. They had many instances on record of persons dying from the effects of constipation, produced by the overloading of the stomach. And what evidence was there, from the beginning to the end of this case, to make them believe that the death of this man did not ensue from a similar cause? He asked Mr. Wilson why he did not bring some lobelia here—nay, why did he not bring some of that vegetable powder found in deceased's stomach? "It was not necessary." Not necessary! Why, the jury were called upon to decide a matter materially affecting the character and happiness of a fellow-creature—a man accused of felony, and yet Mr. Wilson did not think it worth while! Did he think the jury were going to take for granted all he said? They had no respect of persons; they sought to get at the truth; and it was necessary they should have all that could assist them in getting at the truth. He asked again, what evidence was there that, except upon one occasion, the prisoner ever administered lobelia to the deceased at all? The only evidence they had was, that he gave two doses on Wednesday night within a quarter of an hour of each other; there was not a tittle of evidence that he gave any after. How much was given? One grain, two grains, three grains?—a spoonful, or a drachm? They did not know; there was no evidence. He asked one of the medical men this: "Suppose you had come to this patient, and found him labouring under the influence of lobelia, what would you have done?" He said—and he prayed the jury's attention to this—"I would have given him an emetic if I had thought it had been too large a dose." Clearly he did not think he had had too large a dose of lobelia, because he did not give him an emetic. "But if you had given him an emetic, what would it have been for?" "Why, to make him sick." "What would have been the effect of making him sick?" "To throw up the contents of the stomach." Ah! but those people said there was no occasion for that, because the man had been sick for twenty-four hours. If, then, no lobelia had been given to this man for twelve hours before his death, and during that time he was constantly sick and throwing off the con-

tents of his stomach, how happened it that any lobelia was afterwards found in his stomach at all? But he took a basin of cold sour small beer that morning. The jury all looked like healthy men; but would any one tell him whether he would venture to take a basin of cold sour small beer at five o'clock in the morning, or, if he did, whether he would not expect spasms, cramp in the stomach, and other violent symptoms that showed themselves in the deceased's case? Again, he reminded them that there was no evidence to show that after Wednesday night deceased took one grain of lobelia into his stomach, and none to show how much he took on Wednesday night, or whether it was a dangerous dose; besides, not one particle of what he did take would be left in his stomach if vomiting produced the effect that the medical gentleman said it did. What did they find in the stomach? "Some vegetable matter." "Was it poisonous?" "They did not know." Potatoes, greens, onions, were vegetables; they could not tell whether it was any of these. All they knew was, it was "vegetable matter," and that under it, in some parts of the stomach, were signs of irritation. Whether it was lobelia, or whether it was decomposed vegetable matter, lodged in the stomach from imperfect digestion, none could tell. (?) Again, what was deceased's treatment? What was his complaint? Spasms. What was lobelia? An anti-spasmodic. What besides? He complained of heartburn; that came from indigestion, from the stomach being oppressed and loaded. Common sense taught them that the remedy for that was to unload the stomach. The doctors admitted that lobelia was an emetic, and given in America in large doses by medical men. *Why, then, the thing to be done was the very thing that was done in this case.* Another circumstance: the gall-bladder contained, when the body was opened, a quantity of thick black bile. He asked Mr. Wilson—"How long, in your opinion, was that bile undisturbed?" "Eighteen hours," was his reply. "How long, in your opinion?" he asked another young gentleman. "Three or four days." He took it upon him to say the latter was more correct. Well, then, if, after all their remedies, their calomel, their injections, their hot applications, the gall-bladder was so inert, what stronger proof could he have of an impaired digestion and want of power of relieving the body of its feculent contents? What was the use of the gall? To promote, by lubrication, the motion of the intestines. If a man had no motion for twenty-four hours, or more, could they wonder at pain, or even spasms, ensuing? And could they wonder that much reliance

could not be placed upon gentlemen who differed so much, one saying eighteen hours, and another three or four days, by those who had been behind the scenes, and knew something of the profession itself? One medical gentleman told them that, with the exception of purging, deceased exhibited all the symptoms of cholera. Why, *the absence of purging might have produced all the symptoms seen.* Let them look at it mechanically. After mastication, the food passed into the stomach; the contents of the stomach must have free access to the bowels; if in the bowels stoppage takes place, a check ensues on the stomach, the bowels act upon it sympathetically, and produce nausea and increased disease; and if the stomach be allowed to retain its contents, a sickly smell will ensue. If the jury had ever laboured under indigestion, they must remember how their mouths tasted, and how the edges of their lips sometimes grew sore. The mouth itself sometimes became ulcerated from the same cause. They had seen how children frequently suffered from their foolish parents giving them improper food. Men were but "children of a larger growth," if they indulged themselves in the same way, the same effects would be produced.

He had, he thought, now gone over all the facts of the case: he came now to the law. The opinion of one learned judge had been quoted: he would quote another. In the case of the Queen v. Winterbottom, at last Liverpool assizes, before Baron Alderson, that learned judge said, "*The question for the jury to consider was, whether the prisoner had done what he considered most advantageous for the recovery of the patient? if he had done so, he ought not to be declared guilty of manslaughter.*" Unless the prosecutor (continued his lordship) made it plain that the prisoner was *not doing his best*, there was an end of the case: and if people employed unqualified persons to dispense medicines, they themselves must take the consequences." Now that, he (Sergeant Wilkins) submitted, was *good law*. There was no law to prevent any one practising; and this case was not, as his learned friend had suggested, one in which medical attendance was at hand, and the prisoner obtruded himself on the patient, and so excluded professional attendance, but one in which the prisoner had been invited by both the deceased and his father. Would any one doubt that the young man did what he conceived to be right and best under the circumstances, and that the result came not *through any gross negligence or want of proper care on his part*? He was a friend of the deceased, and had every desire to save him. He stood high in his position of life,

having been *five years* in his present employ, with a character unimpaired and unimpeachable, and he (Mr. Sergeant Wilkins) did hope and trust that they would feel themselves justified in saying that there was *so much* doubt and difficulty in this case, and the possibilities and probabilities, least in so many directions, that they could not convict the accused as conscientious men, having a regard to their own interest, and that of others.

His LORDSHIP then charged the jury. The prisoner, he said, was accused of manslaughter, by having administered to the deceased a certain quantity of a noxious drug called lobelia, by the effects of which death was said to have ensued. Now, whatever might be the result of this inquiry, he felt himself called upon, in justice to the prisoner, to say that there was no imputation whatever on his motives. His feelings towards the deceased seemed to have been of the very best and most friendly kind; he acted quite gratuitously, and with a view, as he said, to do him good. So far, therefore, *there was no imputation whatever on the prisoner's motives.* But the law, as he understood it, was this, that if an ignorant and unskilful person undertook *voluntarily, and without necessity,* where competent medical assistance could be procured, to administer drugs of a *poisonous or dangerous nature*, and which required the greatest caution and skill in their use, and the patient should die in consequence of the *ignorant and unskilful* use of such drug, the party who administered that drug did so at his own risk, and would be guilty of manslaughter, notwithstanding that he administered it, believing that it would turn out to be beneficial. Now, in this case a great question for their consideration was, whether they were perfectly satisfied that the deceased died of lobelia? because if they were not satisfied of that, the rest of the case need not be considered, the charge being that he died of that noxious drug as administered by the prisoner. Should they be of opinion that he did die of lobelia, the question would then be whether the prisoner at the bar, in administering that lobelia, administered it, under the circumstances, ignorantly, presumptuously, unskilfully, and without that due care and caution which every one was required to act with who professed to deal with such drugs. Now, this drug called lobelia appeared to be but little known in England until lately—in deed, it was not very much known now. It was said to be extensively used in America, and in larger doses than in England. It was in use in England by regular medical practitioners as an anti-spasmodic, and also as an emetic, and its qualities

were such as had induced the scientific world to class it among what were called the narcotico-irritant poisons. When administered in large doses, and under improper circumstances, the effects of these drugs were such as many persons were unable to endure. Many drugs, even in the most common use, were, when given in large doses, and under improper circumstances, most deadly poisons. Hence the skill and care required in administering them. Now, the prisoner at the bar did not profess the practice of medicine. He was employed in a foundry, and was a fellow-workman with deceased. On the whole, the judge seemed favourably impressed towards the prisoner, and concluded by again putting the question before the jury:—*viz.* whether they were satisfied, upon the evidence, that the prisoner died from natural causes, as *indigestion*, however produced, or by the undue, improper, careless, and ignorant administration to him of lobelia by the prisoner? If satisfied that he died from the latter cause, they would find him guilty; but if they were satisfied he died from natural causes, they would acquit him.

The jury then retired, and returned in about ten minutes with a verdict of *Not Guilty*. On the foreman pronouncing the verdict, a murmur of satisfaction arose in the court, which the court officer immediately suppressed.

On discharging the prisoner, the judge remarked that whatever might have been the result in this instance, the motives of the prisoner had been perfectly good, though it was to be hoped that all parties of insufficient education and skill would for the future be cautious how they dealt with such drugs as this.

Correspondence.

ON THE NATURAL PERIOD OF UTERO-GESTATION.

SIR,—On Jan. 25 you published my report of a case in which the secretion of milk had occurred at an unusually early period of pregnancy. The sequel of this case may not prove uninteresting, throwing some light, as it seems to me, on questions not altogether resolved as yet, if indeed they are capable of solution by our limited reasoning powers.

The subject of the report alluded to was after a very short and easy labour delivered of twins on the 8th day of May, 1850. The children were and still are both fine and healthy infants.

Now supposing that conception did really take place on the 12th of August, 1849,—and I need not enter into my reasons for accrediting M. F.'s statements, which are detailed in my former communication,—it is worthy of remark that the period of utero-gestation exactly corresponded with that which is now generally looked on as its average duration,—i. e. nine lunar months of 30 days each, or 270 days, including, as is usual, the first and last days of pregnancy, or those of conception and delivery.

Again, when a healthy young woman is, in her first confinement, delivered after a natural labour, it is not unfair to argue that the duration of her pregnancy was of average length; the less so when it is remembered that the time when quickening took place is in a most unsuspecting manner fixed for a period anterior to delivery by 22 weeks and a half, "*Quam proxime*" (vide No. for Jan. 25, 1850, p. 145), which, with 16 weeks, the interval from conception to quickening, generally allowed as a fair average, make 38 weeks and 4 days, or the above average duration of natural pregnancy. Thus I conceive the chain of reasoning becomes the more strengthened by the mutual fitness of its links to each other; and what, on the 27th of Dec. 1849, was merely a very probable assumption, may now be looked on as an established fact; and we may give full credence to her statement, that on Sunday, the 12th of August, 1849, she being in expectation of the catamenial flux on that day, allowed the approaches of her lover, not absolutely for the first time, but for the first time since many months; *that coitus*, notwithstanding the menses appeared and lasted their usual time (with her) till the 15th or Lady-day, which, being a great holiday, served with her as an epoch from which to count. *En passant*, it may be remarked that this gains additional probability from the fact that, while she is a Roman Catholic, her seducer is a Protestant, and thus Sunday was the only day on which they were both likely to be at liberty in the early part of the day, when intercourse is stated to have occurred. That a day or two before the 1st of December the train of symptoms usually existing before the uterus rises out of the pelvis, had, in a rather sudden manner, been considerably relieved, and in fine, that on the 8th of January, 1850, she actually was, as every thing considered she was pronounced to be, in the latter end of the 21st week of pregnancy. It now remains to be considered whether any additional evidence can be offered in support of the fluid secreted by the mammary glands at this early period and in abundance, being *bonâ fide* milk.

Upon this I may remark, that having

since that time become much more familiar with the use of the microscope, and having examined the milk of women nursing, and at very different periods after delivery, I am prepared to assert that it was not only real milk, but a milk very rich in oil globules; in fact, a very creamy milk.

The only circumstance that seemed to throw a doubt on its real nature was the fact of its showing an acid reaction so soon after it was taken from the breast. On this point I have to remark that, shortly after the date of that observation, and in consequence of my attention being drawn to the subject, I met with a case in which, in a remarkably healthy woman, and a fine nurse, the milk was actually and energetically acid at the breast, reddening litmus paper instantly on being milked immediately on it. This woman had been nursing for the long period of two years and about six weeks; still both she and her nursing were in good health. I have lately met with a case in which, in a young and healthy woman (a well-fed person too) in whom, seven days after her second confinement, the milk at the breast was scarcely alkaline (this so marked in my note-book), in three-quarters of an hour it was neutral; and in two hours after it was drawn it was faintly acid. This was a very rich creamy milk, and under the microscope reminded me strongly of that of M. F., which was the first human milk I had ever examined in this way: indeed, I am inclined to think that further observation may furnish some not merely solitary exceptions to the generally received maxim that human milk is, when freshly taken, always alkaline.

You will use your own discretion in inserting or not the above observations, and believe me to be,

Very faithfully yours,
J. I. TRAYER.

Dunow, Queen's County,
August 16, 1860.

Medical Intelligence.

THE NATIONAL INSTITUTE AND THE SOCIETY OF APOTHECARIES.

THE Committee of the Council of the National Institute have agreed to the following resolution:—"1. That this Committee, having considered the propositions contained in the letter from the Society of Apothecaries to the Right Honourable Sir George Grey, Bart., for altering the Apothecaries' Act of 1845, hereby declare, that

they cannot support any scheme for regulating the education and conducting the examinations of the general practitioners in medicine, surgery, and midwifery, which is incomplete, or which does not embrace all the branches of education and practice necessary for the efficient performance of the duties of general practitioners. That, in particular, the Committee repudiate the remodelling of either of the existing institutions (as they have, hitherto, the establishment of any new institution) that is not based upon the unity of medicine and surgery, and which, in the education and examination for admission into such institution (which is also intended to confer a title to practice) does not embrace all the branches both of medicine and surgery. 2. That the want of proper authority to examine in surgery has, at all times since the Act of 1816, operated prejudicially upon the examinations instituted by the Society of Apothecaries; and that this Committee unequivocally condemns any proposition to alter that Act which does not embrace an amendment in this essential point. That the unity of medicine and surgery has been the fundamental principle of the National Association and of the National Institute, and has been consistently adhered to throughout; and that, without questioning the right of the existing institutions to retain their special character, the Council resolves to adhere to this principle in reference to the education and qualification of the general practitioners as essential to any measure of medical reform."

FEMALE DOCTORS IN THE UNITED STATES.

WITH the exception of Madame Restall, which is the *nomme de guerre* of a notorious abortionist, and one or more of the sex who have sought the "bad eminence" of being her imitators at the risk of sharing her infamy in the Penitentiary on Blackwell's Island, we have heard but little of female physicians in New York until recently. A more respectable instance is now announced in the case of a lady who has been before the public as a lecturer upon anatomy, physiology, &c., for some years, who has come out in favour of the water cure, and has recently taken to herself a husband, and they twain are conducting a hydropathic establishment, which, under their joint direction, is prepared to pack ladies and gentlemen in wet sheets or blankets, give them the cold or hot douche, sitz bath, &c.; pledging themselves to use no drugs except in the microscopic quantities which are found in the unfiltered croton. We should not marvel if this should turn out to be a popular move in the right direction, seeing that "there is a lady in the case."—*New York Med. Gaz.*

ON THE MORTALITY PRODUCED BY CHOLERA IN THE INDIAN ARMIES. ACCORDING to Dr. Finch, during the prevalence of cholera in India in the year

1847, our armies did not suffer so severely as it is commonly supposed. This is shown by the following table :—

	Number of men.	Attacked by cholera per cent.	Deaths from cholera per cent.
Madras Presidency.	11,429	0.271 (1 man in 500)	0.192 (less than 1 in 500)
Bombay Presidency.	8,756	0.515 (1 man in 200)	0.274 (1 man in 400)

The Native Army appears to be attacked in nearly the same proportion :—

Madras Native Army.	67,850	0.384 (1 man in 300)	0.114 (1 man in 900)
Bombay Native Army.	42,980	0.575 (1 man in 200)	0.227 (1 man in 400)

These results show that the risk of life from the endemic cholera of India is not so great as it is commonly believed to be.

THE CHOLERA AT TUNIS.

LETTERS from Tunis announce that the cholera had begun to cease its ravages ; for the number of deaths, which on the 16th ult. were forty-eight, had fallen to four on the 1st inst.

LOW TEMPERATURE SUSTAINED BY HUMAN BEINGS.

It is stated by Mr. Rae, in his Narrative of the Arctic Expedition, 1846-7, that at Fort Hope, in latitude $66^{\circ} 32'$, and longitude $86^{\circ} 55'$, the lowest temperature experienced during the winter was -47° . This is equal to 79° below the freezing point of water, and 7° below the freezing point of mercury.

A SNAKE STORY—EXTRAORDINARY OPERATION.

THE astonishment of the community has been excited by the report of a most wonderful and extraordinary operation having been performed by some two or three *medical gentlemen* of this city, upon a woman labouring under some supposed obscure abdominal disease. The report has it, that two serpents, or eels, were taken from the stomach or a sac in the peritoneal cavity, and they are actually exhibited *without charge* (preserved in spirits) to any one wishing to see them, at an apothecary store in the vicinity of Broad Street. We understand that *thousands* have already availed themselves of the privilege, many going away satisfied that they have seen the *elephant*, while others are willing to pay the most devoted reverence at the shrine of surgical art. The bold operators are being emphatically lionized ; and certainly nothing could have been more auspicious for an aspirant for medical fame and practice, than the occurrence of such a wonderful phenomenon. We are inclined thus much

to discourse upon the subject by evidence from a reliable source that certain individuals had seen the doctor who performed the operation, and that the doctor, in answer to the many inquiries put to him about the diagnosis and operation, distinctly told the crowd that, by the use of the stethoscope and speculum, and about twenty other instruments, the thing was accomplished. But the richest part of the story is, that the operation lasted one hour and a quarter, on account, it is said, of one of the monsters having his tail coiled around the liver, and its being extricated with much difficulty !—*Boston Medical Journal*.

FLUORINE IN BLOOD AND MILK.

DR. G. WILSON was the first to announce that fluoride of calcium was soluble in water, and that it existed in many natural waters. He states that 16 fluid-ounces, or 7000 grains, of water at 60° hold dissolved 0.28 grs. of fluor spar. This is in the very large proportion of two and a half grains to the imperial gallon : hence, if there be no mistake in the results, it follows that some natural waters contain more fluoride of calcium than sulphate of lime !

In pursuing his investigations, Dr. G. Wilson has recently examined the fresh-drawn blood of the ox ; and in the experiment he employed 26 imperial pints, or $3\frac{1}{2}$ gallons of blood. The mode in which the analysis was performed is not described ; but the analyst states that the evidence in favour of the presence of fluorine in the blood of the ox seems unexceptionable : and it cannot be doubted that the blood of other animals will be found to contain the same element. The fluorine was supposed to be in the state of fluoride of calcium ; but the amount was so small that no quantitative estimate was made.

Nine imperial pints (rather more than a gallon) of rich milk were examined, and the vapour which they evolved etched glass distinctly. The ashes of twelve pounds of new skim-milk cheese, treated in the same way, occasioned deep etching of glass. Dr. W. thinks that, as in other cases, the fluoride of calcium is associated with phosphate of lime, and that when milk is coagulated it separates with the caseine.

* * Hitherto chemists have found fluoride of calcium as a constituent of the body in the teeth, bones, and, according to some, in the urine. Its presence in the teeth rests upon the authority of Berzelius. Dr. Wilson admits that, notwithstanding the enormous quantities of blood and milk used by him, the proofs of the presence of fluoride of calcium were not so decisive as he could have wished. We think further experiments are required before the fluoride can be considered as a constituent of these animal liquids. It is a remarkable fact that the fluoride of calcium is generally associated with phosphate of lime in the mineral kingdom: and while barely a trace of fluoride is found in recent bones, it is very abundant as a constituent of the fossilized bones of extinct animals. Is there any conversion of phosphate to fluoride of lime yet unrecognized by chemists?

PHOSPHORESCENCE OF POTASSIUM.

At the recent meeting of the British Association, it was stated by Mr. Petrie, that when cold potassium was cut in the dark, he found the two cut surfaces to be distinctly luminous, although the luminosity was much less than that of phosphorus under the same circumstances. This evolution of light probably arises from rapid oxidation. So soon as the metallic surface is covered with a layer of oxide of potassium the phenomena of phosphorescence ceases.

QUACKERY IN AND OUT OF THE PROFESSION.

We quote the following from our new transatlantic contemporary, the *New York Medical Gazette*. It is too much the custom to apply the term quack to those only who happen to have no license or diploma; but this is a perversion of the real meaning of the word.

"The term quack is properly applied to an ignorant or unprincipled practitioner of the healing art, and to such only. He who dares to give remedies, of which he knows little, in diseases of which he knows less, and applies them to living beings, of whose structure or functions he knows nothing, is demonstrably a quack by reason of his ignorance and temerity; while he who imposes upon the public by seeming to be what he is not, and who, for the sake of

filthy lucre, sacrifices principle, honesty, and conscience, in the sale of nostrums or specifics, promising to cure incurable diseases, and exacting extortionate fees from the afflicted, by taking advantage of their ignorance or credulity,—is a quack because he is unprincipled. Such knaves abound in the profession, and they are such whether with or without diplomas, and to whatever school they belong; they are all alike,—whether called allopathists, homoeopaths, or chrono-thermalists; and whether they employ mineral, botanical, electrical, galvanic, magnetic, or metaphysical remedies. Their ignorance and unprincipled conduct constitute them quacks; and to protect the public from being plundered and victimised by such, is the only and all-sufficient reason for the warnings against quackery which the regular profession are ever reiterating through the public press. That these warnings are to a great extent unheeded, is the chief cause of the great mortality, especially in the large cities, where ignorant and unprincipled physicians abound, and where they find their most numerous victims."

REPORT OF THE MEDICAL BENEVOLENT FUND FOR THE YEAR 1849-50.

REMARKABLE as was the history of the last year in the development of their resources, and in the increased demand upon those resources, yet the Committee observe, that the year just concluded has in both instances exceeded its predecessor; for the amount received in subscriptions for the last year has been £490. 17s. 3d., and the sum granted in benevolent aid has been £439, distributed among forty-five cases of distress.

It will be seen that the former has exceeded the amount of last year's subscriptions by £74. 2s. 1d. notwithstanding that death has been unusually rife among the subscribers;—and that this fact, added to the pressure of the times upon professional income, has occasioned many subscriptions to be lost to the fund; and, notwithstanding this other fact, that some subscriptions have been this year omitted, in consequence of the parties having given donations to the special fund,—a consequence which your Committee had last year foreseen and deprecated. Your Committee are pleased to record, that these instances have been few; but, although few, they have had a certain influence upon the amount of your income. It is with real pleasure, however, they announce the accession of a considerable number of new subscribers, and the augmented subscriptions of many old ones.

Your Committee beg to call your attention to the circumstance, that while the number of cases relieved this year has ex-

ceeded that of the former year in the proportion of forty-five to thirty-four, the sum expended in benevolent aid has been less by £15. 4s. This has not arisen from the cases being less pressing or urgent, but from the smallness of their resources: they have been often obliged to vote £5, when their judgment and their feelings would have dictated a much larger sum; but under their inability to give more, they rejoice to say that their grants have been most thankfully received, and have been productive of the most signal benefit; in no preceding year has the amount of distress relieved been so great.

There have been cases which your Committee have been compelled to dismiss without relief, upon their own demerits; but there have been others which they have been obliged to dismiss without aid, simply because they had no funds from which to give. They also beg to state that they have experienced considerable embarrassment in the past year, from the irregular or tardy payment of subscriptions; and that had it not been for the kindness of their Treasurer, many of the cases must have been postponed at a time when present aid was of the utmost importance. They therefore beg to impress upon the subscribers generally, and upon their local Secretaries particularly, the great value of regularity of payment at the commencement of each half-year.

Your Committee took occasion, in their last Report, to urge upon you the importance of special donations, in order to bring the Donation Fund into action. An appeal upon this subject was circulated, and has been responded to in a way which has exceeded their most sanguine expectations. Your Committee had certainly hoped that they should by this measure bring up the Donation Fund to the sum required by your constitution—viz., £3,000; and they have much pleasure in informing you that it has reached £2,179. 10s. 6d.

Your Committee deeply feel that each one of their generous donors is entitled to their best thanks for the aid thus afforded; and they venture to hope that in the majority of instances this countenance may be continued in the shape of some annual contribution: the pressing demands for more income to their subscription fund will be seen by a consideration of the cases appended to their printed statement; and it is only by enlarged annual subscriptions that the prosperity of your institution can be secured and increased.

Your Committee congratulate you upon the origin, during the past year, of a Provident Institution for medical men and their families, upon just and safe principles: and to those who possess the means,

they earnestly recommend this method of providing against old age and infirmity, as well as for widows and orphans, and other calculable contingencies. A benevolent branch is attached to this fund for the relief of its members, who may fall into difficulties,—a provision which can in no way interfere with your fund, though in process of time it may serve to diminish the number of dependants upon it,—a consumption most devoutly to be wished, when it is considered how great are the demands upon your resources,—how inadequate are your resources to those demands,—and the high probability, that as your purely charitable agency is more widely known, a greater amount of professional misery will be detected. Nor can it be hoped, that even in the event of the new provident fund being eminently successful, it can ever supersede, or indeed materially diminish the necessity for your own simply benevolent institution. Your Committee, therefore, earnestly entreat you to strengthen their hands, and to extend the influence of the only purely charitable fund in England.

Your Committee have the pleasure to inform you that steps have been taken to bring their donation fund into action on the first of November next; though prudence has compelled them to suspend for the present, until more prosperous days, the granting loans to medical men for a given time without interest.

Your Committee beg your attention to their financial statement, by which it will be seen that their balance in hand is reduced to £33. 1s. 1d. and it is only by renewed efforts, especially to promote subscriptions, that they can in any way hope to relieve those who are now waiting for assistance, and to meet the misery which is so continually brought before their notice.

Your Committee beg your attention to some partial changes which they have felt called upon to make, and which they trust you will sanction with your approbation.

In consequence of some persons mistaking the object of your fund, and believing that its operation was confined to the provinces, your Committee have deemed it necessary to declare its purely catholic nature—and to designate it, as indeed it is in truth, "The Medical Benevolent Fund."

And in consequence of the illness of Dr. Baron, the removal of Dr. Conolly, and from the circumstances of Cheltenham, as well as of the increasing importance of the metropolis, both in the amount of subscriptions, and the number of cases relieved,—it has been thought advisable to remove the meetings of the Committee to London, and to associate with the old members fifteen gentlemen chosen from subscribers in the metropolis. They are

happy to inform you that Dr. Baron, as President, will continue to take the same interest in the welfare of the Society which he has felt from its foundation; and that Dr. William Conolly will also still act as Secretary to the Committee, and correspond, as he has previously done, with the Treasurer, Mr. Newnham.

Your Committee have farther to report, that they have maturely considered various propositions which have been placed before them for the extension of their body, in order to give a broader hold upon the affections of the many for their fund; and to interest a greater number of individuals in the cases to be relieved; and they have resolved upon giving a seat at the Committee-table, with the privilege of voting, to the President of the Council, the President of the Association, the President of each Branch of the Association, for the time being, and to each Local Secretary of the Benevolent Fund; thus securing that representative system in their operations which has been, by some, thought so desirable.

Your Committee assure you, that they will ever give their best attention to every proposition which may be made to them for the improvement of the fund, though they must reserve to themselves the right of their own unbiassed judgment; and will ever unflinchingly act upon their convictions, uninfluenced by the smiles of approbation or the frowns of displeasure.

A comparative view of the progress of your Institution has been also this year added to this report, as well as a separate list of all donors of £5 and upwards, *previously to the 1st of July, 1847.*

Your Committee would not quit this part of their report without earnestly recommending to your notice and kind feelings, the adoption of an after-dinner collection, at each one of your anniversary dinners, and also of your branches; thus assimilating yourselves with almost all the leading charities of the day, and securing aid for the relief of those who have no means of festive enjoyment.

Your Committee beg to express their deep sense of gratitude to their non-professional donors and subscribers; and to breathe their earnest hope that their contributions may be twice blessed.

Also your Committee are anxious thus publicly to record their thankfulness to the "Medical and Legal," the "Eagle and Protector," and the "Crown" Life Assurance Offices, for their generous donations towards their common object.

Again your Committee desire emphatically to offer the tribute of their thanks to the Editors of the *Lancet*, the *Medical Times*, the *Provincial Medical and Sur-*

gical Journal, and the *Medical Directory*,* for their respective kindness in assisting the extension of the knowledge of your Institution.

Your Committee have long felt the importance of possessing a London banker to whom, and through whom, subscriptions might be paid; and they have much pleasure in informing you, that this advantage has been obtained in the firm of *Herries, Farquhar, and Co.*

Your Committee are not aware that further motives can be required for activity and energy in a cause so pure, so benevolent, so heavenly. But they are unwilling to take leave of you for another year, without humbly avowing their deep sense of gratitude to the Giver of all good, for the measure of success which has attended their efforts; and without inscribing upon their pages, the words of the great Author of Benevolence, "Inasmuch as ye have done it unto one of the least of these, my brethren, ye have done it unto me."

JOHN BARON, M.D. F.R.S.,
President.

W. NEWNHAM,
Treasurer and Hon. Sec.

DEGREES GRANTED BY THE UNIVERSITY OF GLASGOW.

THE Senate of the University of Glasgow, at their meeting on the 7th inst., conferred the Degree of M.D. on the following gentlemen:—John Coombe Maddever, England; George Lade, Scotland; George M'Gillivray, Scotland; John Grieve, Scotland; James Brash, Scotland; James M'Collum, Ireland; James March Craig, Scotland; and the Degree of C.M. on Dr. John Coombe Maddever.

APOTHECARIES' HALL.

NAMES of gentlemen who passed their examination in the science and practice of medicine, and received certificates to practise, on Thursday, 22d August, 1850:—John Rigby, Preston, Lancashire—Charles Anderson Dalgairns, Guernsey—Claudius Galen Wheelhouse, Leeds—James Hurdie Black, Dysart, Fife, N.B.

OBITUARY.

On the 16th inst., at his residence, 2, Priory Parade, Chesham, James Williamson, Esq., M.D., late H. E. I. O. Bengal Service, in the 82d year of his age.

* We shall be glad if Dr. Baron or Mr. Newnham will inform us why the *London Medical Gazette* has been excluded from this list. The interests of the Benevolent Fund have always been advocated in our pages.

CASE OF MELANOSIS OF THE EYE.

BY DR. BETHUNE.

THE patient, a healthy farmer, sixty-five of age, entered the Eye and Ear Infirmary under his care. Twenty years ago, he first observed a red spot at the outer angle of the left eye, wedge-shaped, and with the apex towards the pupil, as in pterygium. For fifteen years it was stationary; but five years ago it began to grow, and at the end of one year he was only able to discern the light. Pain came on, when the disease began to increase, and was severe for the first two years; it was then less again till last autumn, since which time it has increased, being occasionally severe and darting, and at times dull and heavy.

On examination, the right eye is well. Lid of left eye separated by a black, smooth, but irregular mass, projecting from the anterior third of the ball, and compared to a peach-nut with the base outward and the anterior half cut off. A few days after his admission the eye was removed, and at the end of a week the parts were healing well, and he was discharged. The eye seems to be healthy, except for the tumor, which does not invade the internal parts.

Under the microscope, Dr. H. J. Bigelow had observed the following appearances: "First, numerous cells, apparently epithelial; secondly, numerous cells, of irregular outline, enlarged by a power of five hundred diameters to the size of a five cent piece, and containing sub-cells and nuclei; also some of a marked caudate figure. These, with others decreasing to simple nucleated cells, with one or two nuclei, the diameter of which cells was only three or four times that of blood corpuscles, were probably cancerous. And to account for the black colour the whole field was filled with granules, often aggregated into masses resembling Gluge's granulation cells; which last, however, may have been the product of independent inflammation."—*American Journal of the Medical Sciences*, July 1860.

BOOKS & PERIODICALS RECEIVED FOR REVIEW.

(The List will be given in our next No.)

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer 29.68
 " " " Thermometer 55°
 Self-registering do. Max. 82° Min. 33°

• From 13 observations daily. • Sun.

RAIN, in inches, 0.67. — Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was 4° below the mean of the month.

BIRTHS & DEATHS IN THE METROPOLIS During the Week ending Saturday, Aug. 22.

BIRTHS.		DEATHS.	
Males....	733	Males....	464
Females..	683	Females..	441
1416		905	

CAUSES OF DEATH.

ALL CAUSES	905
SPECIFIED CAUSES	904
1. Zymotic (or Epidemic, Endemic, Contagious) Diseases....	239
Sporadic Diseases, viz.—	
1. Dropsy, Cancer, &c.	45
2. Brain, Spinal Marrow, Nerves, and Senses	165
4. Heart and Bloodvessels	94
5. Lungs and organs of Respiration ..	64
6. Stomach, Liver, &c.	63
7. Diseases of the Kidneys, &c.	17
8. Childbirth, Diseases of Uterus, &c. ..	12
9. Rheumatism, Diseases of Bones, Joints, &c.	5
10. Skin	1
11. Old Age	48
12. Sudden Deaths	9
13. Violence, Privation, Cold, &c....	34

The following is a selection of the numbers of Deaths from the most important special causes:

Small-pox.....	8	Convulsions.....	29
Measles.....	11	Bronchitis	29
Scarlatina.....	19	Pneumonia	29
Hoooping-cough	25	Phthisis	117
Diarrhoea.....	118	Lungs	7
Cholera.....	5	Teething	8
Typhus.....	33	Stomach	5
Dropsy.....	13	Liver	19
Hydrocephalus	24	Childbirth	12
Apoplexy.....	25	Uterus	1
Paralysis.....	18		

REMARKS.—The total number of deaths was 139 below the average mortality of the 24th week of ten previous years.

NOTICES TO CORRESPONDENTS.

Mr. Herbert Williams.—The report has been received: it shall have our immediate attention.

Mr. Hunter's additional paper has come to hand, but too late for the proposed addition.

The letter of Mr. Henry Smith, the Hospital report of Dr. Barclay, and the Memorial of the Manchester Ethical Association, will be published in the next number.

Communications have been received from Mr. D. (I. Edwards—Dr. Nevins—and Mr. W. Smith, of Belper. To the latter, a private note will be sent.

RECEIVED.—The Dorset County Chronicle.

Mr. R. J. A., Neath.—Dr. Canning, of Edinburgh, has written to inform us, in answer to the question addressed to him by this correspondent, that, in the cases referred to, the tar is usually administered in the form of Capsule (similar to the Carter's Oil or Cod-liver Capsules), one thrice a-day on the average. The Capsules are generally sold by the box, and can easily be procured either in London or Edinburgh.

Not unfrequently an eruption, like that of scarlet fever, follows their use. I always inform the patients of this, as they are apt to become alarmed by its appearance.

ERRATA.—In Mr. Bate's Notes on the Teeth, in the last number, page 328, under Fig. 2, for "tubular structure of loop," read "tubular structure of dentine."—P. 330, under Fig. 4, for "malbecius," read "merluccius."—P. 332, col. 2, line 25, for "is," read "are;" and l. 40, for "formed," read "found."—Page 333, l. 1 (of text), for "soda," read "lime."

Lecturers.

COURSE OF LECTURES

ON

DISEASES OF THE HEART.

Delivered at St. Vincent's Hospital during the Session 1849-50.

By O'BRYEN BELLINGHAM, M.D.

Fellow of, and Member of the Court of Examiners of, the Royal College of Surgeons in Ireland, and one of the Medical Officers of the Hospital.

LECTURE XI.

EXAMINATION OF THE HEART IN DISEASE, CONTINUED.

Abnormal sounds developed during the heart's action—Sawing, filing, and grating valvular murmurs—Conclusions from the presence of a rough valvular murmur—Musical valvular murmurs—Arterial murmurs—Arterial bruit de soufflet—Rough or grating arterial murmurs—Venous murmurs—Continuous venous murmur—Musical venous murmur—Seat of the continuous and musical murmurs—Mechanism by which they are produced.

Sawing, Filing, and Grating Valvular murmurs.

THE valvular or endocardial murmurs next to be considered have a harsh, rough, or grating character: they were first described by Laennec, and have received various names: as the "sawing murmur" (bruit de scie); the "rasping or grating murmur" (bruit de râpe); and the "filing murmur" (bruit de lime); which more or less resemble these familiar sounds. Although the rough valvular murmurs are distinguished by different names, they are by no means to be regarded as characteristic of distinct forms of disease;—in fact, they are nothing more than degrees or varieties of rough sounds, having the same origin as bruit de soufflet, although less common than it, and more decidedly pathognomonic of organic disease of the valves or orifices of the heart.

It is a common opinion that the harsh grating valvular murmurs are the result of osseous or calcareous degeneration of the valve at which they have their seat, and that they always indicate a more advanced state of disease of the valves, or a greater amount of contraction of the orifice than bruit de soufflet. This was the opinion of

Laennec, and thus appears to be the view taken by M. Bouillaud; it is far, however, from being correct: these sounds are heard in cases where the valves have undergone neither calcareous or osseous degeneration; and a very advanced state of disease of a valve is often indicated simply by bruit de soufflet, while, in the most advanced stage of all, it may disappear, and no murmur of any kind be audible. This is sometimes witnessed at the mitral orifice, when the contraction becomes so extreme as not to permit a sufficient current of reflux blood through it to develop a murmur.

Although these harsh grating sounds do not necessarily indicate either osseous or cartilaginous degeneration of the valves, and are often not heard in cases where valvular disease is much advanced; yet, when they are well marked, we may conclude that the degree of friction between the blood and the parietes of the orifice at which they are developed, is greater than where simply bruit de soufflet is heard. In the majority of cases this is owing to hypertrophy, or hypertrophy with some dilatation of the ventricle complicating the valvular disease, by which the contractile power of the ventricle is increased, and the blood is transmitted with augmented force through the diseased orifice. Hence the rough valvular murmurs are limited in a great measure to the period of the ventricular systole and the first sound of the heart, and are not heard in inorganic affections of the organ, in which bruit de soufflet is so common.

In almost every instance the rough grating murmurs are preceded by bruit de soufflet; the one insensibly passes into the other; and the former are to be regarded as merely more intense degrees of the latter. For instance, when, owing to disease of the semilunar valves of the aorta, or of the aortic orifice itself, the passage outwards of the blood is obstructed, a bruit de soufflet will be developed: if the impediment increases, owing to increase of the obstruction,—and if, as usually occurs, the parietes of the left ventricle become hypertrophied,—the bruit de soufflet will pass into a sawing, filing, or rasping murmur. On the other hand, when the aortic valves merely permit regurgitation, no increase of the disease will convert the bruit de soufflet into a rough or grating murmur,—because the force with which the current of blood enters the ventricle is never sufficiently great to develop a harsher sound than bruit de soufflet. The rough grating valvular murmurs are consequently limited to the period of the ventricular systole; they replace only the first sound of the heart, and are, in a great measure, limited to disease of the aortic orifice or its valves.

Conclusions from the presence of a Rough Valvular Murmur.

1. The sawing, filing, grating and valvular murmurs are to be regarded as nothing more than bruit de soufflet exaggerated.
2. They are limited to the period of the first sound of the heart, and accompany only the systole of the left ventricle.
3. They are heard only in cases of organic disease of the valves or orifices.
4. The diseased state in which they are most generally audible is narrowing of the aortic orifices.
5. In almost every instance hypertrophy, with dilatation of the left ventricle, accompanies the valvular disease.
6. The sawing, filing, and grating valvular murmurs do not necessarily indicate either osseous, calcareous, or cartilaginous degeneration of the valves or orifices.

Musical Valvular Murmur.

The musical murmur of the heart—the "bruit de soufflet musical ou sibilant" of the French writers—is a very remarkable and rather rare sound: it resembles sometimes, as the name denotes, a whistle; sometimes the notes of a wind instrument; at others, the chirping of a bird, or the whining of a puppy; and at others, the cooing of a dove, or the mewing of a kitten. It may consist of only a single note, or of two or three; seldom more: it is sometimes so loud as to be audible without the stethoscope, and at some distance from the patient; and it may accompany either the systole or diastole of the ventricle.

We are indebted to Dr. Elliottson* for the earliest description of this murmur: he first called attention to it as a sign of valvular disease, and described the particular lesion of the valve in which he had met with it. Laennec says he never heard a musical murmur in the heart, but he has described a musical murmur of the arteries: the latter, however, we know now has its seat in the veins, not the arteries.

The musical valvular murmur is to be regarded as nothing more than a variety of bruit de soufflet, which almost always precedes it, and usually takes its place when it intermits or subsides, or when the exciting cause has been removed. In fact, as both Dr. Hope and M. Bouillaud remark, "there is no greater difference between the two sounds than there is between blowing with the lips, and whistling." Thus, in a case which was in hospital some time since, a loud musical murmur was heard upon the day of the patient's admission, after the exertion of walking to the hospital. On the day following, when the

circulation had become more tranquil, this murmur had disappeared, and was replaced by bruit de soufflet. On a post-mortem examination some time subsequently, the valvular lesion was found to consist in a cribriform condition of the curtains of the mitral valve, by which regurgitation had been permitted. In a case of acute endocarditis, which was in hospital a short time since, the bruit de soufflet was first heard; and when the patient had been submitted to treatment for a short time, this was replaced by a musical murmur, which continued to be audible as long as the patient remained under observation.

Like the sawing, rasping, filing, and other rough valvular murmurs, the musical murmur is almost always a sign of organic lesion of one of the valves or orifices of the left side of the heart; and, so far as I have had the opportunity of observing, it is almost limited to the regurgitant lesions of the aortic and mitral valves. This murmur is frequently accompanied by fremitus cataire, and may be audible in any case in which the semilunar or the mitral valves imperfectly fulfil their functions, provided the aperture through which regurgitation occurs is small, and that the heart acts vigorously.

As regurgitation cannot occur in a healthy condition of the valves, and as the musical murmur is in a great measure limited to cases of regurgitation, it is a valuable physical sign, when present, of valvular disease; and it may be regarded as almost pathognomonic, if combined with other symptoms of organic disease of the heart.

The division of abnormal sounds, which we have next to consider, have their seat in the arteries or in their veins, or in both. They all, with one exception, come under the head of what are called "*inorganic murmurs*." They are—

1. The arterial bruit de soufflet.
2. The rough grating arterial murmur.
3. The continuous venous murmur, or "bruit de diable."
4. The musical venous murmur.

Arterial Murmurs.

The abnormal sounds which have their seat in the arteries may, like those developed at the orifices of the heart, have either a blowing or a rough grating character. It is only in the large arteries that they are heard; and in investigating them it is always necessary to employ the stethoscope,—indeed, M. Vernois has proposed a modification of this instrument for the sole purpose of examining the arteries, which consists in "excavating the two opposite points of its circumference for the reception of the vessels subjected to examination," by which the pressure upon the artery

* Lathamian Lectures.

caused by the stethoscopes ordinarily employed will be obviated. This appears to be an unnecessary refinement, as the instruments in common use are quite sufficient for every purpose.

Arterial Bruit de Soufflet:

The abnormal sound heard most frequently in the arteries is bruit de soufflet. In a state of health, the blood, in its passage through the large arteries, causes such slight friction that very little sound is produced: this is short, single, and repeated at equal intervals, from sixty to eighty times in a minute, and is synchronous with the ventricular systole, and with the pulse. If slight pressure be made upon the vessel with the finger, or with the stethoscope, this sound increases in intensity; and, if the pressure be increased, it is converted into a murmur having the character of bruit de soufflet. Increase of friction between the blood and the lining membrane of a healthy vessel, produced by merely diminishing the channel through which the current flows, is therefore sufficient to produce a murmur in a large artery.

Again, one of the most important properties of the blood is its viscosity. In some morbid conditions of the system this property is impaired; the blood becomes impoverished, attenuated, and watery; the red globules diminish; serum superabounds; and, as a natural consequence, the viscosity of this fluid is less than natural. In other cases the actual amount of blood in the system is diminished, as after profuse hæmorrhage, or when venesection has been carried to an extreme degree. In both these cases, the amount of friction between the blood and the lining membrane of the arteries must necessarily be greater than natural; and this increased friction is capable of developing a murmur analogous to the bruit de soufflet produced by pressure upon a healthy vessel. In order that the murmur be well marked, it is necessary that a certain degree of velocity of the current above the natural standard should occur in addition.

The arteries in which bruit de soufflet is most commonly heard are the aorta, the carotids and subclavians, the iliac and femoral arteries. It may be heard in any one, or in all these vessels, when the blood is propelled with increased force and velocity through them; or when the calibre of the artery is diminished by pressure from without; or when the coats of the arteries have lost their natural and healthy state of tension; or when the blood is more or less attenuated, owing to its watery parts being increased, and the red globules diminished,—the result either of profuse hæmorrhage, or of a chlorotic state of the

system. The latter are frequently accompanied by the abnormal sounds (presently to be described) which have their seat in the veins; in these, also, the action of the heart is more rapid than natural; and the arterial bruit de soufflet is sometimes accompanied by a murmur at the aortic orifice, depending upon the same cause.

The bruit de soufflet, which has its seat in the large arteries, has been mistaken for a valvular murmur, particularly when it has its seat in the arch of the aorta, or in the carotid and subclavian arteries. It may almost always be distinguished,—

1. By its character, which, as Dr. Hope observes, is "usually a mere whiff."
2. By its never being heard at the period of the ventricular diastole, and the second sound of the heart.
3. By its being usually audible over several arteries at the same time.
4. By the facility with which the murmur is altered by varying the amount of pressure of the stethoscope, by which it is converted into a hissing, or harsh murmur.
5. By its not being constantly present: by its subsiding occasionally when the circulation becomes tranquil, and returning when palpitation ensues.
6. By the general signs of anæmia being present.
7. By the absence of the physical or general signs of organic disease of the heart.
8. By its entirely disappearing under treatment calculated to relieve the anæmic state of the system.

Rough or Grating Arterial Murmurs.

It is a very general opinion that bruit de soufflet is the only abnormal murmur developed in the large arteries. This, however, is far from being the case: a rough, harsh, or grating murmur, is not infrequent, but its seat is limited in a great measure to the arch of the aorta, and it is not confined to the period of the ventricular systole, but frequently accompanies the ventricular diastole likewise. It is probably owing to these circumstances that the phenomenon has been overlooked, the murmur, when heard, having been confounded with a cardiac murmur.

In a healthy state of the arterial system, the arteries, we know, are constantly full; when the amount of blood transmitted to them is increased, the artery dilates, in order to accommodate itself to the increased quantity; when the amount of blood is diminished, the arterial coats contract upon their contents, and the calibre of the artery is diminished in proportion; but, whether the amount of blood is greater or less, these vessels are always full. This depends upon the peculiar property possessed by arterial tissue, usually termed elasticity,

but which is evidently something more than mere elasticity, which is a property common to dead as well as living tissues. The aorta in a healthy state possesses this peculiar vital property in a high degree; which is due not only to its proper fibrous coat, but to its suberous or *sclerous* coat, which in this artery is very well developed, as described by Dr. Norman Chevers.*

It not unfrequently happens that the coats of the arch of the aorta suffer from disease, which impairs its elasticity; the tube becomes rigid, and its calibre cannot alter with the increased or diminished amount of blood transmitted to it. At the same time, the natural smoothness and polish which its lining membrane presents in the healthy state is diminished; or, the interior of the artery becomes rough and uneven from adventitious deposit; while very generally its calibre becomes somewhat increased.

During the systole of the left ventricle the arch of the aorta is necessarily more distended than during the ventricular diastole: in the morbid condition under consideration, however, as its coats are incapable of contracting upon their contents, and as the calibre of this part of the vessel remains the same at these different periods of the heart's action, a vacuum would occur here when the ventricular systole ceases, but that the blood regurgitates from the carotid and subclavian arteries to occupy it; and this can scarcely occur without producing sound, which will, of course, accompany the ventricular diastole, and the second sound of the heart.

In this diseased state of the arch of the aorta, not only is the artery rigid and inelastic, but often its interior, owing to adventitious deposit, becomes uneven and rough; there will therefore necessarily be increased friction between the blood and the lining membrane of the vessel, and a murmur will be developed, which will be heard at the period of the ventricular systole, and will be synchronous with the first sound of the heart, and with the pulse. As the ventricular systole ceases, the blood from the large vessels which come off from the arch of the aorta regurgitates into this vessel, and the passage backwards of this fluid over a similar rough surface causes a second murmur, which will, of course, be synchronous with the ventricular diastole, and the second sound of the heart.

When the stethoscope is applied over the first bone of the sternum in this diseased state of the arch of the aorta, a short, double, rough, harsh murmur, will be heard, loudest over the first bone of the sternum, and audibly frequently as low as the base of

the heart, sometimes a little lower down, but never at the apex of the organ. The first of these sounds is synchronous with the ventricular systole, and the pulse; the second, with the ventricular diastole. Both sounds appear to be near, and both are short and rough, not either prolonged or blowing. In some instances a murmur is present only with the first sound, or even this may be absent, and the second sound may be neither rough or harsh. This will occur when the arch of the aorta, although rigid and inelastic, has not lost, in any great degree, its natural smoothness and polish, but still permits regurgitation into it: the double sound then heard resembles very closely the double sound of the heart, and is commonly supposed to be transmitted beyond its normal limits. It may even happen that the arch of the aorta is dilated, rigid, and inelastic, and its lining rough and irregular, and yet no murmur will be developed: this will occur when the heart's action is feeble from softening or other alteration of the muscular tissue of the left ventricle, or when the arterial system is congested; or when the cavities of the left side of the heart are overloaded, or over-distended with blood. Under similar circumstances we know that a murmur may be absent in cases of considerable disease of the valves or orifices of the heart.

In addition to the physical signs above mentioned, this diseased state of the arch of the aorta is accompanied by the peculiar jarring of the pulse which was supposed to be characteristic of regurgitation through the aortic valves; by strong and visible pulsation of the carotids; and by a visible and locomotive pulse in the arteries of the upper extremities. The peculiar jerking of the pulse, and the visible and locomotive pulsation of the arteries, depend upon the same cause as where the aortic valves permit regurgitation: here, however, the regurgitation is into the aorta itself. These signs will be more marked if the arch of the aorta, in addition to being rigid and inelastic, is at the same time dilated, and, if the action of the heart is strong.

The physical signs of this diseased state of the arch of the aorta have been hitherto supposed to be referable to valvular disease of the aortic orifice: no writer that I am acquainted with seems to have been aware, that regurgitation into the arch of the aorta, from the larger vessels which come off from it, ever occurs, or that a backward current of blood in this part is capable of developing a murmur. This has probably arisen from the general symptoms, in the early stage, not being such as to attract particular attention; and, as patency of the aortic valves is one of the consequences of the long continuance of this diseased

* Guy's Hospital Reports.

state of the aorta, when the latter lesion has been found after death, the symptoms have been always referred to it.

The form of cardiac disease with which this diseased condition of the coats of the arch of the aorta has been most frequently confounded, is a state of the aortic valves permitting regurgitation, which it resembles in a murmur, synchronous with the second cardiac sound, accompanying both morbid conditions, in the jarring pulse, and the visible pulsation in the arteries of the neck and upper extremities, which are common to both. It may always, however, be distinguished by the character of the murmur with the second sound, which, in this diseased state of the aorta, is short, rough, and harsh, while in patency of the aortic valves it is always prolonged, soft, and blowing; as well as by the situation in which the murmur is transmitted to the ear: in patency of the aortic valves it is audible from the base to the apex of the heart: in this morbid condition it is loudest over the first bone of the sternum, and it is not heard below the base of the heart; at least, it is very indistinct below this point. Besides, in this diseased state, when a murmur accompanies the second sound, one is likewise always heard with the first, which is not the case in simple patency of the semilunar valves.

Venous Murmurs.

The abnormal sounds, which have their seat in the veins, may have either a blowing or a musical character. They all come under the class "inorganic murmurs," and have received a variety of names; as the venous murmur, or venous hum, the sound of continuous blowing, bruit de soufflet continu ou à double courant, bruit de diable, and musical venous murmur, or melodious blowing sound. They may all be included under the heads, continuous, and musical venous murmur.

Continuous venous murmur.—The continuous venous murmur, or venous hum, is termed continuous from its being a continuous uninterrupted sound, and venous from its seat. M. Bouillaud named it "bruit de diable," from its resemblance to the familiar sound made in the spinning of a kind of humming-top, which is known under the popular name of *diable* in France, and this name has been very generally retained since. It resembles pretty accurately the sound heard when a large univalve shell is held close to the ear; it has been also compared to the noise of the sea upon a distant sand, when the tide is flowing, or to the hollow sound made by a large forge-bellows constantly worked, or to

the buzz produced by a number of people talking together in a large room; and it may be imitated, as Dr. Hope observes, "by forcing the breath in whispering a continuous *who*." It is often accompanied by the arterial bruit de soufflet, which appears to increase its intensity at each systole of the ventricle, and it often acquires increased loudness at each inspiration, owing to the expansion of the thorax, which allows the blood more rapidly to descend to the right side of the heart.

The veins in which the continuous murmur is heard, are the jugulars, particularly the internal jugular, and the veins which open them. The situation in which it is best marked is immediately above the clavicle; it is not audible close under the chin; it may often be heard at both sides of the neck, though usually it is more feeble upon one side, or it may be limited to one side; and, in my experience, it is both more frequent and more intense upon the right than the left side, though M. Bouillaud states that he most frequently finds it upon the left side. The more direct course of the veins upon the right side of the neck, to reach the *vena cava*, may account for its greater frequency upon the right than the left side. The position in which to place the patient, in order to hear this murmur, is the sitting or standing posture, with the face inclined slightly towards the opposite shoulder, and the chin somewhat elevated.

The continuous venous murmur sometimes subsides suddenly at the point where it was audible a few minutes previously; slight change of position of the patient's head will produce this effect, or will alter the character of the sound. This depends upon the pressure exercised on the vein by the parts lying over it, which varies with the motions of the head. M. Bouillaud says, that drawing the larynx towards the opposite side will diminish the murmur suddenly, or cause it to cease; it does so by putting the omo-hyoid muscle upon the stretch, by which the deep cervical fascia is rendered tense, and pressure is made upon the internal jugular vein. Altering the pressure of the stethoscope alters the character of this murmur in a remarkable manner, sometimes diminishing, sometimes considerably increasing its intensity, and rendering it hoarse, or converting it into a musical murmur: when pressure sufficient to obliterate the current through the vein is made, either with the edge of the stethoscope, or with the finger above, the murmur ceases altogether.

Musical venous murmur.—The musical venous murmur is less frequently heard

than the continuous murmur: it has, however, its seat in the same vessels, is audible in the same situation, and occurs in precisely the same kind of cases. It is seldom audible in the male, and like the continuous murmur is more frequently heard upon the right than the left side of the neck. Like the latter, also, it intermits, changes its character, or ceases from very trifling alterations in the position of the patient: it is always preceded by the continuous murmur, which takes its place when it subsides. In order to hear either murmur, it is essential to employ the stethoscope: and although it is sometimes audible on very gentle pressure by the stethoscope, it generally requires stronger pressure with the instrument than is sufficient to develop a continuous murmur.

The musical venous murmur sometimes resembles a whistle with the lips, or the prolonged whistling sound of a current of air through a key-hole; sometimes it is more like the chirping of a bird, the singing of a kettle, or the humming of a fly; it has been also compared to the resonance of a tuning fork, the sound of the Jew's harp, or the prolonged vibration of a metallic cord. It often consists of a single note, sometimes of two, more rarely of several distinct notes. The French writers, who place its seat in the arteries, term it the melodious blowing sound, or the chant of the arteries; and both Laennec's and Bouillaud's works contain a scale, in which they have endeavoured to represent by musical notes the melody or air heard in some cases.

The musical venous murmur appears to have the same relation to the continuous venous murmur as the musical murmur of valvular disease has to the bellows murmur; the one is preceded by, and passes into the other, and the former is to be regarded as nothing more than an exaggerated variety of the latter. There is, in fact, no greater difference in the nature of the sounds, and in the mode in which they are produced, than there is (Dr. Hope observes) "between blowing with the lips and whistling."

Seat of the continuous and musical murmurs.—The continuous and the musical venous murmurs were for a long time supposed to have their seat in the larger arteries which come off from the arch of the aorta: this was the opinion of Laennec, who first discovered them; and it has been advocated by almost every French writer from him, to M. Bouillaud, M.M. Barth and Roger, and M. Beau, the latest authorities upon the subject. The first to correct

this error was Dr. Ogier Ward,* he it was who first demonstrated that the veins, not the arteries, were the seat of these murmurs, and who first pointed out the distinguishing marks between the venous and the arterial murmur.

In many cases, the continuous or the musical venous murmur are accompanied by the arterial bruit de soufflet already described; they may always, however, be readily distinguished from each other: the arterial murmur is an intermittent sound, the venous a continuous sound; the arterial murmur is a short, whiffing sound, while the venous has a humming or whistling character.

That the continuous and the musical murmur have their seat in the veins is proved by their diminishing, or ceasing altogether, when pressure with the finger is made upon the vein above, or when the stethoscope is pressed firmly on it, so as to obliterate the current: the feeble bruit, which is still occasionally audible, comes from the small venous branches in the neighbourhood. M. Bouillaud, who places the seat of these murmurs in the arteries, observes that "pressure above the vessel with sufficient force to interrupt the current will cause a cessation of the murmur," which is quite true: but it is not quite as easy as M. Bouillaud seems to think, to "interrupt the current" in the carotid or subclavian arteries; the degree of pressure required would cause rather more pain than most patients would like to submit to often; while the moderate degree of pressure necessary to interrupt the venous murmur would, if it had its seat in the artery, render it louder, instead of interrupting it. In fact, the trifling pressure which is necessary to cause the cessation of these sounds is the best proof that they have their seat in the veins; and that they are not seated in the arteries is proved by the true arterial bruit de soufflet being very often heard at the same time; and, by its continuing to be audible when the venous murmurs have been checked by the pressure of the finger or of the stethoscope.

Mechanism of production of the continuous and musical venous murmurs.—In order to explain the mechanism by which these murmurs are produced, it will be necessary to premise a few remarks. In preceding lectures I have endeavoured to prove that every sound, whether normal or abnormal, developed in the heart or arteries, is produced by friction between the blood and the lining membrane of the part; that abnormal sounds are nothing more than

* LONDON MEDICAL GAZETTE.

exaggerated normal sounds, and that the character of the abnormal sounds will vary according to a variety of circumstances; as the roughness or smoothness of the lining membrane, the nature and amount of the obstruction to the current of blood, the rapidity and strength with which the blood is propelled, and the physical condition of the circulating fluid, particularly its density and viscosity.

In the cases in which the continuous or the musical murmur are heard, the organs of circulation are in a healthy state; but the blood is altered; its density is diminished, its watery parts are increased, and its viscosity is less than that of healthy blood, while the current is more rapid than in health—conditions sufficient to develop a murmur in the arteries; and accordingly the arterial bruit de soufflet is very generally audible in such cases. The current through the veins being naturally feeble, compared to that through the arteries, its coats being lax and readily distensible, no great amount of friction can occur between the blood and the lining membrane of these vessels, even though this fluid be altered in the way that I have mentioned. In order, therefore, that a murmur should be developed in a vein, the friction between the blood and its lining membrane must be *mechanically* increased.

The murmurs which we are considering have their seat essentially in the jugular veins; principally, I believe, in the internal jugulars. Now in order that friction sufficient to develop sound should take place between the blood and the lining membrane of these veins, it is necessary that the tissues covering them—viz. the skin, the platysma, and the cervical fascia, should be made tense; which is effected by turning the patient's head slightly towards the opposite shoulder, and by elevating the chin a little, by which a moderate degree of pressure is exercised upon these veins in their whole course in the neck, and they are placed in a favourable condition for the development of sound. If the stethoscope be now applied, its pressure will slightly diminish the calibre of the vein, the friction between the blood and the lining membrane of the vessel will be increased, and a murmur will be developed, which will of course have a continuous character, because the current through the vessel is continuous. If increased pressure be made by the stethoscope the character of the murmur will be altered; it will be rendered harsher, or hoarser, or converted into a whistle, or other musical murmur, as the case may be; while if the stethoscope is pressed still more strongly upon the vein, so as to obliterate the current, the murmur will instantly cease.

I am therefore of opinion that the continuous and musical murmur, unlike the cardiac and arterial murmurs, are not naturally present in the veins of the neck, but that they are mechanically produced by the pressure of the stethoscope, when the coats of the vein have been rendered tense, by putting the parts covering it upon the stretch, by which the friction between the blood and the lining membrane of the vessel is increased. Thus, if we completely relax the fascia and muscles of the neck, by making the patient incline the head forward, and then apply the stethoscope, the murmur will be inaudible, or scarcely heard; while if we turn the same patient's head slightly towards the opposite side, the murmur will be loudly heard, although the same degree of pressure is made by the stethoscope.

Again, the bruit de diable is described by writers as being often audible one day and inaudible the next, and as disappearing suddenly at a point where immediately previous it had been loud. Now if we examine the connections of the deep fascia of the neck with the sheath of the cervical vessels, and with the muscles, we will perceive that a very trifling alteration in the position of the patient's head will render this fascia tense, or the contrary: and we can easily understand, therefore, how the pressure of the stethoscope may, in one position of the patient, fail to develop the murmur, while in another position the same amount of pressure will render it loud.

The venous murmur (as has often been observed) diminishes or subsides when the face is turned quite round towards the opposite side. Now the effect of this position is to render tense both the cervical fascia and the muscles which cover the internal jugular, by which so much pressure is made upon the vein as greatly to diminish the current through it, when of course the murmur will either subside or become very feeble. The venous murmur is sometimes heard on both sides, sometimes only on one side, and is usually better marked upon the right than the left side. This is readily explained by the more direct course of the blood towards the heart on the right than the left side. When this murmur has been only heard on the left side, the French writers (who suppose the carotid artery to be its seat) are obliged to refer it then to "some irregular distribution of the left carotid artery, by which this vessel is rendered more *superficielle*." "We must allow (M. Beau observes) a large share to the anomalies so frequent in the arterial system."

The continuous murmur ceases or diminishes materially in the recumbent posture, and returns again in the sitting or erect

posture. This may be accounted for partly by the effect of gravity upon the circulation in the jugular vein, and partly owing to the rapidity of the circulation being greater in the erect or sitting than in the recumbent posture. In cases of anæmia, change of posture exercises a more remarkable influence upon the rapidity of the heart's action than in the healthy subject: and as rapidity of the circulation is one of the conditions necessary for the development of the venous murmur, it follows that it ought to be more evident in the erect or sitting than in the recumbent posture. Besides, in the recumbent posture the fascia of the neck is more or less relaxed, and the vein is placed in an unfavourable state for the development of sound.

The foregoing circumstances all render it probable that the venous murmur is developed in the way which I have stated: this view alone reconciles all the apparent anomalies connected with it. Indeed, the theories heretofore advanced fail to account for its presence or its absence under many circumstances; while the majority of these theories seem to be little better than hypotheses, the correctness of which would require to be demonstrated before they could be adduced to explain the mechanism by which the venous murmur is produced.

The arterial and the venous murmurs will be again alluded to when I come to speak of what are termed "inorganic affections." It is only necessary here to observe that these abnormal sounds are very common in chlorotic, anæmic, and hysterical females, and in individuals who have suffered considerable loss of blood; and they are heard in all cases where the blood is altered, its serous portion increased, and its viscosity lessened. We seldom or never examine a chlorotic subject in whom some of these sounds are not present, frequently upon both sides of the neck, but seldom so loud upon one as on the other. The more advanced the stage of anæmia, the more intense in general are the murmurs; and as the anæmic condition is relieved by medical treatment, the sounds diminish in intensity, and eventually, when the patient's health is completely restored, they disappear.

CHLOROFORM IN OBSTETRIC PRACTICE. BY A. P. KING, BOSTON, U.S.

CHLOROFORM, as an anæsthetic agent, is one of the most powerful, and therefore one of the most valuable of its class. It is not proper that it should, in so short a time after its introduction, receive its final doom. More is to be learned of its power—its *modus operandi*—before we can justly place

upon it a proper estimate. To say that chloroform should be administered at all times and under all circumstances where anæsthesia is desirable, or that it should never be used because harm has been the result of its improper administration, is equally foolhardy.

The point which I should raise is this—Should chloroform be used in the practice of obstetrics? I answer, Yes—because it is more agreeably and conveniently administered than most other articles of the same class, and because it relieves the patient of a vast deal of suffering, which, without the aid of some anæsthetic agent, would be almost intolerable. We know that women have given birth to children, for thousands of years, without it—that they have borne the sufferings of travail with a fortitude unknown to the other sex. But the fact that they always *have*, is not conclusive that they always *should*, suffer, so long as satisfactory demonstration has proved that the parturient couch can be made

"Soft as downy pillows are."

We are culpable for withholding anæsthetic agents, in the practice of obstetrics, when they can be safely administered, as we should be for refusing to prescribe for disease under similar circumstances. The only question in the minds of many is as to the safety of the patient while under its influence. The general rule is, I believe, the patient is safe: notwithstanding, the rule, like all other general rules, may have its exceptions.

Opium does not *always* produce stupor—ipecac. does not *always* produce emesis; yet the general rule is not yielded in consequence of these exceptions. So with chloroform. Owing to some idiosyncrasy, beyond our power to fathom, it does not always produce the same effect upon different constitutions. Yet enough has been learned of its effect (I had almost said *modus operandi*) to satisfy all but the overtimid that a judicious administration of chloroform is not only practicable, but desirable. That there are circumstances under which it should not be administered, I have no doubt: I have seen them.

It has been asked by some, if it does not interfere with the progress of labour. I can answer from experience in the affirmative, so far as the exceptions to the rule go—but in the negative generally. By generally, I mean that in a large proportion of cases the contractions of the uterus are not interfered with. Rarely, *very rarely*, does it retard labour. I am using it with most of my lying-in women, and as yet have seen no ill effects from its inhalation.—*Boston Med. Journal*.

Original Communications.

AN ESSAY ON

UNHEALTHY INFLAMMATIONS.*

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Woolwich, Kent.

EPISTELAS.†

THAT medicine continues to lie under the opprobrium of a lack of *settled principles*, and that her sisters—the “exact sciences”—refuse to acknowledge her as one of themselves, is due, I think, to nothing more ostensibly than to the disregard which the great truth, that “the life of the flesh is in the blood,”‡ has experienced at the hands of her disciples. The long reign of *solidism*, under which she has dragged “her slow length along,” has operated, I think, most injuriously to her interests, and kept her down in the struggle for existence. When the *humoral* aspect of disease began to lose its ascendancy, and to be remembered only as a creation of the distempered fancy of the ancients, when that crisis overtook her, it is not, I think, too sorrowful a tribute to its memory to say, in the language of the illustrious departed, “Sol occubuit; nox” longa “secuta est.” The luminous mind of Sydenham was exerted for a while with some success to arrest the downward course of humoralism, and to throw the protection of a great name around the peculiar offspring of our forefathers; to which end, however, it needed no less unequivocal a declaration than this, that “a disease, how adverse soever its cause may be to the human body, is nothing else but nature’s endeavour with all her

might to expel the morbid matter for the good of the patient.” That in this aphorism resides the essence of almost all that has yet been *made out* in the numerous family of the blood diseases, is a truth which the humoral tendency of the present day, I think, sufficiently attests. The primary import of the *blood* (wherein, as early as the days of Moses, we were admonished that “the life” resides), in the causation of disease, has been industriously kept out of sight by the solidist philosophers who came upon the stage almost before the curtain had fallen upon the illustrious interpreter of nature here passingly alluded to. “The humoral pathology,” says Bichat, “is founded on truth, and in a great many cases we must allow that all should be referred to morbid humours.” Andral (Path. Anat.—“Lesions of the Blood”) observes, “I have now stated the facts and arguments which, in the present state of the science, ought to lead us to acknowledge the existence of certain alterations in the blood. What I have already said on the subject, in my opinion sufficiently demonstrates not only that they are often primary, that they precede those of the solids, and that, consequently, the origin of many diseases lies in the blood. If it is true that the mass of the blood may in certain cases be primarily altered, it follows that the existence of general disease is not merely imaginary. In fact, when all the tissues thus receive a vitiated blood, is it not consistent with sound physiology to admit that their regular modes of vitality, nutrition, and secretion, must be more or less deeply modified? We must either admit this conclusion, or deny the influence which, according to every physiologist, the blood exerts over each solid.” That the life of the flesh is in the *blood* is a lesson forced upon us as well by physiology and pathology as it is by reason. We have never heard, I was going to say, of such caprices of nature as beings without hearts—acardiac monsters; but it would be more correct to say, of beings without some fitting provision for the propulsion of the blood. But we know that the absence of a brain, the great centre of nervous life, is not incompatible with the continuance of existence; and that the removal of the head in some of the inferior animals is not attended by immediate arrest of the action of the heart; whilst we daily witness the annihilation

* Some papers, preliminary to this subject, have already appeared in the *Lancet* for 1849.

† I have already devoted one article in the *Lancet* (vide the No. for Dec. 22, p. 655) to the opening of this subject, wherein the literary history of the disease may be referred to.

‡ “When the blood once becomes solid, it displays indubitable symptoms of *vitality*;—vessels are produced, and secretions formed in it; and different alterations of nutrition, resembling those observed in the tissues, may also occur. If we examine whence this coagulated blood derives its vitality, we find that it cannot partake of the common life of the rest of the body, since it very often merely touches the surrounding tissues, without being in any manner continuous with them.”—A Treatise on Pathological Anatomy, by G. Andral, vol. i. pp. 647-8.

of the functions of the spinal cord by disease, without the necessary death of the individual. But the moment we withdraw the influence of the *blood* from an animal, as immediately is the disruption productive, as a direct sequitur, of its death; whilst, if instead of withdrawing it altogether, we do so only partially, a series of phenomena ensues which eloquently bespeaks the difficulty with which the functions of life are carried on upon the diminished supply, and which denotes the struggle to which they have been brought in the effort to support themselves upon the same. If, again, in place of experimenting upon the quantity, we act upon the *quality* of the blood, we arrive at equally serious results in the end, though not without some difference in the aspect and order of the phenomena. M. Gendrin (*Histoire des Inflammations*, tome ii), injected some variolous blood into the circulation of an animal, which was rapidly followed by fatal symptoms; and in his work on fevers, he recounts the particulars of another experiment upon a cat, into the cellular tissue of whose groin he injected some of the blood taken recently from a patient who laboured under a "putrid fever." The animal immediately fell under an aggravated form of typhoid symptoms, and died in about seven hours after. The post-mortem appearances were such as principally depended on rapidly ensuing decomposition; but what concerns our present subject to notice more particularly, is that "the blood throughout the whole body was black and fluid;" in a word, decomposed and dissolved. In the experiment of injecting poisons into the bloodvessels of animals, we are introduced to an illustration of the *more immediately fatal* consequences attendant on a vitiation of the properties of the blood; whilst in diseases where the blood has undergone a palpable and acknowledged deterioration or process of poisoning, as in chlorosis* and yellow fever,† we witness a slower and less

striking influence upon life, because its manifestations are *not at once* suspended by the operation of the latter.

How forcibly do not these facts confirm the great truth of the Mosaeic writer, to which I again invite the reader's earnest attention. I might advert, too, I think, to the relatively *larger proportion* of the fluids of the body as compared with that of the solids, as another proof of the importance of the humors in the economy, an argument in itself in favour of the blood's pretensions to *originate* disease, as I believe it to be the *primum mobile* of health. "Blumenbach possessed the entire *perfectly dry* mummy of a guanche, or aboriginal inhabitant of Teneriffe, presented to him by Sir Joseph Banks, which, with all its muscles and viscera, weighed only seven pounds and a half."* The

expedient to bleed, the blood wore the same general appearance. After a separation had taken place, the serum assumed a yellow shade, often a deep orange, and a portion of the red globules was invariably precipitated. It occurred to me, that if the remote cause resided in the common atmosphere, the blood of all who had inhaled it a certain time would exhibit similar phenomena. It accorded with the pathology I had conceived, to conclude that all who lived in an atmosphere so impregnated were constantly predisposed, and that an additional or exciting cause only would be required to develop the symptoms in form. To ascertain the appearances of the blood, in subjects apparently in good health, I drew it from five persons who had lived, during the whole season, in the most infected parts of the city, who were, to every external appearance and inward feeling, in perfect health. The appearances of the blood could not be distinguished from that of those who laboured under the most inveterate grades of the disease. As this experiment might have been considered inconclusive, unless the blood could be compared with that of those who lived in a purer atmosphere, remote from the evolution of miasmata, I selected an equal number of persons who dwelt on the hills in Baltimore Country, and drew from each of them ten ounces of blood. The contrast in the appearances was so manifest, that no cause for hesitation remained. There was neither a preternaturally yellow serum, nor a red precipitate: the appearances were such as we find in the blood of healthy subjects. A young gentleman having returned from the western part of Pennsylvania, on the 10th September, in good health, I drew a few ounces of blood from a vein on that day: it discovered no deviation from that of other healthy persons. He remained in my family till the 26th of the month, and on that day I repeated the blood-letting. The serum had assumed a *deep yellow hue*, and *copious precipitate of red globules* had fallen to the bottom of the receiving vessel. Of the six persons whose blood assumed the indications of the remote cause, four were seized with fever during the epidemic: the other two escaped any formal attack, but complained occasionally of headache, nausea, and other indications of disease."—*Observations on the Healthy and Diseased Properties of the Blood*, by William Stevens, M.D., pp. 223-5.

* *The Physiological Anatomy and Physiology of Man*, by R. B. Todd, M.D., and William Bowman. Part i. p. 84.

* Although chlorosis may not be thought fairly to come within the category of the *blood poisons* in the same sense as fevers and the exanthemata, yet its eminently humoral essence exposes it to the opprobrium of relationship (though a distant one) with that family.

† Dr. Potter observes, in his *Memoir on the Yellow Fever*, that "in September, 1800, a fever was daily obtruded on the observant practitioner, which, although it had long been familiar to me, had not then been fully estimated. It was remarkable that, in all cases in which it was detected,

primary importance of the humors in the great scheme of animal life is happily instanced in the following quaint sketch:—"Certainly, an adult man, who weighs two hundred pounds, in his first original lay concealed in a very little drop of a seminal liquid, and from so small a beginning growing up to so large a weight, owed all his increase in the solid parts to the supplies he received from the fluids, as appears from the observations of Malpighius, attempted of old by Hippocrates (*De Naturâ Pueri*), upon the incubation of eggs; in which we see the young chicken grows in one and twenty days, from too small a bulk to be seen by the naked eye, to have very firm parts out of the liquid of the white attenuated by incubation."

"Inasmuch," says Andral, "as the blood nourishes the solids, and as without its presence they cannot support life, the state of the solids cannot but be influenced by the state of the blood. The chemist might as well say that the nature of a body does not depend on the nature of the elements that compose it. On the other hand, the solids, considered with respect to their relations to the blood, form but two classes; the one, contributing to *make* the blood, such as those concerned in the actions of absorption, digestion, arterial circulation, and respiration; the other, contributing to *unmake* it, those, namely, concerned in the actions of various circulation, secretion, and nutrition. No one solid, therefore, can undergo the slightest modification, without producing some derangement in the nature or quantity of the materials destined to form the blood, or to be separated from it. Physiology, then, leads us to the conclusion that every alteration of the solids must be succeeded by an alteration of the blood, just as every modification of the blood must be succeeded by a modification of the solids. Viewed in this light," continues he, "there is no longer any meaning in the disputes between the solidists and the humorists; the system appears to constitute but one great whole, indivisible in the state of health, as well as in that of disease; the division of the parts of the body into solids and fluids seems to be a distinction of small importance, and one

that is not always just, since it ceases to exist in the intimate structure of the organs, in which all the grand vital phenomena take place, and in which, also, occur all the changes that constitute the morbid state."* I do not recognize either the force or the correctness of the argument as set forth in the concluding words of the last paragraph. Does Andral mean that because "all the grand vital phenomena take place in the intimate structure of the organs," those phenomena are therefore independent of a healthy quality of blood—nay, that they are not, therefore, more directly dependent upon such? "Physiology leads us to the conclusion that every *alteration of the solids must be succeeded by an alteration of the blood.*" But I am at issue with M. Andral upon the very *premises* of his argument. I deny the logic of his reasoning: I deny that the alteration of the solids ever *precedes* the alteration of the blood; although, when so altered, they may and do react upon the latter by interrupting the physical and vital processes which are exerted upon it by the bloodvessels. What! exclaims the solidist, shall not the earthy and atheromatous deposit in the bloodvessels occasion an alteration in the "manner or the matter" of the blood? And shall not the tubercular or malignant disorganization of the respiratory organs interrupt the vital and physical changes essential to the integrity of the blood? Assuredly they shall, *in their turn*. But whence originated the earthy and atheromatous, the tubercular and malignant alterations, but in *that blood* which now is *secondarily* influenced by those deposits?

Huxham observes, that "a due consideration of the state of the solids is a matter of high importance to physicians; not only in chronic, but also in acute diseases; for they are generally the *primary efficient causes of the particular states of the fluids.*" This authority seeks to illustrate the above principle by describing two opposite states of the blood,—one, in which "the humors are too dense and viscous; in which the blood globules are in too great quantity, and too closely compacted or condensed; in which the serous globules are so likewise;" the other, characterized by "too few blood globules, and those too loosely

* The Commentaries upon the Aphorisms of Dr. Herman Boerhave. By Gerard Van-Swieten, M.D. Vol I. pp. 39-40.

* Andral's Pathological Anatomy—Lesions of the Blood, vol. I. pp. 648-3.

compacted; and in which the serum is too watery and rapid, and sometimes of a ropy, slimy nature," &c. Now, although Huxham does admit these opposite conditions to have a primary and independent origin of their own, he nevertheless sets down their respective operation upon the several organs of the body to the account altogether of the physical, and not of the vital, effects which they produce upon the bloodvessels. Thus, for example, "a strong, rich blood always attends a strong elastic set of vessels, and a weak, watery blood, a relaxed habit of body,"—as though the constitution of the blood took its standard of perfection, or the opposite, from the character of the bloodvessels, instead of itself determining the quality of the latter. But Huxham only followed here in the footsteps of his illustrious predecessor, Boerhaave, in whose "weak lax fibre," and "stiff elastic fibre," he formed the model of his own system of physiology.

We meet with a remarkably diverting example of the truth with which I have opened my present paper—to wit, the exclusion of the blood from a due share in the causation of disease,—in the celebrated doctrine of *Girtanner*, who endeavoured to prove that the principle of life was dependent on irritability, or rather, that irritability is itself the principle of life! "The irritable pulse," says Girtanner, "from the first moment of its existence to that of its dissolution, being constantly surrounded by the body which acts upon it, and stimulates it, and upon which it reacts by its contraction, it follows, that during the period of its existence, the irritable fibre is in continual action; that its existence consists in action; and, that it is not a passive state as some authors have asserted. Hence, external objects having no immediate action upon the nerves, and only acting upon them and producing their different sensations through the medium of the irritable fibre, it is plain that the ideas we have of external objects are not conformable to those objects, but that they are varied and modified by the irritable fibre through which they are transmitted to us. Objects, therefore, appear different according to the different states of the fibres. When the irritable fibre has lost its tone, and fails, either from an access of the irritable principle, or from a deficiency of this principle, it is dis-

eased, and the system of which it forms a part suffers and becomes diseased, through sympathy. All the diseases of animals may be ranged under two heads; to wit, first, the *diseases of accumulation*, caused by the accumulation of the irritable principle and the diminished action of the habitual stimuli; secondly, the *diseases of exhaustion*, caused by a defect of the irritable principle proceeding from the increased action of the habitual stimuli, or from the addition of new stimuli. Under these two classes may be ranged all diseases whatsoever (!)"

But, at the risk even of fatiguing the patience of the *matter-of-fact* reader, I must be suffered to complete this diverting picture.

As though Girtanner had been seized, at the close of the foregoing passage, with some forebodings of the fate of his bantling, he exclaims:—

"Paradoxical as this proposition must necessarily appear to those who have not reflected on the subject, it is nevertheless true, and I shall give the most convincing proofs of it in a *work I am about to publish* (!)"

Girtanner continues—"The effect produced upon the irritable fibre by any stimulus or s , being always equal to $a \cdot b$ " (a representing the force of the stimulus, and b the degree of irritability of the fibre), "it follows, that the value of s and b being known, the value of a is also known. But, admitting an unity fixed and constant, it will be easy, in all cases, to express by numbers, the degree of irritability of the fibre, and the degree of the force of the stimulus, or the value of a and b ; consequently, it will be easy to find the value of s . All the art of medicine, then, consists in finding the value of s , that is to say, in finding a stimulus adequate to restore the tone of the fibre. Thus, if these principles be true, physis, which at present is an art of mere conjecture, will be reduced in time to the certainty of calculation; and, after tables shall be formed to express the value of a and b , and the signs by which they may be known, this calculation will be so simple and easy, that it will form a part of the education of every individual (!) But further, the irritable fibre being the same in all organized nature, diseases and their remedies will, of course, be the same for all organized beings; there will then be no distinction between me-

dicine, farriery, and agriculture, but all these sciences will be confounded and become one, under the general name of universal physiology (!)"

Girtanner then brings these striking principles to elucidate the subject of fever. "When any local stimulus continues to act upon any part of the system, the circulation becomes more rapid, and a fever is the consequence. Is the stimulus weak, a slow fever ensues, which will, by little and little, exhaust the irritability of the system, and the patient will die of a consumption. Is the stimulus stronger, or the fibre upon which it acts more irritable, we shall have an ardent fever, which will exhaust the irritability in a less time. In fine, is the stimulus very violent, or the fibre diseased by an excess of irritability, we shall have a putrid fever, which will destroy the patient, whether animal or vegetable, and will exhaust the irritability in a very short time (!)"*

Well would it have been for suffering humanity had we stuck to the *simple principles* of Girtanner, and, reducing our corporeal infirmities under the discipline of a few mathematical tables, have elevated medicine to the "certainty of calculation!" But, then, we have since embarrassed ourselves with the pretensions of that very inconvenient claimant, the *blood*; and hence the obscurities and perplexities which in consequence we have brought about our ears!

But I crave the reader's forgiveness for so long a digression from the proper subject of this paper—*Erysipelas. A nos montons, donc.*

In introducing this subject originally to the notice of my professional brethren,† I set out with the remark that "I am much mistaken if I do not succeed in establishing the claims of this disease to take its stand in the foremost rank of that category of inflammations which possesses the distinctive characteristics of an origin in a morbid poison, and an immediate connection with an adynamic condition of the system."

I now proceed to redeem this undertaking, as far as the present amount of our knowledge on the subject shall enable me; and, in doing so, I propose to examine these two questions in the

order in which I have enumerated them, and to examine them separately and apart from each other. And, first, of the former.

That there is something very peculiar, if not specific, in this particular form of inflammation, is a conclusion arrived at by the concurrent testimony of all ages, and by the champions of almost every sect into which the sciences of medicine has been cut up. "To rest satisfied that erysipelas is an inflammatory affection of the skin would be to remain in ignorance of its practical nature; for such has been the advance in our method of looking on things, that we have learned to regard the word inflammation* as expressing a very variable state, oftentimes such as by no means warrants depletion nor so-called antiphlogistics. That erysipelas is of the nature of inflammation we cannot doubt: we can imitate its results by applying a blister to the skin; and, when the integument is loose, we get a large amount of cedematous effusion, and often the production of pus in the parts beneath, as in the eyelids." "That erysipelas is an inflammatory disorder few will be disposed to deny; but I think as few will hence infer that we must treat it by depletion, antimony, &c. Inflammation is a condition which occurs in states of the system most

* "When the augmentation of the natural excitability is attended by pain, redness, and swelling, it is termed inflammation. This metaphorical expression, invented in the infancy of the science, was originally intended to represent a morbid state in which the parts affected appeared as if they had been actually submitted to the action of fire. As it was originally adopted into medical language without having any precise or well-defined idea attached to its signification, either as regards its intimate nature, the symptoms that indicate its existence, or the morbid lesions that characterise its progress, it has now become so vague in its signification, and in its interpretation so arbitrary, that it has really lost all value as a term of science; and, like an old coin from which the original impression has been effaced, should be forthwith withdrawn from circulation, as calculated to produce constant error and confusion. In fact, inflammation can now only be considered as the expression of a complex phenomenon, comprehending under it several other phenomena which have neither a necessary nor even a constant dependence on each other."—*André's Pathological Anatomy, Preliminary Observations*, vol. i., pp. 12-13.

Another distinguished physiologist of the present day has said—"Nothing is better known in the aggregate than the indications and tendencies of that state which pathologists have long been accustomed to denominate 'inflammation'; and, in their detail, few things are less clearly understood and satisfactorily explained."—*The Physiology of Inflammation and the Healing Process*, by Benjamin Travers, F.R.S., p. 24.

* Vide Girtanner's *Memoir on the Laws of Irritability*, translated by Dr. Beddoes.

† *Lancet*, December 2nd, 1846, p. 635.

widely different, as between simple pustular acne and violent typhoid small-pox. Pustules result in both, and both are inflammations of the skin; but, to say no more than this would leave us far off from anything like appropriate treatment, which, indeed, can never be determined by classifying a disease, but by studying the state of the system in which it occurs.*

But, now to address ourselves to the difficult question, upon what circumstance, or combination of circumstances, we are to account for the peculiar features which the erysipelatous inflammation (when and wheresoever occurring) is in general observed to put on? If erysipelas be in reality an *ordinary* inflammation, then let the supporters of such opinion explain the causes of its frequently sudden appearance and as sudden disappearance—of its erratic habits—of its occasional dependence upon a wound, but more frequent occurrence without assignable cause—of its disobedience in general to depletory treatment, and marked subservience to the administration of stimulants—of its inability to limit its range by the effusion of coagulable lymph, as in the instance of ordinary or common inflammation, &c. &c. &c.?

That these peculiarities cannot be explained upon a reference to the nature of the structure involved, is plain from the very different behaviour of "*common inflammation*" when developing itself in the same texture—viz., in the cutis,—an opinion, however, I must admit, directly at variance with that of Mr. Lawrence, who lays it down that there is no difference between erysipelas and other inflammations, and that any anomalies which may seem to exist in the former are referable to the peculiarities of the cutaneous and cellular structures in which it occurs

But that this position of Mr. Lawrence's is untenable must be evident from the circumstance to which I have just alluded,—namely, that the skin is subject to "*common*" inflammation (as Mr. Samuel Cooper expressively designates it); and which common inflam-

mation, when attacking that structure, observes a line of conduct in every respect the reverse to that of the erysipelatous. I ought, however, to remark that Mr. Travers has lent the weight of his name to that view of the question which seeks to account, in some measure, for the peculiarities of erysipelas upon the argument of peculiarity of structure involved:—"My own opinion is, from careful reflection on its history and phenomena, that erysipelas derives its local peculiarities from those of its seat,—namely, the membranous capillary circulation; and from the intermediate influence of the nervous system between this and the heart, and all the other organs of the body, its peculiar 'pathology.'"

"It has been supposed," observes Hunter, "that the different species or varieties of inflammation arise from the difference in the nature of the part inflamed; but this is not the case; for, if it was, we should soon be made acquainted with all the different inflammations in the same person, at the same time, and even in the same wound: for instance, in an amputation of a leg, where we cut through skin, cellular membrane, muscle, tendon, periosteum, bone, and marrow, the skin should give us the inflammation of its kind, the cellular membrane of its kind, the muscle of theirs, the tendons of theirs, the periosteum, bone, marrow, &c. of theirs; but we find it is the same inflammation in them all: it is the adhesive in them all if the parts are brought together; it is the suppurative if the parts are exposed."—Vol. iii. "*Fundamental Principles of Inflammation*," p. 313.

That the specific and *unhealthy* characters of the erysipelatous inflammation are not to be explained upon the assumption of its predilection for strumous subjects, or for those otherwise debilitated and depraved in constitution, is demonstrable from the fact that we see it daily set up, as well in the young, robust, and temperate, as in those more advanced in life, the cachectic, and the debauched. It is as common in the infant as in the sexagenarian—in the artisan as in the noble—in the one sex as in the other.*

* Dr. Gull on the "*Alliances of Erysipelas*," vide a paper read at the South London Medical Society, *MEDICAL GAZETTE*, vol. xlii. p. 999.

† "The skin, like all other parts, is often the seat of *common inflammation*."—*Cooper's Surgical Dictionary*, article *Erysipelas*, p. 481, ed. 6th.

* I have had no personal experience of the greater frequency of erysipelas among women than in our own sex, although, that such is the case is usually laid down by systematic writers on the disease, Copland amongst others.

Upon what, then, do its peculiarities depend? "It does not appear to me," observes our lamented lexicographer, "that any very exact information has been established respecting the causes of erysipelas. We absolutely know nothing about the immediate cause; the prevailing ideas concerning the predisposing causes are vague; and only those causes termed *exciting* appear entitled to much confidence"—(Cooper's Surg. Dict. p. 453). Upon what, I repeat, do its peculiarities depend; if not upon the nature of the structures involved—neither upon the existence of any of the cachexias in those on whom it fastens—nor in sex, age, or habit of body? If upon none of these we may throw ourselves for a solution of the question, from what quarter are we to expect a glimpse of light that shall open a path to a better understanding of its nature, and, as a direct consequence, to some fixed and rational principles for its treatment?*

If we turn to the *solidists* in our difficulties, we derive but little satisfaction at their hands. In their "weak, lax," or "stiff elastic fibre," we shall find but little to help us out of these perplexities.

[To be continued.]

* Although it may be difficult to carry the "*ablatâ causâ*" principle into immediate operation in this class of diseases, yet in none can it be more important to address ourselves to the *origo mali*, if the view which I take of the "immediate" cause of erysipelas be the correct one. I cannot but think that Dr. Graves has made a false move in medical philosophy, when, in the present inquiring age, he inculcates such principles as these:—"The moment we attempt to inquire into the causes and modes of vital action we begin to, retrograde, and all becomes hypothesis and confusion." And, in recommending a closer study of "morbid anatomy" as the most unerring guide to the proper comprehension of disease, and to its best directed treatment, he observes—"In order justly to estimate its importance (morbid anatomy), we should recollect that the first alteration in the texture of a part is not the cause but the consequence of disease; for in every healthy organ the texture is natural; and as every change of texture is produced in consequence of derangement in the vital action of the vascular system of the part, it is obvious that structural alteration must in the first instance be always produced by functional derangement. Thus the physical alterations which attend external inflammation—the tumefaction, the heat, the redness, are not the causes but the consequences of disease. But in thus reducing them to the rank of symptoms, do we diminish their importance? Certainly not; for, being immediately connected as effects with the primary cause, they prove the most useful of all symptoms in enabling us to ascertain the seat and progress of diseased action. In this respect they possess a manifest advantage over the general or constitutional symptoms. Thus, in cases of spontaneous gangrene, phlegmonous inflammation,

ON THE
TREATMENT OF THE "WARTY"
ULCERS OF MARJOLIN, BY
MEANS OF THE CHLORIDE OF ZINC.

By H. FEARNSIDE, M.B. LOND.,
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In the May number of the Dublin Quarterly Journal of Medical Science, at the close of a "valuable paper on the 'Warty Ulcer of Marjolin,' by Dr. Robert Smith, there occurs the following passage:—"Were I to attempt the cure of this disease by caustic, I would feel inclined to place most reliance upon the chloride of zinc, the efficacy of which in destroying cutaneous cancer has been highly extolled by Dr. Canquoin, of Paris, and likewise by Dr. Uré."

Having successfully employed this agent in an example of the disease which fell under my care about two years ago, it has appeared to me that a short history of the case may probably be deemed of sufficient interest to merit publication.

G. H., æt. 55, a large unwieldy man, of phlegmatic temperament, came under treatment in April 1848. His occupation was that of a porter. He stated that his habits were temperate, but he had the aspect of a man accustomed to indulge in malt liquor. He had always enjoyed good health. His parents died at an advanced period of life.

About nine years previously, in unloading a dray, he sustained a severe injury to the front of his right leg by its coming into violent contact with a large and heavy cask. A considerable amount

tion, or erysipelas, what practitioner would be contented to draw his indications from the general symptoms, disregarding the appearance of the affected part?" (Graves's Clinical Medicine, p. 15, ed. 1st.) I will answer this question by propounding another to the writer. In the treatment of the cases of glanders and button-farcy so graphically detailed in the forty-fourth chapter of this same work, was a knowledge of the "immediate" cause of the inflammatory swellings over the exterior of the body of *insignificant* import in comparison with a study of those local appearances themselves? Of what advantage were the usual appliances for inflammatory swellings under such circumstances? And, under an ignorance of the "*immediate cause*" of those inflammatory swellings, how greatly would not the fatal issue have been precipitated by an active system of depletion for their relief!

of inflammation ensued, upon the subsidence of which there remained a circumscribed swelling of the size of a pullet's egg, which underwent little or no change for some years. About ten or twelve months prior to his coming under my notice, he began to experience pain in the tumor, which slowly increased in size, and acquired a dark red colour: the skin covering it was indurated and uneven. Poultrices were applied for some time, until the skin gave way, and an open sore resulted, which presented a grey unhealthy appearance. The discharge from it was thin, fetid, and often bloody. From the surface of the ulcer arose a diseased growth, which soon acquired considerable elevation, and from which profuse hæmorrhage took place from time to time: on being removed by any cause it was rapidly reproduced. When I first saw him, the morbid growth presented the following characters:—From the centre of the front of the right leg, occupying a space of about three inches in diameter, there sprung a dark grey substance, which projected at least two-thirds of an inch above the surrounding skin. Its surface had a granular appearance—at first view not unlike the head of a cauliflower: on closer examination, it was seen that this was occasioned by the prominent extremities of coarse fibres which arose from the base of the ulcer, and were collected into masses, separated from each other by deep fissures. The margin of the sore was thickened, elevated, and possessed little or no connection with the fibrous structure above described: the surrounding integument had undergone considerable warty induration and discolouration. A thin, ichorous discharge proceeded from the part; the pain experienced was not violent; there was no enlargement of the popliteal or inguinal glands. He complained of being weakened by repeated loss of blood, but his health was otherwise good. He had been previously under the care of one or two medical men, and had been advised to submit to amputation of the leg, on the supposition that the disease was fungus hæmatodes.

He was seen with me by my friend Mr. Noble, who considered the case a favourable one for the employment of the chloride of zinc; and, accordingly, it was determined to attempt the cure of the disease on the plan recommended

by Mr. Alexander Ure (MED. GAZ., vol. xvii. p. 891).

On its first application, the remedy was mixed with flour (Canquoin's formula). But little pain was occasioned by it, and the resulting slough was not deep. When it was next employed, it was blended with pure sulphate of lime, as proposed by Mr. Ure. The whole of the morbid growth was covered to the depth of a third of an inch with a paste composed of one part of chloride of zinc and two parts of sulphate of lime. The application gave rise to severe pain, which was only partially under the control of opium. At the expiration of four days an extensive and deep slough was produced, which separated in the course of the ensuing week or ten days. The greater part of the sore was then covered with healthy-looking granulations; but, to get rid of two or three small masses of whitish semi-cartilaginous substance, as well as to overcome the indurated condition of the margin of the ulcer, it was requisite again to have recourse to the caustic paste. The subsequent progress of the case was, upon the whole, highly satisfactory. No hæmorrhage ensued after the first complete application of the chloride of zinc. In the course of a month small florid granulations had risen to the level of the adjoining surface, and cicatrization commenced. The complete healing of the sore was delayed for some time by the patient's imprudence; for, considering himself well, he could not be induced to submit longer to regular treatment. After the lapse of three months, however, a small ulcer of the size of a shilling was all that remained of the apparently formidable disease; and he afterwards reported himself as cured.

REMARKS.—The case of which an outline has been given above exemplified well the characters of a peculiar form of ulceration sometimes seen in old cicatrices, and to which the attention of the profession has been specially called by Marjolin,* Mr. Cæsar Hawkins,† and Dr. Robert Smith,‡ to whose several papers I beg to refer for a complete history of the affection. An ac-

* Dictionnaire de Médecine.

† Medico Chirurgical Transactions, vol. xix.; and MEDICAL GAZETTE, vol. ii. 1840-41.

‡ Dublin Quarterly Journal of Medical Science, May 1850.

quaintance with its distinguishing features is important, both from the severity of the disease in itself, and also from its resemblance to fungus hæmatodes, with which it has several times been confounded.

As before stated, the case is recorded for the purpose of illustrating (so far as one case can) its successful treatment by cauterisation with the chloride of zinc. Where the diseased growth is of moderate size, as in the case detailed above, and where it has not involved (at least seriously) the subjacent periosteum and bone, this mode of attacking it seems to be principally applicable.

Respecting the nature of this disease some diversity of opinion has prevailed.

In his first memoir upon this subject, Mr. Hawkins remarked that "it did not produce any contaminating influence upon the absorbent glands, and had no tendency whatever to reappear in a distant and unconnected part of the body;" but, in a lecture published in 1841, he viewed its cancerous nature as more decided. Dr. Robert Smith declares it to be "undoubtedly a form of cutaneous cancer;" but he thinks that it is "malignant only in the lowest degree."

The microscopical evidence as to its cancerous nature is negative. Rayer considers the peculiar fibrous growth as the result of hypertrophy of the papillæ of the skin. This explanation is also offered by Mr. Paget (*MEDICAL GAZETTE*, vol. i., p. 286, 1838-9), who states that he "finds this disease of the papillæ connected with a peculiar and intractable form of ulceration, which occurs not unfrequently upon the shins. It is remarkable for the manner in which it spreads down to the periosteum of the tibia, and even into the bone itself, from whose medullary tissue a soft, fungous, and irritable mass of vascular granulations arises, and occupies the centre of the sore."

It would thus appear that this diseased structure belongs to the class of morbid tissues described by Professor Bennett under the name of "*epithelial canceroid growths*." To this head must be referred many cases of so-called cancer of the lip, chimney-sweepers' cancer, cauliflower excrescence of the uterus, and some other forms of disease which, although possessing characters of considerable gravity, yet differ mate-

rially in structure from true cancerous or malignant growths.

But, from its occasionally affecting the absorbent glands, and its removal being followed by unequivocal cancerous disease elsewhere (as in Dr. Smith's cases, I. and IV.) we must admit that in some instances it has a more decidedly malignant nature; thus affording an exemplification of a class of cases lying on the confines of benign and malignant disease, but capable of assuming the character of the latter under favourable circumstances.

INCOMPETENCY OF MEDICAL EVIDENCE IN CERTAIN CASES.

In a late trial before the Supreme Court at Lowell, in a case of alleged lunacy, medical testimony was brought to bear upon both sides. The petitioner, an aged man, among other testimony offered by him to prove his sanity, brought forward several most respectable and distinguished physicians of this city; the respondent, who was the guardian, having Drs. Bell and Fox, of the McLean Lunatic Asylum, on his side. Judge Metcalf, in his charge to the jury, ruled all the medical testimony as *incompetent*, excepting Drs. Bell and Fox's, they being considered the only *experts*. However, the intelligent jury thought differently, and rendered their verdict for the petitioner,—a just rebuke to such a judicial decision. Who are considered competent to certify to insanity in case of admission into lunatic asylums? Would not these same medical gentlemen have been deemed suitable? If so, we really should like to know if they are not *experts* enough to tell when their patient has recovered his reason? Such partiality for *experts*, in judicial investigations, is getting to be more fashionable than wise. Common sense is, after all, the best guide for a magistrate; for then he can ascertain for *himself* who is qualified to give an opinion, and who is not. This is also the best guide for a professional, as well as a non-professional man, in ascertaining whether there is soundness of mind; and we are surprised to learn that a magistrate should feel inclined to exclude medical testimony in such a case, from the fact that the witnesses were not considered *experts*.—*Boston Medical Journal*.

. The question is, Are they competent to certify or give testimony, unless they have had experience in cases of insanity?

PRESENTATION OF THE BACK AT THE FULL PERIOD OF GESTATION.

BY W. B. KESTYEN, M.R.C.S.

PRESENTATIONS of the back or loins are forms of preternatural labour so rarely met with, that the following case seems worth recording:—

Mrs. D—, aged about 30, at the expiration of the full period of her fifth pregnancy, was taken in labour at 5 a.m. on May 26th, 1869. I saw her between 6 and 7 o'clock; the os uteri was fully dilated, the pains strong, and occurring regularly. The membranes were entire. I could not then reach the presentation sufficiently to form an exact opinion of its nature. The pains continued regular and strong. At 11 a.m. I could detect a broad flat surface about the brim, which clearly was neither head nor breech. The mother was becoming much fatigued, and no further progress made in the descent of the child, still above the brim of the pelvis. The membranes gave way at 1 p.m. I could then bring my finger in contact with a greater surface of the presentation: I felt what I supposed to be a shoulder, and considered that the occiput was resting on the pubis. I then considered it to be my duty to turn, without waiting for assistance, as the pains had been strong for between seven and eight hours, and the patient's strength was becoming exhausted, and there appeared no chance of the position of the child being changed by the natural action of the uterus. On passing my hand into the uterus, I found that what I had supposed to be the shoulders was the crest of the ileum; and that my hand, in passing to the fundus of the uterus, traversed the length of the back, and came in contact with the vertex, situated posteriorly and superiorly in the fundus of the uterus. I then passed my hand (the right hand) over the body of the fœtus anteriorly, and, fixing my forefinger in the groin, brought down one leg into the vagina; at the same time pressing the body of the child upwards into the cavity of the uterus, and delivered footling, bringing down one extremity only.

The child was apparently still-born, but the means employed for its resuscitation were successful after having been perseveringly employed for an hour.

The mother had a speedy and perfect recovery. It may be remarked, that in her preceding labour I had been obliged to have recourse to the use of the forceps for the delivery of this patient, owing to an arm having passed into the vagina with the head.

REMARKS.—The rarity of this form of preternatural labour is shown by the following notices, collected from numerous obstetric authors:—

Dr. Merriman, in his treatise on "Difficult Parturition," states:—"In the very extensive practice of my uncle, the late Dr. Merriman, and in my own practice, amounting together to nearly 20,000 labours, no instance has occurred of either of these presentations, except in one or two cases where the mother had not completed her seventh month of utero-gestation, and in these the children passed double through the pelvis." Dr. Merriman, however, relates one case which was communicated to him by a practitioner in the country, and states that he was informed by another friend that he had twice met with the presentation of the back.

The advice given by Dr. Merriman, on theoretical grounds, for the management of this case, is precisely such as it was found practically necessary to follow in the case that has been now related. "Should such an unusual case occur," observes that author, "it is possible that in the course of the labour the presentation would be changed to one more favourable. If no alteration in the position took place spontaneously, the introduction of the hand would be necessary, as soon as the parts were sufficiently dilated, to bring down the feet, and to deliver before the strength of the patient was exhausted." p. 96.

Dr. Collins, in his "Practical Treatise on Midwifery," does not mention this presentation. It is surprising that, in such a vast mass of personal experience as therein recorded, a similar case should not have occurred.

Neither, in Dr. Robert Lee's "Lectures on the Theory and Practice of Midwifery," nor in the same author's "Clinical Midwifery," which contains the histories of 546 cases of difficult, preternatural, and complicated labour, do I find any case of presentation of

the back or loins recorded; and Dr. Lee has informed me that he has never met with an undoubted case of back presentation in his large experience.

Dr. Churchill places this presentation in his second division, but does not speak of having met with a case of the kind. Dr. Ramsbotham says it may occur, but is very uncommon. Dr. Blundell mentions only its possibility. Dr. Burns regards it merely as a stage of breech presentation that would terminate in that manner if left to nature. The narrative of the above case proves that such is not necessarily the course of this position. Although averse to meddling midwifery, and having almost unlimited faith in the powers of nature, yet I conceive that I should not have done my duty to my patient, had I left her to wait until nature changed the position of her child.

I am indebted to my friend, Dr. Allan, of Islington, for the opportunity, in his extensive library of obstetric works, of examining numerous French authors, and have found mention of back presentations in several. Thus Madame de Boivin* describes four positions of dorsal presentations:—1. The head placed anteriorly above the pubes, the back posteriorly; the left side of the infant corresponding to the right side of the mother: the nates and feet being in the fundus uteri. 2. The head placed posteriorly, the pelvis anteriorly; the right side of the child corresponding to the right side of the mother. 3. The head to the right side. 4. The head to the left side. The directions given by Madame de Boivin for the management of these cases are most minute, but appear more theoretical than experimental. Madame de Boivin mentions its occurrence six times in upwards of twenty thousand cases in her practice and at the *Maternité*.

Baudeloque† also describes four positions of back presentations, and gives very precise instructions for the conduct of the labour, but does not state that he had ever met with the presentation.

Velpeau‡ likewise refines on the management of this form of preternatural labour, and also gives an engraving thereof, but does not state that the case had occurred to him.

Holloway, July 1850.

* *Memorial de l'Art des Accouchemens.*

† *L'Art des Accouchemens*, t. i. p. 630.

‡ *L'Art des Accouchemens.*

MEDICAL GAZETTE.

FRIDAY, SEPTEMBER 6, 1850.

SOME months since we called attention to a case of alleged poisoning, which occurred in the County of Surrey, in which the magistrates refused to make any allowance for a chemical investigation, beyond the fee awarded by the Medical Witnesses' Act, and which we need hardly say, is wholly inadequate for researches that may occupy two or three days. The facts as they have been communicated to us are as follows:—A young woman died very suddenly without any previous illness. No poison was found, and poisoning was not even suspected, as she was living on good terms with her relations, and there was no apparent motive for self-destruction. A fee of six guineas was demanded for the chemical analysis—the coroner refused to grant more than two guineas: but he could find no one to undertake it upon these terms. In the end the analysis was made, after a week's delay; and it was then distinctly proved that the deceased had died from arsenic. But for the analysis, the verdict would probably have been "death from natural causes," since some of the chief symptoms of poisoning by arsenic were absent.

This, then, was a case in which a proper chemical investigation was really required. The answer to an application made by the coroner to the magistrates of Surrey for a special fee, is subjoined:—

"DEAR SIR,—I am directed by the magistrates to express to you their regret that they do not possess the power, under any act of Parliament, to make such an order. Under the existing statutes the magistrates have no original power either to order payment, or to determine the amount of fees to Medical Witnesses, but that power is

vested in the coroner both by stat. 6 and 7 W. IV., c. 89, and 1 Vict. c. 68, and the only power which the magistrates possess in this respect, is to order payment out of the county rate of the fees advanced by the coroner according to the scales established by the first of these acts.—I am, dear sir,

"Yours truly,

"WORONZOW GREIG,

"Clerk of the Peace.

"To —, Esq.,
Coroner for Surrey."

June 27, 1850.

The result has been, that the Vicar and principal inhabitants of the village in which the death occurred, have raised the amount by subscription; but we ask—Is it right that the public justices of the country should rest upon so slender a foundation as this? Is it possible that secret murders by poison can be brought to light under such a system as this? It is said that in Surrey, deaths from poisoning are rarely or ever heard of. This can create no surprise, when it is known that the magistrates of the county positively refuse to pay the fair expenses of the investigations required; and thus hold out the strongest encouragement to the concealment of crime. It is unreasonable to suppose that the county coroners should pay these special expenses out of their own fees; and it cannot be expected that such investigations will be made gratuitously by persons competent to undertake them, or that the inhabitants of the Surrey villages will, as a general rule, tax themselves in order to pay charges which really concern the proper administration of public justice.

In the late mysterious death of a female at Clapham, the proceedings were arrested, because the coroner could not, and the magistrates of Surrey and the chief Commissioner of Police would not, issue an order for an investigation and analysis. Owing to this, the mystery of that extraordinary case, in which chloroform was suspected to have caused death, has never been cleared up.

We, however, deny the statements contained in the letter of the magistrate's clerk regarding their want of power to pay special fees to medical witnesses. Such fees are frequently paid by the magistrates of most other counties in England, except Surrey and Middlesex. An act of Parliament is certainly not required for the exercise of this power. The fees are a fair and necessary charge on the county rates, and if allowed by the magistrates at Quarter Sessions, there is no rate-payer that could reasonably demur to the propriety and necessity of such an allowance being made, or compel the Bench of Magistrates to refund the money so paid by them for the benefit of the county, and for the due administration of public justice. The truth is, they have the power to order these expenses, if they choose to exercise it, just as much as the magistrates of other counties, who have not the meanness to raise this technical difficulty.

In the meantime, the decision of the Surrey magistrates should teach all practitioners who undertake these researches, that the maximum fee which they have to expect, whatever time, trouble, and expense they may have devoted to the investigation, will be a payment of two guineas! The evil would speedily be remedied if medical practitioners, unused to such researches, declined to undertake them.

Since writing the above, we find that this false economy regarding the payment of fees to medical practitioners at coroners' inquests has been strongly denounced from the judicial bench, although the censure of the learned judge was directed against the coroner, when the county magistrates were really the parties who had thrown an impediment in the way of public justice.

A man was tried at Wells before Mr Justice Coleridge on a charge of wilful

murder by cutting the throat of a person. An inquest was held, but the coroner would not allow any medical investigation to be made, and the jury came to no satisfactory verdict. On a private examination of the body, it was pretty clear from the nature and situation of the cuts in the throat, as well as the presence of other injuries, that the deceased had been the victim of homicidal violence. The prisoners were acquitted. We quote the following remarks from the report of the trial.

"Mr. Justice COLEBRIDGE said, he did not know the name of the coroner who had held the inquest upon this body, but he was much wanting in his duty in not having called in medical testimony; because, if he had done so, it was impossible his jury could have separated without returning a verdict of wilful murder against some person unknown; and then persons' attention would have been directed to it, and probably a different result might have been arrived at; either the prisoner might not have been put upon his trial at all, or else further evidence might have been adduced against him. Either way, he thought the coroner extremely wrong; and if any coroner went upon a system of abstaining from calling medical evidence for the purpose of saving expence, or for other reasons, he might lay himself open to proceedings in the Court of Queen's Bench, and subject himself to extremely unpleasant consequences."

"In exoneration of the coroner it is stated by the reporter that the magistrates are so particular at the quarter sessions about the expenses of coroners, that these gentlemen are really afraid to call in medical or other aid. It had long been anticipated that in consequence of the difficulties thrown in the way of coroners by the magistrates with regard to the allowance of expenses, the administration of justice would be impeded; and now we have a glaring instance of it."

Having thus an exposition of the law as it applies to coroners, we should be glad to know whether county magistrates who refuse to indemnify coroners for the necessary expenses of inquests,

do not also expose themselves "to unpleasant consequences." At present they appear to set at defiance all rules of reason and justice.

References.

1. *State of the Lincoln Lunatic Asylum*, 1849.
2. *The Fifth Report of the Committee of Visitors of the County of Middlesex Lunatic Asylum, at Hanwell*. 1849.
3. *State of an Institution near York, called the Retreat, for persons afflicted with Disorders of the Mind. Fifty-third Report*. 1849.
4. *Report of the West Riding of Yorkshire Lunatic Asylum*. 1849.
5. *Second Report of the Somerset County Asylum, for Insane Paupers*. 1849.
6. *Report of the County Lunatic Asylum, Gloucester*. 1849.
7. *Medical Report of the Lunatic Asylum, Aberdeen*. 1849.
8. *Report of the Montrose Lunatic Asylum*. 1849.
9. *Report of the Pennsylvania Hospital for the Insane*. 1849.

We propose, in the first place, to bring these reports, *seriatim*, under the notice of our readers, pointing out the peculiar features which they may present; and in the second place, by constructing a few tables from the statistics they may contain, to exhibit, so far as they afford materials, the collective information which they may possess.

1. LINCOLN.—The directors of this asylum justly claim the honour of its having been the first institution in Great Britain in which the non-restraint system was efficiently carried into practice. To Mr. Hill, the former resident medical officer of Lincoln Asylum, the directors accord the merit of having introduced, and shown practically, the advantage of, a humane system of control and restraint which should supersede the barbarities of coercion-chains, handcuffs, strait-jackets, &c. &c.

The report of the Lincoln Asylum contains a series of judicious practical observations on the minor contrivances which experience has suggested, to meet

various inconveniences and difficulties which arise in the management of the insane. "The special difficulties of an asylum," this report observes, "are the proper department of the house-surgeon and matron, as the medical economy is that of the physician." The subjects of the "minor contrivances" here referred to, are—escape, violence, insomnolence, noisiness, breakage, refusal of food, perverted instinct, indecency of action and of language, irretention of the bladder and bowels, fits, suicide and sickness. The observations on these several topics abound with such valuable practical hints as must be of service to all entrusted with the management of insane patients, whether in asylums or under private care, as they are the fruits of much practical experience.

2. HANWELL.—"Cheerfully and gratefully THE COMMITTEE terminate their functions by presenting the Report of their Proceedings during the past year. They are cheerful as they commenced—have carried on—and now conclude—the fulfilment of their duties, deeply impressed with their interesting importance, and satisfied by their conviction that they have been usefully discharged."

"THE COMMITTEE, however, perceive that they have *few novel topics whereby to awaken any peculiar interest in the public mind.*" Nevertheless, "the COMMITTEE" flatter themselves that their report will afford information to the medical profession and to other asylums; and "that it will not be unwelcome to *inquiring philanthropists, or to those gentle spirits who have so often manifested an interest in a chority, which has won so much attention, and afforded an example of extensive and generous good.*"*

These gentlemen of "THE COMMITTEE," who honour their collective dignity by capital letters, confess, somewhat reluctantly we think, that they "have indeed experienced, on the part of the medical and superior officers, an intelligent, indefatigable, and kind-hearted assistance which they cannot too highly commend;" yet they throughout speak of their medical staff as quite secondary, and in a tone of patronization unbecoming their relative positions. The directors of the Lincoln Asylum remark that "the special difficulties of an asylum devolve

upon the Resident Surgeons and the Matrons;" but the directors of the Hanwell Asylum seem to be quite ignorant of the fact, that individually the members of their medical staff are capable of the superintendence of an asylum, and that their magisterial functions endow them with no special fitness for such an office. The reverse of this would seem, however, to be the opinion of the twenty-one magistrates of Middlesex, who compose "THE COMMITTEE," if we may judge from their own self-complacent report. This public document is one uninterrupted song of self-praise and self-gratulations. In the estimation of its authors, the entire success of the institution is derived from *their labours*; the completeness of the details of its several parts is attributable solely to *their* superintendence; the restoration of its inmates to health results solely from *their* superior management.

Under such impressions, however false, we are not surprised at the cheerful tone in which they commence their report. At the same time, from anything that we can discover therein, we see no reason to think that "our Asylum" will become "an ornament to the county, and an example to the land!" over and above all others, until, at all events, the reports of "THE COMMITTEE" consist less of *ad captandum* common-places, and extravagant self-laudation. This, however, is a perfection which cannot be attained until these gentlemen have learnt to place a proper value upon the services of their medical staff, and have discovered the true position of their medical officers.

We perceive from public advertisements of the committee of visitors of the new asylum at Colney Hatch, which is to contain 1000 patients, that the same system of medical degradation is to be perpetuated. For so large a number of patients, two resident medical officers are deemed sufficient. And why? Because the appointing magistrates regard themselves as more competent than medical officers to the immediate and constant superintendence of so large and complicated an institution.

Dr. Conolly states in his report, which is permitted to form merely an appendix, that although severe cases of diarrhoea occurred in the asylum, and although

* The italics are our own.

cholera had appeared at Brentford, about two miles distant from Hanwell, yet that owing, under Providence, to the precautions taken, no case of cholera occurred in the asylum.

The usual gratuitous course of clinical lectures was delivered by Dr. Conolly, with the assistance of Dr. Hitchman.

Dr. Conolly reiterates the assertion, that "the substitution of attendants' hands, and the selection of attendants merely on account of their physical strength, has never formed any part of the system of non-restraint as pursued at Hanwell." We have on former occasions expressed our views in relation to the non-restraint system; we shall here take occasion to make a few further remarks upon this subject.

Society undoubtedly owes a large debt of gratitude to those who have followed so perseveringly the example that was set first in France by Esquirol, and afterwards at Lincoln, by Mr. Hill. But, at the same time, society should not in return consent to be imposed upon by a name. The public is told, and believes, that the phrase *non-restraint* signifies the absolutely entire removal of every check upon the voluntary movements of the insane. Common sense, however, will not fail to recognise somewhat powerful means of *restraint* in the walls of asylums, their padded rooms, their seclusion-rooms, their numerous attendants, their padlocked garments, &c. &c. We do not say that these are not required, or are too severely employed; on the contrary, we emphatically assert that they are indispensable, and moreover that they are now used with humane and kind consideration; yet we cannot but regard it as a deceptive use of words, to call the present system of the management of the insane one of *total non-restraint*, instead of simply one of *humane restraint*. An examination of any one, or all of the present Reports will justify these remarks.

We agree with Dr. Conolly that the close of the first decennial period of "this great experiment" would have been a fitting occasion for the managers of this asylum to have given a detailed and explicit public statement of its results. We suppose that the performance of this duty would have involved the performance of another—viz., the confession that to others than themselves,

is due nearly all the credit that may attach thereto. We cannot but mark the striking difference in the expression of two parties here concerned. "THE COMMITTEE" say, we have done this, we have done that, we have done all; but Dr. Conolly, whose wise and judicious superintendence controlled and directed the whole machinery, modestly says—"My inward thought, in all the steps of the attempt which it has been my privilege to make, has still ever been, *Quia fuisti adiutor meus*!"

3. The report of The FRIENDS' RETREAT, near York, may afford interest to its supporters. It is a short, business-like affair. What statistical information it may afford will be found below.

4. The principal and almost the only fact conveyed in the Report of the WEST RIDING OF YORKSHIRE Pauper Lunatic Asylum, is a grave and melancholy fact—the appearance of cholera in the institution, and its spread to a serious extent. The medical officers report it to have been communicated to their inmates from an adjoining workhouse. "The first alarm as to the presence of cholera within the asylum, was excited by the illness of a female brought in on the 17th September, from Gomersale Workhouse, where it was stated two persons had died of cholera on the previous night." From this patient the disease spread to others in the same gallery and room, and to the attendants. Subsequently, other cases occurred simultaneously, in all parts of the institution, the origin of which was not so distinctly traceable to contagion. The whole number of deaths during the month of October was eighty-five! The report states, somewhat pointedly, that one of the days of highest mortality was "the day of the Bishop's visit to the asylum." The total number of patients was six hundred and thirty-three. This report is singularly deficient in statistical information.

5. SOMERSET COUNTY.—This report is drawn up by the superintendent, Dr. Boyd, and presented to the Committee of visitors, who in Somerset seem to be wiser as to knowing their own position, than their fellows in other general features of the report in this asylum.

system now almost universally adopted, that of substituting moral for brute force in the control of the insane.

Dr. Boyd takes the opportunity of this report, to call the attention of pathologists to the dependence of general paralysis on "softening and other results of inflammation of the spinal marrow, and frequently of the ventricles and membranes at the base of the brain." Dr. Boyd states that he has not found this observation in any British writer: we may, however, refer him to our notice of Dr. Winn's treatise on General Paralysis, in which the same opinion is expressed. We quote the following:—"With regard to the pathological appearances, we observe that they present all the varieties and contraries found in the crania of the insane, in apoplexy, and in paralysis, with this one additional feature, that the results of inflammatory action were more frequently found than in simple insanity." (*M&D. GAZ.*, N. S., Vol. viii. p. 861.)

6. GLOUCESTER.—In this asylum, also, the "non-restraint system" is reported to have had full trial. Here, also, a truly charitable plan is about to be carried into effect—viz., provision for the gratuitous admission, in the earliest stage of the disease, of a limited number of those whose position, just above parochial aid, precludes their admission as county patients, but whose means are inadequate to the payments required for maintenance in the subscribers' "part of the establishment." We shall rejoice in the completion of this good intention.

7. ABERDEEN.—It does not appear from the report that the "non-restraint system" is regarded by the officers of the Aberdeen Asylum as the *ae plus ultra* of the management of insane patients. No direct mention of this system is made; but occupation, amusement, and the granting a considerable degree of personal liberty, are among the principal means of cure that have been here adopted.

The report contains carefully drawn up tables, which illustrate various points in the history of insanity. These will be noticed further on.

8. MONTROSE.—We feel called upon to object strongly to one proposition contained in this report—that is, the

employment of chloroform in order to overcome the refusal of food.

Dr. McGavin relates a case in which it was attended with success, and was not followed by any ill effects. This, however, we may suggest, is a better result than can be looked for in every instance. In the case given, "the patient was plied with chloroform, and while partially under its influence he eat (ate) and drank everything put before him, laughing and cracking jokes the whole time." The experiment, we may observe, might have resulted in no laughing matter. We are informed by a dentist in large practice in London, that whenever he has seen any ill consequences follow the use of chloroform, it has been when administered on a full stomach, and this is strictly in accordance with the experience of others. We mention this by way of caution in reference to the use of chloroform for compulsory feeding.

9. PENNSYLVANIA.—This hospital enjoys a considerable celebrity in the United States. We perceive that the treatment here consists principally in the employment of moral influence, combined with so much physical exertion as shall effectually maintain bodily health, while diverting the mind from dwelling upon its own morbid impressions. We find workshops, out-door labour, a museum, a reading-room, lectures on natural sciences, and musical entertainments, among the means employed there, as in asylums on this side the Atlantic.

The following are the only tables which we have been able to construct from the statistical observations contained in these various reports. The deficiencies under this head constitute a very great defect in the majority of the reports. Much light might be thrown upon many points in the history of insanity, if the materials placed at the disposal of the medical officers of asylums were not permitted to be entirely lost: this we are compelled to say is the case in most of the reports now before us. The omissions will be evident from a glance at the tables which we have drawn up, particularly with reference to the forms of the disease, its causes, duration, pathological appearances, influence of rank, occupation, &c. &c.

TABLE 1.

Showing the Number of Admissions, Recoveries, and Deaths, with the whole Numbers and Proportions of the Sexes, in the several Asylums.

1849.	Admissions.		Cures.		Deaths.		Whole number remaining.	
	Males.	Fem.	Males.	Fem.	Males.	Fem.	Males.	Fem.
Lincoln . . .	29	27	23*		9*		71	67
Hanwell . . .	67	48	10	7	8	5	409	554
Friends' Retreat	9	11	3	7	2	5	43	67
West Riding of Yorkshire . .	234*		106*		106*		303	327
Somerset . . .	68	80	13	9	14	7	130	156
Gloucester . .	49	62	13	34	18	5	186	160
Aberdeen . . .	35	47	12	35	10	7	119	107
Montrose . . .	17	23	7	7	7	4	69	73
Pennsylvania .	1599†		737†		149†		118	103

TABLE 2.

Principal Forms of the Disease.

	Hanwell.		Somerset.		Gloucester.‡		Aberdeen.		Pennsylvania.	
	Male	Fem.	Male	Fem.	Male	Fem.	Male	Fem.	Male	Fem.
Mania	44	28	33	46	25	31	20	28	429	373
Melancholia	10	12	4	15	5	17	5	8	181	181
Imbecility and idiocy	6	5	3	3	7	6	2	1		
Dementia	2	2	6	6	60	80	5	1	132	68
Epileptics	9	9	4	2	15	11				
General Paralysis	5	5	5	2	14	3				
Monomania			9	4	2	2	3	8	140	88

TABLE 3.

	Moral.		Physical.		Hereditary.		Unascertained.	
	Males.	Fem.	Males.	Fem.	Males.	Fem.	Males.	Fem.
Hanwell	23	14	30	17	10	3	23	19
Somerset	5	9	10	10	2	3	3	
Gloucester	20	23	14	25	17	21	15	16
Aberdeen	8	12	18	30	15	14	2	3
Pennsylvania§	173	204	332	219			367	297

* The proportion of the sexes not distinguished in the report.

† Proportion of sexes not distinguished. The numbers here given include all admissions since the opening of the hospital, the date of which event is not given.

‡ The numbers extracted from the Report of the Gloucester Asylum are from the whole number remaining at the end of the year, and not from the admission only, as in the other reports.

§ As before mentioned, the period over which these numbers extend is not specified in the report.

TABLE 4.

Duration of the Disease at the time of Admission.

	Under 3 months.		Under 6 months.		Under 1 year.		Under 2 years.		Under 5 years.		Under 10 years.		Under 20 years.	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
Hanwell	12	11	8	10	5	7	9	10	7	3	2	1	1	1
Somerset	15	20	0	2	1	0	1	1	1	0				
Gloucester	12	20	0	5	1	0								
Aberdeen	12	14			18	24	5	9						
* Pennsylvania	464	346	66	60	100	79	112	72	65	62	28	29	27	40

TABLE 5.

Causes of Death.†

	Lincoln	Hanwell.		Somerset.		Aberdeen.	
		Males.	Fem.	Males.	Fem.	Males.	Fem.
Apoplexy	18	3	2	2	2	2	0
Diseases of the lungs	30	5	0	13	4	4	2
Diseases of the brain	24	16	4	13	6	1	0
Other visceral diseases	56	7	5	6	5	1	1
Erysipelas	5						
Epilepsy	14	2	2				
Fever	19	1	1	1	1		
Exhaustion and old age	83	2	7			2	4

TABLE 6.

Showing the Cost per head for the Maintenance of Pauper Patients.

	Per year.			Per week.		Per day.	
	£	s.	d.	s.	d.	s.	d.
Lincoln	27	19	2½	10	8½	1	6½
Hanwell	22	15	0	8	9	1	3
Somerset	22	15	0	8	9	1	3

We have in hand several other Reports of Asylums for the current year. We shall notice these in a separate article.

Familiar Views of Lunacy and Lunatic Life; with Hints on the Personal Care and Management of those who are afflicted with temporary or permanent Mental Derangement. By the late Superintendent of an Asylum for the Insane. Small 8vo. pp. 195. London: Parker. 1850.

This volume is addressed to the general reader, the author supposing "that a work in which all medical and other offensive detail should be entirely

omitted, might find favour with such readers as wish to indulge a rational curiosity regarding the past and present state of knowledge on the subject of intellectual disorder, and the institutions and remedies which have from time to time been adapted to the purpose of its amendment, and yet are unprepared to encounter reports and session-papers, or prohibited the use of such books as are too exclusively of a professional character."

* Out of 1899 patients.

† The immediate cause of death is specified in these four only among the reports.

We should have judged the subject to be one certainly difficult to popularise, and that the work in which it is thus brought to the level of the comprehension of the general reader must necessarily have but a limited circulation; that is to say, that it will be confined to the perusal of those who may unfortunately become directly concerned with mental disease in their families or friends. To this class of readers the volume before us affords much useful instruction, and conveys some judicious advice; while to others not so immediately interested in the subject, it will at the same time furnish much interesting historical information.

Proceedings of Societies.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Edinburgh, August 1.

Some Observations on the Growth of Plants in Abnormal Atmospheres. By DR. J. H. GLADSTONE and MR. G. GLAD- STONE.

ACCORDING to the authors of this paper, while oxygen is the most important constituent of the atmosphere, so far as animal life is concerned, it is upon the carbonic acid, ammonia, and aqueous vapour that the vegetable world is supremely dependent. The question arises, Do the oxygen and nitrogen of the air play no important part in the process of vegetation? The following preliminary experiments, with a view to the solution of this and similar inquiries, were detailed by the authors:—A pansy lived for the length of twenty-four days in an atmosphere of hydrogen, containing 5 per cent. of carbonic acid; one similarly placed in an atmosphere of common air remained healthy for a longer period. Five onions, just commencing to sprout, were severally placed in carbonic acid, carbonic oxide, coal-gas, air containing 8 per cent. of light carburetted hydrogen, and ordinary atmospheric air. The germination of the first two was entirely stopped, while the hydrocarbons appeared considerably to accelerate the growth of the vegetable. The plants in each instance lost weight. A pansy in flower, a young stock, and a grass plant, were placed in pure nitrogen gas. The former two soon died; but the grass was

still growing a month after the commencement of the experiment. Another pansy was placed in a mixture of oxygen and hydrogen gases in the proportions requisite to form water. In order to imitate the balance that obtains in nature between animal and vegetable life, some flies were introduced, along with some sugar, to serve as their food. The experiment was commenced a fortnight since, and the plant, when last observed, was in good condition. Owing to the low specific gravity of the mixed gases, the flies were unable to mount on the wing, or make the usual buzzing noise; but the substitution of hydrogen for nitrogen in the atmosphere had no marked effect upon their breathing; thus confirming the observations of M. Regnault by an instance drawn from the Articulate.

On the Air and Water in Towns, and the Action of Pansy Strata on Water and Organic Matter. By DR. R. A. SMITH.

IT is a matter of great importance to find from what source it is best to obtain water for large towns, and how it is to be collected. To these points Dr. Smith particularly directs attention. Regarding the conditions of many springs, which never become muddy, but possess a constant brilliancy and a very equal temperature at all seasons of the year, the author thinks that there is a purifying and cooling action going on beneath. The surface water from the same place, even if filtered, has not the same brilliancy; it has not the same freedom from organic matter, neither is it equally charged with carbonic acid or oxygen gas: there are other influences, therefore, at work. The rain which falls has not the purity, although it comes directly from the clouds; it may even be wanting in cleanness, as is often the case. Springs rise through a great extent of soil, and collect a considerable amount of inorganic salts; and it is shown by Dr. Smith that their purity is due entirely to the power of the soil to separate all organic matter, and at the same time to compel the mixture of carbonic acid and oxygen. The amount of organic matter removed in this way is surprising, and it is a most important and valuable property of the soil. The change even takes place close to cesspools and sewers: at a very short distance from the most offensive organic matter there may be found water having little or none in it. As an agent for purifying towns, this oxidation of organic matter is the most extraordinary, and we find the soil of towns which have been inhabited for centuries still possessing this remarkable power. St. Paul's Churchyard may be looked upon as one of the oldest parts of London; and the water from the wells around it is remark-

ably pure, and the drainage of the soil is such that there is very little, if any, salts of nitric acid in it. If the soil, says Dr. Smith, has such a power to decompose by oxidation, we want to know how it gets so much of its oxygen. We must, however, look to the air as the only source, and see how it can come from it. When water becomes deprived of oxygen, it very soon takes it up again, as may be proved by experiment. This shows us that, as fast as the oxygen is consumed by the organic matter, it receives a fresh portion, conveyed to it by the porous soil. Several experiments of the following character were given, to show the filtering power of the soil:—A solution of peaty matter was made in ammonia: the solution was very dark, so that some colour was perceived through a film of only the twentieth of an inch in thickness. This was filtered through sand, and came out perfectly clear and colourless. Organic matter dissolved in oil of vitriol was separated from it by a thickness of stratum of only four inches. A bottle of porter was by the same process deprived of all its colour. The material of which this filter is made is of little importance,—one of the best, according to Dr. Smith, as far as clearing the water is concerned, being of steel filings; oxide of iron, oxide of manganese, and powdered bricks all answering equally well. This shows that the separation of the organic matter is due to some peculiar attraction of the surfaces of the porous mass presented to the fluid. This paper was a continuation of Dr. Smith's report published last year, and he purposes continuing the inquiry.

On the relative values of the Dietsaries in use by different classes of the Population.
By DR. LYON PLAYFAIR.

DR. PLAYFAIR has been engaged for some time in an examination of the dietaries adopted in the union houses, schools, and other great establishments in this country, the object of the inquiry being the determination of the most nutritious diet. The result of this inquiry has proved that no system of any value has been adopted by any of the boards controlling our national schools and charities; and hence the high importance is shown of some accurate examination—such as that brought forward by Dr. Playfair—of the value, chemically and physiologically, of the dietaries adopted.

The reading of this paper gave rise to a discussion as to the merits of the hypothesis upon which the reductions of these dietary tables have been made; and it was argued that we had no distinct evidence to prove that nitrogen alone fairly represents the amount of nutrition contained in an article of diet.

On the Climate of the Valley of the Nile.
By MR. T. SPENCER WELLS, R.N.

THE observations extend from the 6th of December, 1840, to the 16th of March, 1850. The instruments were kept in a cabin in the boat of an invalid. The cabin was six feet high, twelve feet broad, and ten feet deep. Its floor was from one to two feet above the level of the river. The dry and wet bulb thermometers, and the barometer, were fixed to a beam in the centre of the cabin, where they were not exposed either to the direct or reflected rays of the sun. There were six glass windows to the cabin, provided with open blinds. Some of these windows were always open during the day, so that the morning and afternoon observations may be considered to represent the temperature of the open air in the shade. Sometimes a window was open until after the evening observations, but more frequently this was not the case; and to this I ascribe the fact, that the mean of the evening observations is above that of the morning. A register night thermometer was fixed outside one of the windows, and the lowest temperature observed each day is recorded. These daily observations were made at the hours of 9 A.M., 3 P.M., and 11 P.M.; and an abstract of these daily observations was exhibited.

The mean temperature of the air for the period of my observations at Greenwich was $39^{\circ} 3'$; on the Nile it was 61° . Thus there was a difference of 22 degrees in the mean temperature of Egypt over that of Greenwich during these months.

The mean temperature of evaporation at Greenwich was $37^{\circ} 4'$, in Egypt 55° ; being 18 degrees above the mean at Greenwich for the same period.

The mean temperature of the dew point at Greenwich was $84^{\circ} 1'$, in Egypt $50^{\circ} 8'$. Thus in England the air was saturated by the quantity of vapour contained in it at a temperature 16 degrees below that at which saturation occurred in Egypt.

The mean elastic force of vapour in Egypt was 0.384, at Greenwich 0.214. In other words, the pressure of the watery vapour mixed with the air was capable of supporting a column of mercury higher by $\frac{1}{10}$ of an inch in Egypt than in England.

The mean weight of water in a cubic foot of air in England was 3 grains, in Egypt 4 grains and $\frac{1}{10}$; but still, owing to the higher temperature, the air was much drier in Egypt. When the temperature of the air is considerably above that of the dew point, the air is dry, dissolving or absorbing aqueous vapour without any tendency to precipitation in the form of rain, and it is dry in proportion to the difference between the two temperatures. Thus, although the mean weight of water in a cubic

foot of air was greater last winter in Egypt than in England, yet the air was much more nearly saturated with moisture in England than in Egypt. At Greenwich the mean additional weight of water required to saturate a cubic foot of air was only $\frac{1}{10}$ of a grain, while on the Nile it was 1 grain and $\frac{3}{4}$. If we represent air completely deprived of moisture by zero, and air completely saturated as unity, the mean degree of humidity on the Nile was 75 per cent., while at Greenwich it was 85 per cent.

The mean readings of the barometer in the two countries very nearly approach each other; in Egypt being 29.99, at Greenwich 29.87. A glance at the table, however, showed how very small the extreme range of the instrument was on the Nile.

The average weight of a cubic foot of air at Greenwich was 549 grains, in Egypt 527 grains.

Rain fell in various districts of England on averages from 31 to 61 days, while in Egypt it only fell on 5 days, and on three of these a shower was of but a few minutes' duration. On two days rain fell heavily at Cairo for several hours.

The mean daily range of the temperature of the air at Greenwich was 11.37, in Egypt 10.31: but, while the mean extreme range in Egypt was 38, at Greenwich it was but 29; the mean extreme range in the cabin being only 7 degrees below that on the grass at Greenwich in the open air.

Fog was occasionally, but rarely, observed. It was general in the Delta in the early morning; but above Cairo was only observed on three occasions.

ACADEMY OF MEDICINE, PARIS.

Aug. 20, 1850.

Accident from Chloroform.

M. DELABUE stated, that having occasion to make extensive incisions in the thigh of a woman, in order to lay open some fistulous tracts, he had previously administered five grammes (= 75 grains) of chloroform on a sponge, during five minutes. When about to operate he noticed the patient's head fall towards her right shoulder, her body incline forwards, and a sanguinolent froth issue from her mouth, respiration and pulse being scarcely detectable. By repeated slaps on the chest, cold water applied to the head, sinapisms to the feet, and irritation of the fauces, movements of the eyelids were produced, followed by free vomiting. After complete restoration, a haggard look of the eye and extreme loquacity continued for about a quarter of an hour.

Iodated Oil a Substitute for Cod-liver Oil.

M. PERSONNE observed that, as the medicinal properties of cod-liver oil depend upon a combination of iodine and fatty matter, it had appeared to him more rational to employ an oil so prepared as to contain always a known quantity of iodine, in an uniform state of combination.

Black Discolouration of the Countenance.

M. BOUSQUET related two cases, one of which consisted of a form of purpura hæmorrhagica; in the second, a blue tint appeared about the eyelids, ears, and neck, which could be washed off, but returned after a time. The blue tint subsequently changed to black. This patient had been the subject of a convulsive cough, which had disappeared and been replaced, simultaneously with the appearance of the discolouration, by a severe headache. The latter case was regarded as analogous to the instances that are recorded of sanguineous perspiration.

SURGICAL SOCIETY OF PARIS.

Aug. 21, 1850.

MR. COLSON transmitted to the Society a voluminous memoir on the malformations of the anus and rectum. The secretary was requested to prepare an analysis of this work.

Double Lachrymal Tumor.

M. AUZIAS-TURENNE gave a description of the pathological changes observed in the dissection of a case of double lachrymal tumor. The lachrymal orifices and ducts were free from disease, but considerable thickening of the mucous membrane of the sac was observed. The nasal canal was free at its upper part, but was completely obliterated below. The seat of the obstruction had presented an obstacle to a cure.

ON THE EMPLOYMENT OF NITRATE OF SILVER IN INFANTILE ENTERITIS.

M. DUCLOS has found the nitrate of silver, administered in enemata, serviceable in acute or chronic colitis in very young infants, and its administration unattended with danger. It is equally beneficial in acute or chronic dysentery. Administered by the mouth and in enemata at the same time, it has exercised a favourable influence on the vomiting and diarrhoea of choleraic enteritis of infants. In short, M. Duclos states that he has obtained the best results from the exhibition of this medicine in acute inflammation of the mucous membrane throughout the whole length of the alimentary canal.—*L'Union Médicale*. x

Hospital and Infirmary Reports.

FELLOWS' PRIZE ESSAY.

Summer Session 1845.

By C. H. F. ROUTE, M.D. Lond.

QUESTION PROPOSED—*Are There any conditions, and if so, what are they, in which an excess of urea is contained in*

the whole quantity of urine passed in 24 hours?

PART I.—The answer of the above question depends essentially upon that of the two following.

1st. What is the average density and quantity of urine excreted in health, taking the whole quantity passed in 24 hours? 2nd. What is the normal proportion of urea contained in this quantity of urine?

1st. In relation to the quantity and density of urine in the normal state, it is remarkable what a discrepancy exists amongst authors. [This will be at once obvious by reference to the subjoined table.

	Sp. gr.		No. of oz. passed in 24 hours.
Dr. Thomson	1010		
" Prout	1020		80 to 40
" Willis	1010 children	1015 adults	
" Venables	1012 to 1017		
" McGregor	1017		
" Christison	1029		30 to 50
" J. Gregory Secundus	1025		
" Golding Bird			20 to 48
" Becquerel	1018 men	1015 women	17 to 73
" Lecana	1025		19 to 47
" Rayer			21 to 57

And yet these averages may, after all, be quite correct. For, without doubt, in health the urine is liable to great variations. First, it will vary usually in density according to the time of the day in which it has been passed: hence the divisions into *urine potus*, *sanguinis*, and *chylis*. Then the quantity will be increased by the amount of fluid drunk,—frequently by the diuretic effect of medicines or mental emotions,—by disease, as in certain varieties of diabetes. So other diseases and medicines may have an opposite effect. The same causes will also affect its density. Some alcoholic liquors highly nutritious, and especially animal or azotized food, will raise the density. Low diet, and vegetable diet, will lower it. It is to most of these contingencies, therefore, that we must ascribe the discrepancy before remarked.

It is not purposed, therefore, to enter into a controversy as to which of the above-named authors is most correct. These opinions, if questioned at all, will only be canvassed where this is necessary to justify our own, if opposite conclusions: for we cannot admit the correctness of Dr. Christison's opinion (*Library of Med.*), who states "that the determination of the characters of average healthy urine is of little value in physiology, and of no importance in relation to medical practice." On the contrary, we believe that to do this is of paramount importance, as, by the very

means of comparison it holds out, it often enables us, in many instances, to diagnose renal or other disease, where other morbid symptoms are either entirely absent, or doubtful in kind. The truth of this assertion we trust has been already shown in the Fellows' Cases already published.

The average given by Dr. Christison is valuable as an example of the high density to which, even in ordinary instances, the urine of a healthy man may attain. The case was that of an ordinary male in the middle ranks of life, and enjoying ordinary diet,—i. e. from 24 to 30 oz. of solid food daily; rather sparing in the use of liquids, and abstaining for the time from wine and the stronger alcoholic liquors. This man used to pass about 35 oz. of urine, of a sp. gr. of 1029, in the 24 hours; and this average was taken from the whole quantity of urine passed in 7 continuous days. Dr. J. Gregory gives, as the extremes for adult and middle age, 1005 and 1033. The greatest range for the same individual, 21°. The ordinary range between 1816 and 1031, and the mean of 363 experiments on 50 individuals, 1022.5. Remark upon these experiments (*Lib. of Med.*), Dr. Christison states that this average is rather low than high, because he knows that many of the experiments were made soon after breakfast, and comparatively few after dinner; and accordingly the average of 5 individuals whose urine was tried between 20

and 50 times each, was 1025.2; and this last number he supposes would indicate the average.

There appears, however, to us to be another objection to make to this conclusion. To take an average density from specimens collected at different times of the day, and that even when the parties voiding them are under the same circumstances of diet, so that the urines generally must have the same character, is unsafe; for it is found, by actual experiment, that urines so selected

are of very different specific gravities. It has been too generally supposed that the morning urine is usually of a higher specific gravity than the evening. This, doubtless, is often the case, but it is by no means even the general rule. This may be shown by the following table of densities indifferently selected out of several hundred cases, with a view to determine this point. The cases occurred in University College Hospital.

1st. When the morning urine was highest.

No.	Name.	Sp. g. A.M.	Sp. g. P.M.	No.	Name.	Sp. g. A.M.	Sp. g. P.M.	Mean.
1	G. Jones 1.	1023	1020	8	Federmann	1030	1025	A.M. 1023. P.M. 1018.2.
2	Body . .	1021	1016	9	Bell . . .	1025	1019	
3	Dunn . .	1026	1023	10	G. Jones 2.	1035	1030	
4	Plummer .	1020	1010	11	Green . .	1018	1015	
5	Whitehead.	1020	1017	12	Nicholson .	1018	1011	
6	Knight . .	1021	1010	13	Griffiths .	1021	1019	
7	Lee . . .	1033	1030	14	Collier . .	1012	1010	

2nd. When the evening urine was highest.

No.	Name.	Sp. g. A.M.	Sp. g. P.M.	No.	Name.	Sp. g. A.M.	Sp. g. P.M.	Mean.
1	G. Jones 1.	1018	1024	8	Plummer .	1012	1024	A.M. 1021.0. P.M. 1026.7.
2	Stockley .	1020	1026	9	Lee . . .	1024	1031	
3	G. Jones 1.	1018	1023	10	Hill . . .	1018	1027	
4	G. Jones 2.	1023	1033	11	Nicholson .	1023	1027	
5	Plummer .	1021	1024	12	Collins . .	1013	1015	
6	Hayden . .	1028	1031	13	G. Jones 2.	1027	1032	
7	Taylor . .	1028	1031					

Sometimes, however, it should be admitted that the morning and evening urine have the same specific gravity: in twelve cases so collected the average sp. gr. was 1024.6.

Bequerel fixes the specific gravity of healthy urine at 1017; men, 1018; women, 1015. This opinion appears to have been deduced from well-founded experiments. But all our English writers appear to have overlooked one source of fallacy in these averages, as applied to the English. The latter live higher, more exclusively on animal and alcoholic liquors. Moreover, *without any doubt*, the English Hospital diet is far superior to that given in the French Hospitals: necessarily, therefore, the sp. gr. of the urine of the English will be higher, on the average, than that of the French.

Dr. G. Bird, with a view of obtaining the sp. gr. of urine of 24 hours, recommends, where the sample itself of the 24 hours cannot be obtained, the collection of a specimen of the *urina hyali*, or that passed immediately before going to bed, as also of a specimen of the *urina sanguinis*, or that passed in the morning, to be taken,—the

average specific gravity of these two specimens giving a near approach to the sp. gr. of a specimen of the whole urine passed in 24 hours. This is doubtless true, or very nearly so, for many cases. It is also true for the mean of a large number of cases: but it is sometimes very incorrect for a large number of individual cases.

Before, however, proving this assertion by figures, it may be well to explain the manner in which we operated on the specimens of urine ourselves.

One of the chief difficulties we had to contend with was the obtaining samples of the whole quantity of urine passed in a period of 24 hours.

1st. To confide in the statements made by the patients themselves is often very fallacious. Some wilfully deceive, and others fancy they have made a good deal of water, because, in consequence of some ardor *urinae* present,—an enlarged prostate or other cause,—*they frequently micturate*; while in reality the quantity passed each time has been so small that the absolute quantity passed in the 24 hours is deficient.

2nd. When patients are labouring under

acute attacks, purgatives are commonly administered; and thus, passing most of their urine with their motions, they neither will nor can save the whole quantity of urine passed in 24 hours; besides, with females the presence of the catamenia is often another impediment.

3rd. Most patients have a natural disinclination to save the urine passed, from a natural feeling of delicacy, and require to be closely watched not to deceive in this respect. This objection applies especially to females. In No. 3 ward (female ward), this could never be effected; and at the most the measure was given in pints and half-pints. In the other wards

more facilities were afforded. The whole quantity here was saved and measured. In No. 4 ward, however, it was that we collected our more important data. Here the head nurse not only caused the whole quantity of the urine to be saved and measured, but the whole quantity of that passed in the morning, in the evening, and during the whole day, was always separately measured, and not trusted to memory, but carefully recorded on a slate, and separate samples of each set aside for examination. These experiments are therefore to be entirely relied upon.

The following table of examples indiscriminately selected from a large number

No.	Name.	Sp. gr. A.M.	Sp. gr. P.M.	Sp. gr. mean, by calculation.	Sp. gr. of 24 hours' urine by actual experiments
1	Stockley	1027	1027	1027	1025
2	Do.	1030	1032	1031	1025
3	Rouch	1025	1024	1024.5	1023.5
4	Stockley	1030	1027	1026.5	1027
5	Rouch	1030	1027	1026.5	1026
6	Stockley	1010	1020	1015	1024
7	Rigby	1012	1020	1016	1017
8	Rouch	1024	1023	1023.5	1025
9	Stockley	1020	1010	1015	1017
10	Do.	1030	1023	1026.5	1023
11	Do.	1035	1030	1032.5	1031
12	Rigby	1026	1031	1028.5	1027
13	Stockley	1026	1030	1028	1031
14	Pithers	1021	1027	1024	1026
15	Rigby	1025	1025	1025	1023
16	Allen	1026	1023	1024.5	1022
17	Stockley	1031	1035	1033	1029
18	Do.	1022	1020	1021	1023
19	Do.	1024	1018	1021	1024
20	Do.	1010	1022	1019	1024
21	Rouch	1025	1022	1023.5	1023
22	Stockley	1023	1020	1021.5	1021
23	Do.	1013	1017	1015	1021
24	Slight	1020	1017	1018.5	1019
25	Stockley	1023	1022	1022.5	1024
26	Do.	1020	1019	1019.5	1021
27	Grace	1020	1025	1022.5	1021
28	G. Jones 1	1020	1022	1021	1023
29	Do.	1020	1023	1021.5	1023
30	Dunn	1026	1023	1024	1026
31	G. Jones 1	1020	1022	1021	1023
32	Do.	1036	1031	1033.5	1030
33	Kirk	1030	1030	1030	1033
34	G. Jones 1	1031	1034	1032.5	1034
35	Do.	1025	1031	1028	1035
36	Plummer	1019	1008	1013.5	1010
37	Do.	1025	1012	1018.5	1021
38	G. Jones 1	1016	1021	1018.5	1021
39	Whitehead	1007	1010	1008.5	1010
40	Knight	1031	1031	1031	1032
41	Do.	1031.8	1008	1019.5	1011
42	Hill	1016	1027	1022.5	1025
43	Taylor	1028	1031	1029.5	1031
44	Mean	1023.6	1023.2	1023.4	1024.1

of cases, will serve to show that the mean density of morning and evening urine is not always the same as that of a sample of the whole quantity of urine passed in a period of 24 hours, though it is almost time for the average mean.

The explanation seems to be, that, when the density is high, a less quantity of urine is passed at the time, though even this is not invariably the case.

In endeavouring, therefore, to select an average specific gravity and density, we have adopted another plan—the only one, indeed, open to us. We took the average quantity and density of a number of convalescent patients at the hospital, in whom no certain indication of renal disease, especially Bright's, could be traced, their diet being at the time full or extra full. Thus a series of numbers were obtained, and the means obtained looked upon as the average densities of healthy urine.

In this manner eighteen examples were collected; each we divided into two series,—the first where the quantity of urine passed did not exceed 36 ounces,—the second where it did.

The following table is useful for reference, and will further explain the value of these numbers: it is based upon Dr. Day's table, only giving the quantity of solid matter contained in each ounce.

Sp. gr.	Solid matter in 1 oz.	Sp. gr.	Solid matter in 1 oz.	Sp. gr.	Solid matter in 1 oz.
1000	0	1014	15.87	1028	32.19
1001	1.11	1015	17.02	1029	33.37
1002	2.24	1016	18.18	1030	34.55
1003	3.36	1017	19.33	1031	35.76
1004	4.53	1018	20.29	1032	36.93
1005	5.61	1019	21.60	1033	37.72
1006	6.75	1020	22.81	1034	39.79
1007	7.88	1021	23.97	1035	40.51
1008	9.01	1022	25.84	1036	41.71
1009	10.00	1023	26.84	1037	42.91
1010	11.19	1024	27.88	1038	44.06
1011	12.43	1025	29.13	1039	44.48
1012	13.62	1026	29.83	1040	46.52
1013	14.72	1027	31.41		

Applying this table to the numbers above given, we have:—

Solid matters excreted in 24 hours.
grains.

Average for the whole 18 cases . . . 813.12
 " first 13 . . . 748.86
 " last 5 . . . 980.16
 " 2d, 5th, and 13th 1107.3
 " 14th, 16th, & 17th 1120.5
 " Dr. Christison's case 1167.9

These numbers represent nearly enough the limits of health. If due attention be, moreover, paid to the amount of ingesta taken,—and supposing it in all cases to have been sufficient in quantity,—it will afford an easy means very often of diagnosing renal disease where other symptoms are doubtful or obscure.

The same method of calculation cannot be applied to the case where we are seeking to find out the quantity of urea contained in a given quantity of urine. From Berzelius's analysis it would appear that in healthy urine the proportion of urea to the total amount of solid matter contained in the urine is as 1 to 2.23. Becquerel gives for males 1 to 2.43, females 1 to 2.49. These proportions, however, though apparently not very different one from another, are in reality very much so; as Christison makes the amount of solids in 1000 grains equal to the difference between 1000 and its density multiplied by 2.33, whereas Becquerel makes it equal to the difference between 1000 and its density multiplied by 1.65.

Again, urines of the same specific gravity are found by experiment to contain very different quantities of urea. Take,

Name.	Sp. gr.	Oz. in 24 hours	Diet at the time.
1 Nutt. . .	1017	32	Full diet.
2 Hill . . .	1030	32	—
3 Millsom . .	1020	25	—
4 Green . .	1025	28	—
5 Case of gentleman . .	1035	24	Extra full.
6 Tiedemann . .	1020	32	Full diet.
7 Hayden . .	1016	32	—
8 Nutt . . .	1017	30	—
9 Nicholson . .	1025	28	—
10 Loch . .	1018	30	—
11 Stiffe . .	1020	20	—
12 Pithers . .	1024	24	—
13 Stockley . .	1030	36	Ext. full.
Mean of the first series . .	1022.9	28.6	
14 G. Jones 1 . .	1016	56	Full diet.
15 Keye . .	1016	48	—
16 Pithers . .	1023	48	—
17 Whitehead . .	1015	62	—
18 Nicholson . .	1024	42	—
Mean of 2d series . . .	1016.8	51.2	
Mean of whole 18 cases . .	1021.1	34.9	
Mean of 2d, 5th, and 13th . .	1032	31	
Mean of 14th, 16th, & 17th . .	1018	55	

for instance, the following examples:—Becquerel found, in four experiments made upon urines of sp. gr. 1023, the following quantities of urea:—13·06, 10·72, 11·98, and 16·88. We ourselves, in three specimens of urine of sp. gr. 1026, 21·00, 20·64, and 16·00. This difference arises, in many instances, from the quantity of colouring matter contained in the urine. It is for this reason that Becquerel has attempted a division of urine into five varieties:—

1st. Cases in which the urine is very high coloured, of high specific gravity, very acid, and in general deficient in quantity. In these cases there is always more or less febrile movement, or some acute attack present.

2d. Cases in which the urine is pale, clear, of low specific gravity, little or not at all diminished in quantity, and in general passed by weak and anæmic persons.

3d. Cases in which the urine is dark, sufficiently changed, diminished in quantity, as in the first series, but with low specific gravity. This variety is usually passed by individuals weakened by the nature of disease present, or remedies given, differing, however, from the first series, inasmuch as there is febrile movement and functional disease.

4th. Analogous cases to the preceding, in which the amount of watery portions is less considerable.

5th. These cases differ little in appearance from those of the healthy urine. The patients are generally free from disease, but not affected with febrile or functional disorder.

The averages resulting from this arrangement may be expressed in the following table.—(See first table on next page.)

This arrangement, however nice, is yet too complicated for general application. In the first place, as a *general rule*, all urines of a pale colour are of a low specific gravity; whereas those of a dark have a high density. These two, therefore, belong to the same scale, the pale at the beginning, the dark at the end.

In the third and fourth series the cases are analogous to the two former, only there is more colouring matter; while in the fifth the approach is too near the normal state to require a separate division.

It is, however, sometimes of advantage to be able to know at once, even though only approximatively, the quantity of urea excreted in 24 hours. In order to effect this, we have adopted the following arrangement:—We make four divisions in the varieties of urine.

The first division includes the first and second series of Becquerel, and may be considered as subdivided into two parts—the first subdivision, in which the urine has a specific gravity not above 1015, are

also pale and clear, generally voided by anæmic and debilitated persons. The second subdivision, the urine of a specific gravity above 1015, frequently turbid from the presence of lithates, and voided by persons of a plethoric habit, or who are labouring under some febrile attack.

As opposed to these are what may be considered the extremes of both subdivisions; the first with an increase of colouring matter and less urea, the other with more urea and less colouring matter: while, lastly, we have affixed a table of what appears to be healthy urine, and as the quantity of urea would be affected by the quantity of liquid drunk being small or very great, in a healthy and well fed person.

This table has been brought out by a

Sp. gr.	Healthy urine.	Diseased urines.		
		1.	Coloured 2.	3.
1000	0	0	—	—
1001	·30	·26	—	—
1002	·61	·52	—	—
1003	·91	·79	—	—
1004	1·23	1·04	—	—
1005	1·64	1·44	·78	1·87
1006	2·06	1·78	·97	2·29
1007	2·48	2·08	1·15	2·71
1008	2·88	2·38	1·34	3·14
1009	3·29	2·84	1·52	3·57
1010	3·69	3·15	1·72	3·99
1011	4·10	3·44	1·88	4·44
1012	4·42	3·74	2·05	4·90
1013	4·74	4·10	2·21	5·35
1014	5·16	4·34	2·37	5·80
1015	5·55	4·65	2·53	6·25
1016	5·92	4·95	2·72	6·69
1017	6·29	5·25	2·91	6·93
1018	6·64	5·56	3·09	7·28
1019	6·94	5·90	3·28	7·62
1020	7·24	6·28	3·42	7·96
1021	7·54	6·72	3·64	8·57
1022	7·84	7·10	3·86	9·08
1023	8·32	7·49	4·09	9·59
1024	8·89	7·88	4·31	10·00
1025	9·47	8·30	4·53	10·52
1026	10·05	8·64	—	11·00
1027	10·63	9·05	—	11·48
1028	11·13	9·42	—	11·96
1029	11·64	9·81	—	12·44
1030	12·15	10·18	—	12·92
1031	12·61	10·57	—	13·39
1032	13·09	10·96	—	13·87
1033	13·56	11·36	—	14·34
1034	13·92	11·69	—	14·83
1035	14·28	12·08	—	15·30
1036	14·64	12·41	—	15·72
1037	15·01	12·70	—	16·12
1038	15·37	13·08	—	16·54
1039	15·73	13·43	—	16·96
1040	16·10	13·76	—	17·43

series of calculations too tedious to be enumerated, but founded on Becquerel's and our own analyses of healthy and dis-

eased urines; the two, when reduced to a common unit, being found generally to agree very well.

[Table referred to in preceding page.]

	Sp. gr.	No. of oz. in 24 hours.	Urea in 1 oz. grains.	Urea to 1000 parts urine. grains.
1st series, founded on 11 cases	1021.9	19.7	4.16	9.00
2d " " 11 "	1011.8	35.1	3.37	7.03
3d " " 7 "	1014.8	18.4	2.36	4.91
4th " " 5 "	1010.5	40.1	4.27	8.91
5th " " 3 "	1017.7	30.3	5.93	12.35

The largest and smallest quantity of urea upon the average is stated by Becquerel to be in health—

	Sp. gr.	Grains in 1 oz.	Whole quantity excreted in 24 hours.
Males	1018.9	6.64	260.81
Females	1015.1	4.97	255.53
Mean	1017.01	5.80	258.17

Whence he infers that the normal quantities vary usually from 4.80 to 6.72 grains for every ounce of urine, and the quantity excreted in 24 hours varies from 231.60 to 277.92.

These numbers do not quite agree with those obtained by Lecanu from a total of 126 cases: he found that the quantity of urea voided was in 24 hours—

	Minimum. Grains.	Mean. Grains.	Maximum. Grains.
By men	357.33	433.81	510.29
" women	147.66	292.31	436.95
" old men (84 to 86)	60.98	178.01	295.05
" children 8 years	162.64	208.31	254.14
" " 4 years	57.28	69.50	81.10

The experiment, moreover, of Lehmann, of Leipsic, showed that in his case he could greatly increase the urea by animal diet, so as to exceed Lecanu's average:—

	Urea in 24 hours.
By animal diet	819.2
" vegetable do.	346.0
" mixed do.	500.5

Our own experiments, with the same specific gravities as before, give:—

Sp. gr.	Ounces.	Urea in 24 hours. Grains.
1023	29	205.62
1017	51	218.28
1021	35	248.50
1018	55	280.53
1032	31	389.35
General mean		268.78

These numbers, intermediate between those of Becquerel and Lecanu, are perhaps not far from the truth, and may be regarded as the limits of health, as far as relates to excretions of urea in the 24 hours, in a person living in an ordinary manner. The above numbers being taken as the average for males; females may be considered as secreting as near as possible, in the 24 hours, 2 per cent. less urea.

[To be continued.]

Medical Trials and Inquests.

SUPREME JUDICIAL COURT, BOSTON.

March 19, 1850.

REPORT OF THE TRIAL OF DR. WEBSTER FOR THE MURDER OF DR. PARKMAN.

*Before Chief Justice Shaw and Justices
Wilde, Dewey, and Metcalf.*

[SOME delay has taken place in the publication of an abstract of this remarkable trial, owing to our space having been much occupied, and to the fact that we considered it necessary to wait for the publication of the details in an authentic form. The Report now before us, prepared by Dr. J. W. Stone, is a most elaborate account of the proceedings at this trial. It consists of 314 closely-printed octavo pages. We shall confine ourselves chiefly to an outline of the facts, with a transcript of the medical evidence. To the medical jurist the case presents numerous interesting questions relating to the identity of the dead and the cause of death;—and to all readers, the marvellous nature of cumulative circumstantial evidence, with the multiplication of the proofs of guilt, even

where an attempt was made to destroy these proofs by a criminal use of scientific knowledge. Dr. Webster had attained a high position as a scientific chemist in his own country, and had also acquired some repute on this side of the Atlantic. In our forty-third volume, 1849, page 894, we published a remarkable case of murder by poison, in which he was himself the means of leading to the detection of the crime fourteen years after its perpetration. The social position of the criminal, and his standing as a scientific man, give a painful and awful interest to these proceedings, which, chiefly by circumstantial evidence, clearly brought home to him the perpetration of a crime terrible for its atrocity, and for the art with which he attempted to conceal it. These considerations render it a duty to transfer to our pages the principal details of this case: as no doubt they will not only prove interesting to present readers, but will be sought after by future inquirers.]

The prisoner, JOHN W. WEBSTER, was charged with the murder of Dr. GEORGE PARKMAN, of Boston, on the 23d November, 1849. There were several counts in the indictment, laying the mode of death as having been produced by wounds or personal injuries. He pleaded *Not Guilty*.

From the statement of the Attorney-General, for the prosecution, it appeared that Dr. George Parkman, a well-known and highly respectable citizen of Boston, was living, in good health and cheerful spirits, on the morning of Friday, the 23d day of November last; that he was engaged in his usual occupations on that day, up to fifteen minutes before two o'clock, at which time he was last seen alive, about entering the medical building in Grove Street. He did not return to his dinner on that day; a fact which, on account of the habits of Dr. Parkman, was of itself calculated to excite some uneasiness in his family. It appeared that Dr. Parkman left some lettuce in a shop near the Medical College, with the intention of returning to take it, and thence to carry it home. At that shop he made certain purchases. He did not return. The lettuce remained there. After he went to the Medical College, although he was expected to return in a few minutes to take it, he did not return, and he did not return to his home. On the 30th of November, the Friday after his disappearance, in the vault of a privy connected with the defendant's laboratory at the Medical College, were found parts of a body answering to the description of Dr. Parkman. They consisted of a pelvis (the hip bones), the right thigh (from the hip to the

knee), the left leg (from the knee to the ankle); and with them were found certain towels with the initial of the prisoner, and similar to those used by him in his laboratory. On that day, Friday, and the next day, Saturday, were also found in the furnace of Dr. Webster's laboratory, fused in with the slag and cinders, as the evidence will show, and which is important in one view of this case—fused in indiscriminately with the slag, the cinders, the residuum of the coal—a great number of bones, and certain blocks of mineral teeth. Certain quantities of gold which had melted were also found, and other substances, which will be disclosed by the testimony. On Saturday morning there was found a tea-chest, containing, imbedded in a quantity of tan, and covered with minerals, the thorax or entire trunk of a human body, the left thigh, from the hip to the knee, and a hunting knife of a peculiar character; and around the bone of the thigh, a piece of twine or marline, which will be produced—a specimen of which will be produced here, with other specimens found in his laboratory. These parts were subjected to the examination of competent medical and scientific men. They were put in apposition,—I speak now of the parts found in the privy and in the tea-chest, independently of the bones found in the furnace,—they were put in apposition, and found to resemble, in every particular, the body of Dr. Parkman, or such portion of the body as these parts corresponded to; and, in no single particular, dissimilar to the body of Dr. Parkman. There were missing from this human body, when placed in apposition, the head, the arms, hands of course, both feet, and the right leg from the knee to the ankle. The evidence will show that they belonged to a person about the age of Dr. Parkman. He was about 60 years of age. The evidence will also show that the form was peculiar, as his was peculiar. It will be testified by the witnesses, that the height, 5 feet 10½ inches, corresponded precisely to the height of Dr. Parkman, taking, as the witnesses will explain, the average length for these missing parts—the head from the neck, and the foot from the ankle. It is not necessary for me to detain you by details. The witnesses will explain how they reach this result, to which they come, that the height of the person to whom these parts belong was 5 feet 10½ inches. And we shall show, by the passport of Dr. Parkman, that he was of precisely that height. The evidence will also prove that he was of a peculiar form and shape, and that this body was also of that peculiar form and shape; that the hair indicated something similar, and in no respect dissi-

sailor, to that of Dr. Parkman. But then, gentlemen, we shall put into this case evidence which, upon this point, I shall leave you to judge of, and to take the proper estimate upon. For I am not here to comment upon it, but merely to state to you a general outline of what it is. Of the bones found in this furnace, not a fragment was found which duplicates any one found in the vault or the tea-chest, but every particle of bone belonged to some part of a human body not found in that vault and tea-chest; showing that unless by a miracle they agreed, the bones found in the furnace, the parts found in the tea-chest, and the parts found in the vault, all constituted portions of one human body. There will also be some evidence which will perhaps indicate to your minds a probability at least, if not a conviction, that some of the bones found in the furnace were fractured before they had ever been subjected to the action of fire—the bones of the cranium (of the head). I say that there may be some evidence of this, indicating that before the bones were subjected to the action of fire, those bones had been fractured.

Then, gentlemen, you will have placed for your inspection a block of mineral teeth, which was found in that furnace, and found so near the bottom of the furnace that it took the current of cold air,—found resting upon the grate, a fact of some significance,—a block of mineral teeth, which will be testified to by two gentlemen, accomplished dentists, to be the teeth of Dr. Parkman, made for him in 1846, upon an occasion which Dr. Keep distinctly recollects, recognizing his own work, and beyond that giving you the grounds upon which he feels the confidence that he will express in his testimony; a confidence so strong, from his recollection of the work itself, and the formation of these teeth, the peculiarities of their formation, and other facts, that if he had seen them in Africa, or beyond the sea, he should have known them to be the teeth which he made for Dr. George Parkman, in the autumn of 1846. There were other portions of mineral teeth found, which will go to aid you in your judgment of the reliability of the testimony of Dr. Keep, but which are not so characteristic as the block of which I speak. It will be shown that Dr. Keep has in his possession, and can produce now, a mould of the entire jaw of Dr. Parkman, taken at the time he made this block of mineral teeth. You will see, by that mould, which will be testified to you by Dr. Keep, that it is a mould which showed the peculiar conformation of Dr. Parkman's jaw,—a peculiarity so great, that you could not find, through any ex-

prise of nature, another precisely like it. It will appear from the evidence, that these mineral teeth must have been cast into that furnace in connection with the head. That will be made perfectly clear and apparent to you, upon the evidence, found in connection, as they are, with certain portions of the bones, which will be explained to you by another medical, anatomical, and scientific witness. Beyond this, you will have exhibited to you, from among the bones, the bones of the right lower jaw, found in that furnace, in fragments, in portions, there broken and with serrated edges, put together to show that they belonged to one and the same jaw; and the conformation of that jaw, when thus put together, you will find precisely like that taken in the mould in 1846, by Dr. Keep. This will be the nature of the evidence to satisfy you that Dr. Parkman came to his death as charged in that indictment, and in connection with the Medical College.

There are one or two points, before proceeding to the other evidence, which I shall offer, to which it may be proper, in this connection, to advert. The thorax, which I have spoken of as having been found in the tea-chest, exhibits a perforation, upon which there will be evidence laid before you to show that it was a wound which penetrated between the ribs, taking off a portion of the membrane which covers one of the ribs, and entering the region of the heart. It will also appear to you that to these remains there have been chemical applications made of strong alkalis, which, upon a chemical analysis, is found to be demonstrated by an accomplished chemist, who will be here to state the result of his examination. The inference to be drawn from these facts cannot now be properly presented. It will be demonstrated to you, that these remains are not the remains of a subject in the Medical College, for two reasons: one, that there was no injection in the veins of any preservative fluid, which is the invariable custom there; and, secondly, that all the subjects are accounted for, independently of this, by Dr. Ainsworth, who keeps an accurate record of them.

The evidence will show you that the remains were separated (I was about to say mutilated, but separated, perhaps, will be a better word) by some person having an amount of anatomical skill, though evidently not dissected for anatomical purposes. There are various other facts, in connection with these remains, which I will not detain you to recite: you will appreciate them as they come from the witnesses. This is the nature of the evidence upon which the Government say that these were the remains of Dr. Parkman, and

that he must, upon this state of facts, have come to a violent death.

[We do not propose to enter into the moral evidence, supplying a motive, as it was alleged, for the perpetration of the murder. It may suffice to state, that Dr. Parkman had lent Dr. Webster a considerable sum of money, and had vainly endeavoured to procure payment. It appears that the prisoner had called at Dr. Parkman's house, and had made an appointment for him to come to the Medical College on the afternoon of the day on which Dr. Parkman disappeared, in order that the account between them might be settled. We subjoin the medical report on the examination of the remains found in the College, concealed in Dr. Webster's laboratory.]

Report of the Medical Committee on the Investigation of the Remains.

Drs. Winslow Lewis, junior, George H. Gay, and James W. Stone, being severally sworn, on oath depose as follows:—

Having been directed to make a post-mortem examination, at the Boston Medical College, in North Grove Street, attended to that duty December 2d, 1849, at ten o'clock A.M., and examined five portions of a human subject, viz.: a thorax, a pelvis, two thighs, and a left leg. The thorax and left thigh were discoloured, apparently with tan and some caustic substance. The three remaining ones were white, fair, and appeared as if soaked in water. The cartilage on the head of the left thigh-bone was coloured black. The following is a description of the five portions separately:—

I. *Remains of Thorax*, and parts attached to it, which consisted of all the bones except the sternum. Fracture of the fifth right rib, apparently recent, and about four inches from division between ribs and sternum. Both clavicles and scapulae present; clavicles large. Both lungs present, but collapsed. Left lung had pleural adhesions. Structure of both lungs apparently healthy. Anterior thoracic muscles cut up from the ribs, about six inches from the centre, on each side, and with the skin thrown on one side. Posterior portion of integuments, from the scapula to the left lumbar vertebrae, of a dark mahogany colour, and hardened. Remaining portion of integuments generally of a natural appearance, except a little greenness under the right axilla, probably from commencing decomposition, and some blueness under the left axilla, leaving the skin soft and easily broken, through artificial action exerted upon the hair and skin, as far forward as the section in the median line. An opening, slightly ragged, about an inch and a half in length, under the left nipple,

between the sixth and seventh ribs, extending into the cavity of the chest. Remains of thoracic aorta and thoracic oesophagus present. Heart and diaphragm wanting. Trachea divided through the cricoid cartilage. Spleen contracted, externally granulated, and internally red. Left kidney in its natural position, and contracted. No liver, right kidney, pancreas, stomach, or intestines.

N.B. The right kidney, much contracted and discoloured, was discovered on the next day, and given to us.

Sixteen vertebrae present, consisting of three lumbar, twelve dorsal, and the greater portion of the seventh cervical, which appeared to have been sawn through the upper part. Small quantity of long grayish hair on the front of the chest. Some stained dark grayish hair on the back. Periosteum removed from the front part of several left ribs. Both arms severed in a very irregular and unscientific manner.

II. *Pelvic portion* consisted of the bones of the pelvis, two of the inferior lumbar vertebrae, all the integuments, muscles, organs of generation, and the pelvic viscera generally. All of the intestines remaining was about six inches of the rectum, through the anterior and external portions of which a section had been made, and the mucous coat separated from it four or five inches throughout the whole circumference, but not cut off at the lower end. Hair upon this portion of a sandy gray. Both thighs severed from it in a very irregular manner. Integuments divided down to the pubis in the median line. On placing the pelvic portion in apposition with the thoracic, the third and fourth lumbar vertebrae corresponded precisely. The spinous process of the third lumbar vertebra, with a portion of the transverse processes of the same, was absent from the thoracic portion, but was found attached to the fourth lumbar vertebra, which was on the pelvic portion.

III. *Right Thigh*, on being placed in apposition with the pelvic portion, the bones, muscles, and skin corresponded perfectly. Good muscular development, with but little of fatty matter. Patella attached. Some ossification of femoral artery.

IV. *Left Thigh* had a string about two and a half feet long, tied round just above the condyle, leaving loose ends. Patella attached. On being placed in apposition with the pelvis, the bones corresponded, but some portions of the skin and flesh appeared to have been removed, or contracted from artificial means. On the anterior surface of the thigh, and somewhat on other parts, there were appearances of the action of fire, or some caustic matter.

V. *Left Leg* of natural appearance, fair size, and on being placed in apposition

with the left thigh; the articulation corresponding.

Measurements.

	Inches.
Thoracic portion, length . . .	17½
" below axilla, circumference, 30 inches.	
Pelvic portion, length . . .	9½
" circumference below crest of ileum, 30½ inches.	
Both thighs (of the same length) . . .	18
" circumference of largest part of each, 18½ inches.	
Left leg, length to the outer malleolus . . .	16
" " circum. of largest part, 12½ in.	

Total . . .	61
Deduct distance from bottom of pelvis to top of acetabulum . . .	3½
	57½

All the parts being placed in apposition, the distance from the top of seventh cervical vertebra to the outer malleolus . . .	57½
---	-----

Difference . . . ½

Total length of parts discovered . . .	57½
Distance from sole of foot to the outer malleolus on another subject . . .	3
Distance from top of head to base of sixth cervical vertebra . . .	10

Total height, 5 ft. 10½ in., or 70½

The foregoing described portions appeared to belong to a person of between fifty and sixty years of age. The muscular system was well developed, and there was but very little adipose matter.

WINSLOW LEWIS, jun.

GEORGE H. GAY.

JAMES W. STONE.

Attest, J. L. ANDREWS.

Cross-examination of Dr. WINSLOW LEWIS, jun.—[The witness explained the anatomical terms.] Had been acquainted with Dr. Parkman about thirty years—quite intimate. It would not spontaneously have occurred to my mind that it was Dr. Parkman, if I had not known he was missing. Could determine the height of the person within half an inch, to a certainty. The perforation under the left nipple was much affected by some chemical agency. Discovered no marks of a knife on the ribs. A body of the size of Dr. Parkman's would contain about two gallons of blood. About two quarts of blood, or fluid, might be found in the cavities of a body of this size after death. Could not say how long it would take to burn up a head; I think it could be consumed in two hours, but cannot state that with any accuracy; it would depend upon the quantity of fuel. Could not determine the precise age of that body from the remains. There was more muscular development in the lower extremities than I should have expected in a body like that, in other respects.

Direct required.—If a person were wounded, as by the perforation in that thorax, he would, probably, have bled more internally than externally. A body ceases to bleed very soon after death, except from the veins.

Dr. JAMES W. STONE, *sworn*.—I concur in the report that was drawn up and signed by Drs. Lewis, Gay, and myself, and which has been read. The hair on the back of the body examined was much more, and longer, than usual, and of a sandy gray colour. The muscles of the lower extremity were more developed than one would naturally expect from the general size of the body, indicating that the individual had been accustomed to much exercise in walking. In front, on the left side, the skin was burnt, and the hair singed, so that its length could not be determined. On the other side, in front, the skin was not burnt, but decomposition had commenced near the right arm-pit. Judging from the skin, hair, and general appearance of the remains, the body belonged to a person from fifty to sixty years of age.

The amount of ossification of the arteries would seem to indicate that the individual was nearly or quite sixty years of age. There was nothing in the light colour of the skin, the sandy gray hair, the muscular development of the lower extremities, or the apparent age, which would conflict with the idea that it was the body of Dr. Parkman; yet, had I not known that Dr. Parkman was missing, I should not have suspected that these remains were portions of his body. My impression is, that the individual who separated the remains had some anatomical skill. It is difficult for a person who has never done it to remove the sternum, or breast-bone. I have seen good physicians, in a post-mortem examination, even when there was no ossification of the cartilages, give up the attempt to separate the sternum from the first rib and collar-bone, and break it off, leaving the upper part of the breast-bone unremoved. In this case, the incision through the skin and muscles was made, as usual, in the median line from the neck downwards, and the sternum properly removed, by separating it from the clavicle or collar-bone, and by dividing the cartilages close to the ribs. The divisions at the joints were rightly made, though somewhat irregularly. There was but little appearance of these remains having been parts of a subject for dissection.

tion. That which looked more like dissection than any other part was mentioned in our report, viz. an incision through the anterior portion of the external coats of the last six inches of the intestine, and a separation of them from the mucous coat. In preparing bodies for dissection, sometimes one injection into the arteries only is made; at other times, two or more. The first injection is made in all the subjects for anti-septic purposes. It is fluid, and usually consists of an arsenical solution, one of the chloride of zinc, or some similar preservative substance. This fluid is absorbed, so that it is difficult to recognise it, except by chemical tests. The second injection is solid, and is usually of glue or wax, so as to dissect for the arteries. That can be easily distinguished. There was in these remains no solid injection.

Cross-examination.—We examined the aperture between the ribs, to ascertain if it had been caused by a knife, as it was suggested that this might have been the case. In this place the skin was much softer than usual, from the action of fire, and the finger might easily have been pushed through the skin and muscles. The edges of the aperture were rough, as if it had been made by a stick. There was no mark produced by the cut of a knife upon the ribs. A day or two after, I heard that there was such a mark, but at the time of our regular examination, on Sunday, Dec. 2, we could find nothing of the kind.

Direct examination resumed.—There was no mark in this opening which had the appearance of having been made with a knife. The opening extended not only through the skin and external muscles, but also through the muscles between the ribs, and through the lining membrane into the cavity of the chest. The membrane and muscles between the ribs were perforated in other places, but nowhere with such regularity as would indicate that this had been done with a knife. I do not think that all the periosteum remained on the edges of the ribs.

Dr. GEORGE H. GAY, sworn.—I signed the statement that has been read, and agree to it. I saw the remains at the Medical College, and conceived that some anatomical knowledge had been exhibited in their dissection. The separation of the head from the spinal column is not an easy act. They do not use a saw to do it, except when they wish to throw the parts away. A person without anatomical knowledge could not readily cut the head off. It is not easy to separate the sternum from the clavicle. There was no indication, in the process of removing the thigh, showing that degree of anatomical exactness that would accompany the operation on the living sub-

ject. There are several methods of taking the thigh off from the pelvis. The hole in the chest I thought might be made by the pressure of the ribs, or the end of a cane, at the time it was removed from the tea-chest. I saw the remains on Saturday, but did not examine them so closely as I did on Sunday. I made an examination of the puncture through the ribs into the cavity of the thorax, and through the membrane internally, but saw no marks of a cut upon the ribs. I observed nothing more than was stated by Dr. Stone.

Cross-examined.—We examined merely to see the external and internal appearance of the perforation; I thought it previously done with a stick, and saw nothing to make me change my mind. I examined the skin outside, and the membrane inside.

Direct examination resumed.—It was on Saturday afternoon I first saw the body; the officer was poking off the tan from the part with his cane, with which I thought the perforation was made.

[To be continued.]

Correspondence.

THE MEDICAL PROFESSION AND UNDERTAKERS' FEES.

SIR,—I see in your number for August 23, a letter from Mr. Kesteven respecting some statements which have been made to the effect that some of the surgeons in London are connected, in a pecuniary sense, with undertakers. Mr. Kesteven has, with a most laudable motive, endeavoured to wipe away this disgraceful stigma from his profession; but a circumstance which occurred to myself tells me too plainly that certain unworthy members of it are in the habit of receiving fees from undertakers whom they may have employed. I will relate the affair.

Not very long after I had put my name on the door, I was summoned to see a gentleman who was quite a stranger in London, and who was taken suddenly ill: he died in three days. As the patient was lying at a coffee-house, and the weather was excessively hot, it was deemed advisable to have him removed at once. I therefore went myself to the nearest undertaker, who has a large shop, had the dead man removed to his house, and made arrangements for his funeral. This was supplied, and a large sum of money was paid for it. About a week or so afterwards, a tall, sombre-looking young man, wearing a white cravat, walked into my consulting room, made a most polite bow, announced himself as Mr. ———, the

undertaker, thanked me for having given him the job, and at the same time threw down on the table, with an air of great self-satisfaction, something of a metallic nature wrapped in a piece of paper. He also added that he hoped I would receive this small remuneration, and that I would employ him for the future.

Young in the profession, and inexperienced in the ways of this wicked world, I was taken quite aback at what looked very much like a disgraceful insult. I kept my temper, however, and told the maker of coffins that I was not in the habit of doing business in this manner; therefore I must decline his fee. He mumbled out some sort of apology, and quickly relieved me of his presence.

Now, sir, what tales does not this little incident tell? It impresses the conviction, on my mind at least, that there must be surgeons who are in the habit of receiving compensating fees from undertakers, otherwise this man would not have attempted to play the villainous trick upon one who was a total stranger to him.

I have no doubt that the same thing frequently happens to young practitioners in particular, and I sincerely hope, for the honour of our profession, that not any consideration whatever will induce them to take fee or reward of any kind from the undertaker, should it be offered: and I am sure you will agree with me that those who are actually in league with him must be totally devoid of all those sentiments of honour and propriety which should particularly actuate members of such a profession as ours.—I am,

Your obedient servant,

HENRY SMITH, F.R.C.S.

18, Caroline Street, Bedford Square.

THE MANCHESTER MEDICO-ETHICAL ASSOCIATION AND THE POOR LAW BOARD.

SIR,—We are instructed by the Council of the Manchester Medico-Ethical Association to request that you will publish the accompanying memorial to the Poor Law Board in the columns of your journal.

We are, sir,

Your obedient servants,

JOHN AIKENHEAD, } Hon. Secs.
W. C. WILLIAMSON, }

Manchester, Aug. 24, 1880.

To the Honourable the Poor Law Board.

THE Council of the Manchester Medico-Ethical Association beg respectfully to request the attention of your Honourable Board to a resolution recently come to by the Manchester Board of Guardians, involving certain medical regulations of an objectionable character.

It is proposed that a very widely-scattered district, containing at least 50,000 inhabitants, shall be assigned to one medical officer, whose remuneration shall be £150 per annum, out of which all medical and surgical appliances shall be supplied, he being at the same time debarred from private practice; and, further, that no extra fees shall be allowed for midwifery cases, or for surgical operations, except one shilling and sixpence for each successful case of vaccination, which latter, the Council have been informed, will probably not realize more than £50 yearly. There is reason for assuming, then, that an annual average of more than 2000 cases of sickness will have to come under the treatment of the medical officer.

Under such circumstances the Council submit that the salary proposed will not defray the necessary outlay in medicines and leeches; and as no other source of income will be available (private practice being prohibited), the conclusion is certain that the pauper patients will sustain the wrong.

The Council would further submit that the physical capabilities of one man, whose means will neither enable him to employ a dispenser of medicines, to use a conveyance, nor to engage the occasional services of an authorised substitute, will be utterly unequal to the conscientious discharge of the duties imposed upon him.

For the foregoing reasons, amongst others that might be advanced, the Council of the Manchester Medico-Ethical Association most respectfully call upon your Honourable Board to withhold your sanction to the resolution in question, as being one which must inflict certain hardship upon the poor, and involve the degradation of an honourable profession, should any one be found unscrupulous enough to accept the appointment upon the terms proposed.

Signed, on behalf of the Council,

J. L. BARRELLY, M.D., President.

J. AIKENHEAD,

W. C. WILLIAMSON, } Hon. Secs.

Manchester, August 23rd, 1880.

REMOVAL OF AN ENORMOUS FIBROUS POLYPE ATTACHED TO THE PHARYNX. BY M. ROBERT.

THE patient, a young man aged 20 years, was the subject of a large polype which filled the posterior nares, the mouth, and throat. M. Robert had great difficulty in ascertaining the size of the pedicle. The impediment offered to the functions of the parts imperatively called for an operation. M. Robert having removed a portion of the superior maxillary bone, succeeded in liberating the various processes of the

tumor, and tracing its pedicle to the superior and posterior wall of the pharynx attached to the vertebral column. The tumor with pedicle was detached entire by the use of the nails alone. The hemorrhage was arrested by the actual cautery. The patient recovered.—*L'Union Médicale*. X

THE CHOLERA IN EGYPT.

LETTERS from Alexandria of the 21st of August state that cholera continued to prevail throughout the country. At Alexandria there were about 50 deaths daily, at Cairo double the number, and throughout the Delta it exists more or less.

PROFESSOR WEBSTER.

PROFESSOR WEBSTER has confessed that he premeditated the crime of which he was found guilty. Friday last, the 30th ult., was the day appointed for his execution.

APOTHECARIES' HALL.

NAMES of gentlemen who passed their examination in the science and practice of medicine, and received certificates to practise, on Thursday, 29th August, 1850:—Robert Turner Bywater, Leeds—Thomas Evans Evershed, Billingham, Sussex—Thomas Roberts Hitchen, Herts—Charles Harriott Roper, Mount Radford House, Exeter—Robert Harrison Wilson, Howdon Pans, Newcastle-on-Tyne.

OBITUARY.

On the 23d ult., in London, after a long affliction, aged 40, Mr. Henry Thomas Clarke, surgeon, youngest son of the late Mr. William Clarke, of East Bergholt, Suffolk.

At Henley-in-Arden, on the 28th ult., Hugh Francis Burman, Esq., M.D., in the 35th year of his age.

On the 30th ult., at Derby, Wm. Baker, Esq. M.D. aged 60.

On the 30th ult., at his residence in Friar Street, Reading, in the 76th year of his age, John Bulley, Esq., surgeon, sincerely beloved and deeply lamented.

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer 29.79
 " " " Thermometer 56.9
 Self-registering do. Max. 80° Min. 26°
 " From 12 observations daily. " Sun.

RAIN, in inches, — Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was 1° below the mean of the month of September.

BIRTHS & DEATHS IN THE METROPOLIS

During the Week ending Saturday, Aug. 31.

BIRTHS.	DEATHS.
Males.... 737	Males.... 451
Females.. 734	Females.. 431
1461	882

CAUSES OF DEATH.

ALL CAUSES	882
SPECIFIED CAUSES	877
1. <i>Zymotic</i> (or Epidemic, Endemic, Contagious) Diseases....	267
<i>Sporadic Diseases, viz.—</i>	
1. Dropsy, Cancer, &c.	40
2. Brain, Spinal Marrow, Nerves, and Senses	103
4. Heart and Bloodvessels	33
5. Lungs and organs of Respiration	75
6. Stomach, Liver, &c.	34
7. Diseases of the Kidneys, &c.	9
8. Childbirth, Diseases of Uterus, &c.	15
9. Rheumatism, Diseases of Bones, Joints, &c.	2
10. Skin	0
11. Premature Birth	31
12. Old Age	33
13. Sudden Deaths	9
14. Violence, Privation, Cold, &c.	32

The following is a selection of the numbers of Deaths from the most important special causes:

Small-pox	10	Convulsions	30
Measles	11	Bronchitis	39
Scarlatina	33	Pneumonia	33
Whooping-cough	24	Phthisis	130
Diarrhoea	118	Lungs	3
Cholera	4	Teething	6
Typhus	40	Stomach	2
Dropsy	13	Liver	8
Hydrocephalus	16	Childbirth	8
Apoplexy	20	Uterus	3
Paralysis	19		

REMARKS.—The total number of deaths was 196 below the average mortality of the 35th week of ten previous years. It will be perceived that there were only four deaths from cholera;—two of these (in adults) were cases of English cholera, and the remaining two occurred in children of five and eleven months of age respectively. There is not the least appearance of Asiatic or malignant cholera in or about the metropolis. In the corresponding week of 1849, 1,663 persons died of this disease in the metropolis. Diarrhoea is still very prevalent and fatal: there were 118 deaths from this cause during the last week. The deaths from scarlatina appear to be progressively on the increase.

NOTICES TO CORRESPONDENTS.

We are compelled to postpone our notice of the Poor-Law Medical Report until next week.

Dr. Hamilton's paper on the Statistics of Plymouth has been received; and, if possible, it will be inserted in our number of the 20th of September.

Mr. D. O. Edwards.—We wish to be informed when the *Atmoppyre* is in operation, in order that we may give the requisite information to our readers.

Dr. Tilt's second contribution will appear in the following number.

We shall be glad to receive papers in continuation from Mr. C. S. Bate, of Swansea.

Dr. Bucknill.—We regret that, by some accident, the Devon Lunatic Asylum Report has been mislaid. Could our correspondent favour us with another copy?

Communications have been received from Mr. Tucker—Mr. Galloway—Dr. Cox—Mr. Kirk. These will have our early attention.

ERRATA.—In our last number, page 357, col. 1, 20 lines from top, for "imbecility," read "imbecility;" and 24 lines from foot, for "possessed," read "possessor."—In leading article, page 372, col. 2, line 17 from top, for "probably," read "probably."—P. 380, c. 2, line 20 from foot, for "sulphate," read "sulphite."

Lectures.

LECTURES
ON THE
MEDICAL JURISPRUDENCE OF
INSANITY.

*Delivered in the Medical School of King's
College, Aberdeen.*

BY ROBERT JAMIESON, M.D.
Lecturer on Medical Jurisprudence in the
University.

LECTURE IV.

General description of the insane state (continued). (b) Intellectual condition of lunatics (continued)—Imbecility of mind—manifested in various habits and actions—exemplified—Imbecility in thinking—exemplified in writing—intellectual disorder increases with the prolonged effort at control—a lunatic's views of the insane conduct of other lunatics—may be aware of his legal position—the characteristics of lunatic epistles—exemplification of the expression of delusion. Incoherence—not a form of insanity, but a possible symptom in any form of the disease; best marked in mania and dementia—incoherence in monomania. Extravagance of conduct—the result of delusion—exemplified.—motives not traceable in insane actions—criminal deeds of the insane not indicative of viciousness—unsuitableness of insane actions to proposed object.

IMBECILITY is the next mental characteristic to be noticed; but, before alluding to the intellectual indications of it, I may specify some common actions and propensities in the lunatic state that manifest a general weakness of mind. In many, tears flow from very slight emotion, just as may be usually observed in cases of paralysis; and some are shaken by violent fits of laughter, apparently without any adequate reason. Laughter from slight or no cause is not an uncommon symptom in dementia—more common, perhaps, in female than in male lunatics. This risible propensity indicates no appreciation of humour: an indifferent impression on any of the senses may induce it: the sight of a stranger—a word spoken when there is silence—or even the motion of a finger. I doubt in many instances whether the laughter is even aware of his own mirth.

Weakness of mind is seen also in a childish facility of being pleased with trifles, showy articles of dress, silky orna-

ments, sweetmeats, and other things which, offered as consolation, a sensible understanding would despise, but which in a lunatic asylum are daily found advantageous in assuaging grief and conferring temporary pleasure. It requires no little attention to prevent many of the insane from making their personal appearance ridiculous by their dress. When they were more generally at large than they are now permitted to be, and when every village could show its fool, these poor objects could be distinguished by strangers by the monstrosities of their apparel.

Some lunatics exhibit a strange propensity to collect and amass together all sorts of unconsidered trifles. I was taken by an old man who had this crase to a small out-house in which he had collected a treasure of this sort, in which his whole soul was as much engrossed as ever a miser's was in his money, or a biblio-maniac's in his books. The collection, if not admirable for its value or rarity, was astonishing for its extent and variety of absolute rubbish. Pieces of old rope, filthy blacking-bottles, broken crockery, rusty iron, hens' feathers, showy rags, buttons, fish-bones, and a variety of other matters which had by oversight escaped the dunghill, were there laid up for admiration, and were esteemed by their collector a most attractive and imposing spectacle.

Certain demented persons find a childish amusement in jingling together words of a like termination. I have heard some of those who exemplified this whim go on making rhymes for an hour at a time, the similarity of sound in certain words seeming to be the only object in their continued utterance of an incoherent rhapsody. Others will go on reiterating some unmeaning convocation of words or phrases for the greater part of a day. I recollect one instance in which the attention to the sound of words took rather a singular form. The individual had become fascinated in some way by the sound of the vowel O, which he marked energetically whenever it occurred in his speech. This was, however, not so much an indication of mental feebleness as of obedience to an out-of-the-way delusion. He had the idea that there was a spell upon him impelling him to use this letter, and was no easy man to converse with, for he got furiously displeased with those that spoke to him if his ear detected the frequent recurrence of this vocalism in the words which they employed.

Feebleness of mind, instability of attention, and loss of self-control, are apparent in most things which lunatics attempt. The endeavour to instruct them in any new thing is a very difficult business, and,

unless in so far as it aids the moral treatment of the disease, very unsatisfactory in its result. Occupations of various kinds are now carried on in all good lunatic hospitals; but one sane workman would be able to work against two or three lunatics, and his work, under similar instructions, would be superior in quality as well as in quantity.

It is not an unfrequent thing for a lunatic to sit laughing and talking to himself without being aware that he is so occupied. I remember one particular example of an enfeebled mind in which the chief feature was that the patient, when left to himself, fell to talking aloud, his speech being sometimes at the full pitch of his voice, and the matter of it anything but agreeable to those who were within earshot. His talk, besides being noisy, was highly obscene, and rendered him such a nuisance both to his own house and the neighbourhood, that he was sent to a lunatic asylum. I found that in general he was not even aware that he had been talking at all, and when checked he would deny that he had been speaking ever so softly. When the matter of his discourse, however, was represented to him, he then allowed that he had been *thinking* in the way described, but persisted that if he gave utterance to his thoughts he was not aware of it at the time. He coined his thoughts into words without being sensible of his so doing. A person in such a state gives vent to much greater incoherence than his conversation ever manifests; and, accordingly, this gentleman, though evidently of enfeebled mind, was capable of conversing in a rational manner, and for any length of time, but the soliloquy of his solitary thoughts was a most lunatic rhapsody. The act of conversing with such an individual serves the purpose of steadying his thoughts, the part which a second person bears in the conversation affording points of direction and support to the vagrant attention.

Imbecility in thinking is always in a greater or less degree the accompaniment of insanity. The ideas of the insane become objects that they are no longer able to deal with by voluntary effort. Volition has not weight sufficient to make such an impression as will regulate their course. Unless the ideas can be effectively dealt with by the natural amount of power in regulating and controlling their succession, the processes of attention, memory, and comparison, must be impaired, involving with them the faculties of abstraction, imagination, and judgment. The very endeavour of a lunatic to influence the current of thought serves often but to increase the confusion; and, if he manifests imbecility in thinking in conversation, in which

he has the directing aid of another, he is likely to make that imbecility still more apparent when he is left to himself, and allowed to attempt giving utterance to a chain of connected and related thoughts. The attempt to write upon any subject is the one which will render the intellectual derangement most plainly manifest; and, in a medico-legal view of insanity, it will not be throwing away your time to give a few examples of failure in converting thought into thinking, and thinking into words. These I select not so much as being the most characteristic that might be given, but as the most convenient, from their brevity, and the absence of circumstantial and personal allusions. You are to understand that they are examples rather of special than general lunacy, and also that they are the production of those whose education and acquirements enabled them at one time to make a very different exhibition.

A gentleman whose mind had fallen into a state of feebleness, the consequence of a previous maniacal condition, but who could in the slightest effort of ordinary conversation manage to pass muster tolerably well, thus displays his inability to deal with his thoughts when he attempts to write a short letter. His object in the example which I am about to give seems to me to be to express something like the following ideas:—I have no particular object in writing to you at present, unless to take advantage of our former intimacy, and to prove that I consider it a duty to remember you; and I hope that, whatever circumstances occur, we shall still continue to have the same friendly feeling toward each other. Such may, I think, be considered the aim of the epistle. It would be a weak mind that would resolve upon sending a letter with no other object than the above, unless it were to be made a longer and more interesting communication than such a brief statement would be. The way in which he succeeds is this:—"My dear sir,—I cannot but exercise a reclaiming petition in my favour, and therefore bring to view the social intimacy in which we formerly stood connected, and in compliance soften the obligatory duty. We may still aspire to a tangible hope at least to meet the crisis of passing events, and in fellowship conciliate our conditions in like manner. The confiding heart will then furnish its results to a merited condition. With the basis of these propositions suffer me to conclude, with the greatest esteem, your dutiful friend, &c."

Here is another instance of a similar kind:—

The patient, in consequence of having received a visit from an acquaintance who

invites him to pay a visit in return to his house, thinks it necessary to make a written acknowledgment of so much attention, and acquits himself as follows:—"I was particularly happy at the pleasure to see you last day, and more particularly the most discriminate satisfaction to learn of the manifest surety of all terminating to a good effect." (His friend had probably consoled him with the assurance that he would regain his health and freedom.) "I cannot too much estimate the agreeable confidence reposed in me, and the marked attention so constituted of the family, and even friends. This ought to give me a conviction of the good society and ability of influence in point of politeness with whom I judge versed to form acquaintance: If I could see your place I think I should find it curious." (This is not a piece of naïveté in allusion to his restricted freedom, but merely his mode of saying that his friend's residence was well worthy of a visit.) "My motive would be chiefly to see it both as regards natural and that of superficial connection as to improvement. I beg to be most kindly remembered, and I am, &c."

It usually happens that the confusion of ideas gradually increases with the prolonged effort to express them. The first replies of a lunatic to a course of questioning constitute the best exhibition of his faculties; as the conversation continues the replies become more and more insane in their character. The same thing is often exhibited in writing. The following is rather an amusing example:—"One of the gardens to which the patients in a lunatic asylum had access having fallen into a state of disorder from neglect, an inmate who had something of a taste, though an insane one, in horticultural matters, chose to make the following report on the subject:—"In regard to the southern airing ground," writes he, "the shrubberies and flower-plots would be much improved by some attention to the same; therefore the rose-mounts might have a little manure added to the soil, as this appears to be the primary point with respect to these, and cleaned out, and the roots kept free of each other as well as the bushes. The general effect otherwise may speedily be got in trim, when it may form altogether a free and genteel aspect. The box edging may also form an important source of interest to the occupiers by doing it neatly, and at the same time not to lower their proper estimate of themselves as subsidiary exercise for a short time. No doubt it would have been in better order had it not been for the injuries it sometimes receives by the poor invalids that practise their much to be deplored folly; but when the heart of influence has got an acting corrective guide, it sweetens every effect to impress the mind with the

beauties of nature and the direction of man, generally speaking." The reporter gets on very well for a short space, but ultimately loses himself in a maze of words. The "much to be deplored folly" so complacently referred to, and which in the epistle is the initiatory phrase to the deplorable folly of the writer's own words, shows that a lunatic may be perfectly aware of the insanity of his companions, though insensible to his own deficiency.

It is, however, not an infrequent evidence of the diseased imbecility of the intellect to find that the lunatic is not cognizant either of his own or of others' delusions. He is unable to discover anything extraordinary in the conduct of those around him. A maniac he considers to be a very lively person, something too much so perhaps; but he is not struck by anything irrational in his behaviour. It is often the first mark of convalescence for a patient to become sensible to the absurdities of his companions.

I may here direct your attention to the fact that lunatics, though blind to the defect of their own judgment, are often sufficiently aware of the estimation in which they are held by others, and have a perfect knowledge of their irresponsibility in the eye of the law. For example, a lunatic seeks to gain his freedom by bribing his attendant with a promissory note for a considerable amount, and in a private communication to one of his friends tells what he intends to do, remarking that of course there is no danger of loss to be apprehended, for that his position incapacitates him from being lawfully a party in any agreement. This might be pleaded by the patient's curators or his heirs, but not at any time by himself; for it is a principle in law that no man shall stultify his own acts. Not long ago, under my own observation, an insane patient made the attempt to destroy the life of his attendant, who was at the time in a stooping position, by suddenly snatching up a spade and aiming a blow at his head. Manslaughter, he said, was of little consequence to him, for no madman is punishable by law. You will find many instances of a similar kind on record, and in regard to them all you will notice that they occurred in persons who knew that they were considered to be insane: indeed, they are all related of patients under restraint; and such speeches do not indicate that the individual is aware of and believes in his own madness, but only that he knows that he is accounted insane. No person, therefore, who falls suddenly into the state of mental derangement and commits a deed of violence, can be supposed to do so because he knows that he will be exempted from punishment on account of the

state of his mind. A criminal lunatic does not believe in his own insanity at the time that it is pleaded for him by his counsel. He may become aware of his legal position only when experience has taught him how his conduct is judged of by the world.

The following short attempt at letter-writing is a lamentable example of imbecility: it was nearly the forenoon's occupation of one who was in his time reckoned a very clear-headed and intelligent professional man:—"Dear sir,—You are directed pull down all authorities municipalities and powers in which I am invested, and to regard me as extinct and not monumental. You are feudally you hereby required to remodel all authorities powers and dignities but not you are in my name not for the space of five days less or more not exceeding ten years but not in my name not not.—Yours, &c." The production, to be duly appreciated, would require to be seen. It evinces besides delusion great decay of the faculties of attention and memory, and an extraordinary inefficiency of the voluntary power over the current of thoughts.

Though the examples given are all of a brief kind, you are not to suppose that brevity is the characteristic of insane epistles. And here I may take the opportunity of making a few observations on such productions. The letter of one labouring under mania will bear every possible evidence of his condition. The handwriting will not be like his ordinary handwriting; the page will be blurred, blotted, and disfigured; the words frequently misspelled, omitted, and misspelled; the letter probably grotesquely folded, and put up in some absurd way; and the matter rhapsodical, incoherent, and influenced by his delusions. Such a communication is not likely to be very long. On the other hand, the letter of a monomaniac will probably be very much the opposite of terse. A monomaniac, as he never wearies thinking or speaking of his delusion, does not condense when he comes to deal with it in writing. Such a letter, while it may be tolerably connected, and manifest possibly considerable activity of mind, will usually be found to be of a most extravagant length. He knows no limitation but the amount of paper he can at the time command. It will be like a web of inky tartan, crossed and re-crossed; warp and woof of it covered with his insane crêchet,—his insane crêchet alone. Many of the partially demented have also a great plenty of words,—so great, that they could apparently continue writing for ever. Such productions are distinguishable from the profuse communications of the monomaniac by the constant incoherence of thought and language displayed in them. When the imbecility of mind is great the epistle is

very short, from evident want both of ideas and words.

The epistles I have adduced have exemplified the intellectual feebleness of the insane, rather than their diseased imagination and loss of judgment. I shall now finally give you one example of the expression of delusion: it is to this effect:—"A mutual friend informs me that he was with you this morning, and that you was expressing a desire of hearing from me. It is a very long time I have been here, almost fifteen years. I have seen you twice. If you would give me a call occasionally I would take it as a favour. For what reason have I been here such a length of time? As a madman or an idiot? No! There is not a madman or an idiot here, and has not been during the time I have been here. For what purpose, then, are they or I here? Am I here voluntarily? What say you? You know I am not. We who are here are all operated upon by zinking mechanicals. I should think by this time the nature of these is in some degree known beyond the walls of this place, and that yourself knew something of them. Their operation is now general over the whole earth, from pole to pole, and from east to west. Of the structure of the mechanicals I am ignorant. A chief part of them, I understand, is under the surface of the earth. From information given me through my own mouth by some of those persons who operate by means of these mechanicals, I understand that this globe is not a spheroid, but of the shape of a kettle-drum, an instrument used for music. The crust does not in any point exceed a thousand miles, in some cases not half a mile. The latter takes place at the bottom of mountains. The thousand miles is at the pole. The other part of the interior of the globe is a cavity, with light equal to that at the pole in December. The extent of the cavity is from one thousand to twenty thousand miles. In this cavity the zinker is placed in a balloon, in which he travels with the speed of electricity: besides this, he has the benefit of the earth's motion when twenty or forty miles below the surface. He perforates whenever he wishes to make observations. A new era has taken place in medicine. The zinkers have already surveyed all the inhabitants of the globe, and, according to their information to me, of the correctness of which I have no doubt, they have improved the breed of the human species. Their work in curing and preventing fever and other diseases affecting the general system is silent, and perhaps almost unknown. Cases, the cure of which would be considered miraculous, are not yet completed, although I understand entered upon. These are cases of

blindness, deafness, and lameness. These in all possible varieties have been surveyed, and are all curable, and to be cured, over the whole earth by means of the sinking mechanicals. When?—I am yours, &c.”

Incoherence is an indication of the intellectual disorder in insanity on which I shall not long delay. Suppose that a person of sound mind were to give utterance to his internal thoughts just as they arise, the soliloquy would probably be a very rambling one, but it would not be incoherent. He would be sensible of the relation that regulated their succession, and even an auditor would be able to trace something of the connection of each thought with those that preceded it and those that followed. There would doubtless be certain links that had a disjoined appearance, because every one has certain special ties of association that are peculiar to his individual habits and experience; but on the whole there would be evidently a method in the thinking, and intelligible relations betwixt the several ideas. The incoherence of lunacy is not merely the expression of the vagaries of an unreined imagination. In its highest degree it is a string of ideas that have no relation but that of sequence, a sequence of *non-sequiturs*, so to speak. Incoherence results from all causes that occasion loss of voluntary power over thinking and expression. It may be the temporary consequence of undue emotion, the necessity of influencing delusion, the evidence of impaired memory, or of distracted or enfeebled attention.

Incoherence has sometimes been termed a form of insanity, and the word used as synonymous with a variety of dementia; but though a symptom which is best observed in dementia following mania, it may be a feature in every form of insanity. It is a more usual and more early symptom in the mental disorder of the weak-minded and the uneducated, than in that of the better endowed and better instructed. In mania, whenever the excitement runs high the ravings of the patient are remarkably unconnected. In such a condition the symptom originates in a distracted state of the attention, occasioned by the delirious course of images through the mind when the associating faculty is deranged. The speech is often seen to be a tissue of half-expressed thoughts, the images following each other so rapidly that language cannot overtake them. In dementia, incoherence is the language of an enfeebled and unstable attention. This faculty is the union of perception with desire, but in those who have fallen into the state of chronic dementia the voluntary element of action has become so impaired that the faculty of attending is almost

lost. Haslam gives an illustration of this state of the mind in the case of a man who was unable to tell any story straightforward from beginning to end. He would begin with the best intentions, and proceed a little way tolerably well; but the chairs and tables, and all the objects around him, a hat hung upon a peg, or an ornament on the chimney-piece, would become interwoven with his narrative, and lead him from subject to subject. The demented are loquacious about things which have neither sense nor connection, reason nor rhyme. “They speak,” says Feuchtersleben, “in the same breath of sabres and toothpicks, children and hats, broken pitchers and dismantled ships.” In these cases of partial insanity, so called, in which the patient is said to be insane on one subject or set of subjects, it is common enough to find, especially when the case is of some continuance, that, however collectedly the patient may express himself on other subjects, upon this his weak point he cannot give utterance to his thoughts in a coherent strain. The patient is not unsound in mind simply as entertaining a delusion, but still further intellectually disordered that he is also incoherent in reference to his error in judgment. A monomaniac is frequently not merely deluded, but unintelligible: deluded, and incoherent on all points connected with his delusion.

Extravagance of conduct.—The intellectual disorder of lunatic patients, besides being manifested in delusion, imbecility of thought, and incoherence, is also seen in their actions, in unaccountable deeds of violence, unnatural impulses, irrational attempts at impossible ends, unfruitful experience, and an insensibility to many forms of discomfort.

Where not merely automatic or instinctive, lunatic actions are the result of the instigation of delusion, though the connection is not always to be made out. Deeds which seem to be incomprehensible may owe their absurdity not to want of premeditation or design, but to insane reasoning in connection with a hidden governing delusion.

An excited maniac spent his time mostly in turning somersets on the ground, which was certainly an absurd and dangerous occupation for a person past his boyhood, and whose business was the grave study of theology. There was no alleged or apparent motive, and little other indication of delusion. I came to understand, from his own information afterwards, that this was done at the instigation of a voice in his head, which he made of worship most

often. He had a strange propensity to enter the interior of chimneys, to

the great danger of his life: in truth, he narrowly escaped suffocation and burning. He was for a time constantly bent on this as the great aim of his existence. It was an action which no one would pretend to understand, unless as a peculiar kind of suicidal inclination, which however it was not. He told me after his recovery that he believed that the only way in which it would be possible to persuade mankind that he was truly the emperor of Russia, as he then supposed himself to be, was by ascending to the roof of the house in that manner. He was a silent lunatic whose actions were the source of much care and anxiety, and whose delusions could never be surmised.

Professor Alfred Taylor adduces a case which seems usefully illustrative here,—the case of a young man whose insanity led to a criminal deed such as never could have been anticipated from the character of his delusions. "He was a person of mild manners, and laboured under a delusion connected with windmills. He would go any distance to see a windmill, and would sit watching one for days together. His friends removed him to a place where there were no mills, in the hope that this strange propensity would wear away. He enticed a child into a wood, and in attempting to murder it, cut and mangled its limbs with a knife in a horrible manner. How would any sane person," says he, "have connected this delusion with attempted murder? Yet it turned out that he had taken the resolution to commit this horrible crime in the hope that he would be removed as a punishment to some place where there would be a mill."

The violence and destructiveness of which the insane are at times guilty, is often not the evidence of viciousness, but the consequence of disordered reasoning. For example: a maniac, during his paroxysms, amongst other acts of destructiveness used to occasion much trouble by tearing his shirt and bedding into shreds during the night. After recovery, he accounted for so doing in this way. He said that he was perfectly aware of what he was about, but that he had no idea then that it was anything wrong; on the contrary, he did it more to please others than himself, and actually believed that it was a very meritorious action which would do some vague kind of good to himself and others, provided he succeeded in tearing the articles into certain determined shapes. He was very much disappointed to find when visited in the morning that he was blamed instead of being praised by the attendant; and this he attributed not to having torn his bedding, but to his not having succeeded in making the fragments of the proper shape.

In the published narrative of one who experienced and recovered from a lengthened attack of mental derangement, the same absence of malice is prominently represented. "I knew no malice," he says, "no vice. I imagined that the keepers loved me and were all deeply interested in the salvation of my soul; and I imagined, too, that I loved them dearly. Yet I wretched with them, and offered to do so with others, and struck many hard blows, sometimes, as one informed me, making it difficult for three men to control me; yet, whenever I did this, I was commanded that they wished me to do so, to prove my faith and courage, but that they were commanded to prove both till they were satisfied of my sincerity." "It was a great delight to me to get my hand at liberty, even for a moment, and the first use I usually made of it was to strike the keeper who untied me directed by my spirits to do so, as the return he desired above all things else, because he knew I was proving my gratitude to the Lord Jehovah at the risk of being struck myself."

Thus you will observe, many criminal acts might be done by a lunatic without a criminal motive. He might know abstractly that it is wrong to burn or destroy without being aware that the particular deed of violence which he is committing is any other than good and praiseworthy. We are not likely to trace the motive in the action that an insane person has committed, nor, although he may have set about the action, and carried it out by the very means and in the very way that a sane person might have done, can we infer that he therefore was in a position to distinguish right from wrong.

The acts which a madman performs in obedience to his delusion are frequently not merely unindicative of their motive, but ridiculously unsuitable to the object intended. A patient walking about one of the gardens of an hospital, generally picked up little stones and threw them over the wall, because she understood that the house was by and by to be consumed, and she wished to make her escape. She never attempted to force the door or climb the enclosure, nor did the experience of any preceding day suggest the futility of her persevering in this mode of seeking her object.

M. STROMMEYER.

THIS eminent Prussian surgeon was wounded in the battle of Idstedt, taken prisoner by the Danes, and conveyed to Copenhagen. He was immediately set at liberty by the Danish Government.

Original Communications.

ON THE
ORIGIN, CAUSE, AND NATURE OF
CHRONIC OVARIAN TUMORS.

BY EDWARD JOHN TILT, M.D.

Senior Physician to the Paddington Free Dispensary for the Diseases of Women and Children, &c. &c.

Fibro-serous Ovarian Cysts.

WE have already pointed out that cysts may be developed in the cells of the ovarian cellular tissue by a process of hypersecretion dependent on the increased action of its innate powers, and that they may also originate in extravasated blood—the only ways of accounting for the fibro-serous cysts of the broad ligaments. We have, however, in the disturbed secretions of the ovarian follicles, a much more frequent origin of ovarian cysts. Considering the ovarian follicles in the whole tenor of their monthly career, pathological anatomy obliges us to admit that these follicles frequently neither arrive at perfection, nor do they succeed in bursting the thickened ovarian sheath and the peritoneal membrane, which may be covered with false membranes. The little cyst thus originated may go on increasing; but, unless inflammation intervene to give increased impetus to the exhaling process, such formations are arrested in their progress, and we find traces of their existence in those gary, puckered, withered sacs, which Delpech erroneously considered a new kind of cyst, whereas we merely view them as an instance of Nature's power in healing these small cavities, as she sometimes does much larger cysts, by the absorption of their contents. Generally speaking, the ovarian follicle bursts, and the remaining cavity is filled by a blood clot; and, when we consider that the peculiarly-organised body, or corpus luteum, often occupies two-thirds or more of the whole ovary, as may be seen by a glance at the preparations in the museum of the College of Surgeons, we cannot but admit that, notwithstanding the adaptation of the whole energies of the ovary to absorb the corpus luteum, this process may sometimes be arrested, or so disturbed as to brigrate an ovarian cyst of an hematic origin.

This is no fanciful assumption, for in four instances Pouchet found a fibrinous clot in the corpora lutea of sows; and it stands to reason that a clot of blood so situated may undergo the same changes as it does when deposited in the brain or in the cellular tissue;—or, in other words, that it may be transformed into a cyst, and, once formed, it has within itself the power of increase. Duplay and Teissier have, we believe, indicated this possible cause of ovarian cysts. Ruysch, Kruger and others, struck by the great size to which the yolk-bags sometimes attain, have supposed that small ovarian cysts were cases of unfulfilled pregnancy, and that the ovula themselves, imperfectly formed, may protrude from the bursting vesicle, and, remaining attached thereto by inflammatory adhesions, develop themselves unimpregnated, and produce those small pendulous cysts which hang so frequently from the ovary, or from the fimbriated extremities of the oviducts. But this explanation cannot be received in all cases, for it would not account for similar bodies found pendent from the broad ligaments and numerous reflections of the peritoneum; and, when we consider how often the fimbria are in contact with menstrual blood, we cannot but attribute an hematic origin to the little cysts which so frequently are appended to them, or to the interior of the Fallopian tubes. Dr. Bright has published an interesting case, communicated to him by Dr. Beaumont, where innumerable pendulous tumors like polypi, transparent and filled with a watery fluid, some the size of a small pear, were attached by threads to the reflections of the peritoneum. These appearances seem to indicate a secretion settling into a precise form, and are analogous to certain uterine vegetations which are of a gelatinous or cartilaginous consistence, and divided into one or more cells containing a clear serous or a gelatinous fluid. We are inclined to ascribe to both descriptions of bodies an hematic origin, and we shall support our views by giving a case published by Andral (*Precis d'Anatomie Pathologique*, vol. ii.):—

“On opening a patient who had died of ascites, I found red serum effused both in the peritoneum and in the pleuræ. The pleuræ were covered with little round bodies of different sizes: some were mere clots of blood, but

others presented the different stages through which the clot passes before it assumes the pale colour of fibrin. These bodies adhered to the serous membrane by cellular or vasculo-cellular prolongations, and four or five of them were full of a milk-like fluid. The peritoneum presented similar bodies, some like blood-clots, but most of them looked like gray and white bodies full of a matter having the appearance of cream. Some of the intestinal lymphatics contained a similar white matter. After an attentive examination of these little bodies, it appeared to me evident that all of them had the same origin; that they had all been blood-clots, which, on being organised, had become the seat of a morbid secretion. I have met with numerous instances of a similar morbid process."

We have thus established that the fibro-serous cysts may originate—1. in the cellular tissue of an ovary; 2. in the follicles of that organ. With regard to the follicular or multicystic variety, we believe that it is always found in the ovarian follicles. We cannot in any way admit, with Dr. Hodgkin, that such cysts are formed by the primary cyst generating the secondary, and these again the tertiary,—an explanation which we have admitted as satisfactory for certain complicated forms of fibro-serous cysts; and in order to account for these formations, we can only suppose a general hypertrophy of the whole tissues of the ovary, which, if magnified by a solar microscope, would itself present an appearance something similar to that of these strange multilocular tumors.

Having enumerated the various origins of ovarian cysts, we are now better prepared to inquire into the value of the causes which prompt their development. These causes are—I. Predisposing; II. Exciting. 1. The predisposing causes are—*a*. The functions of the ovaries; *b*. The constitution of the patient.

a. With respect to the structure and functions of the ovaries as predisposing causes of ovarian cystic tumors, we may, perhaps, be permitted to observe, that in fishes and birds the ovaries are distinctly formed of vesicles clustered together, sometimes exactly resembling pseudo-hydatid structures; and that in many of the mammalia, as in the sow and the hedgehog, the vesicular dispo-

sition is also apparent; that in the ovaries of the *fœtus* minute vesicles are distinguished embedded in the stroma; and that the monthly duty of the ovary is to elaborate the menstrual cyst: but sufficient having been already said upon this subject, we shall merely add, that the cystic structure of the ovary, its periodically increased flow of blood, and its stimulation by coition, conception, and pregnancy, are predisposing influences, the moments or cumulating effect of which must powerfully tend to increase any morbid growth which may have once taken root. Why should we feel surprised if the morbid operations of the plastic force are most seen in those organs wherein vegetative power has been centred for the propagation of mankind, and if ovarian tumors are most frequent at that period of life when all germs of disease in the genital organs rapidly spread?

The predictions of physiology are confirmed by statistics; for ovarian disease generally begins in that interval comprised between the first appearance and the cessation of menstruation. That its appearance coincided with the disappearance of the menstrual flow was formerly believed; but if we add 260 cases, collected by Dr. Chereau, to 60 of our own, and then deduct 40, wherein neither the age was clearly marked, nor the particular time when a tumor was first perceived by the medical man, we find that the origin of the disease was observed to occur between the ages of 37 and 47 in 106 women, and between the ages of 17 and 37 in 130 cases. According to a table made by Mr. S. Lee, we find that in 140 cases

45 began from 30 to 40 years		
37	"	20 " 30 "
26	"	40 " 50 "
17	"	50 " 60 "
3	"	60 " 70 "
2	"	70 " 80 "

We also obtain an insight into the period of life in which women are most liable to such disorders, from the reports of the Registrar General, which, valuable as they have already been, acquire every year an increased perfection, which they owe to the enlightened and persevering exertions of a member of our profession, Mr. William Farr.

I have been permitted by the Registrar-General to extract from unpublished documents the following figures.

Deaths caused by Ovarian Dropsy in London during the Year 1848:

Years.	Deaths.
20	2
25	1
30	6
35	4
40	7
45	8
50	2
55	5
60	5
65	3
70	1

Deaths . 44

Here, again, we see the majority of deaths occur between the ages of 30 and 45; and no doubt many of the deaths which occurred at a later period were owing to the disease which had originated many years before.

Ovarian cysts may be developed independently of the menstrual *aisus*; for Bergstrand (Hamburgh, 1837) found the left ovary of an infant dropsical, and much increased in size. Meyer, of Bonn, met with a case of multicystic ovary in an infant of 2 years. Schatel (Wurtemberg correspondent) found twenty-five pints of serum in an unilocular cyst in a child. Itard saw an ovarian cyst in a girl of 13; and Frank, one at the age of 14. Dr. Ashwell has known the disease to appear at the same time as menstruation in a girl of 14; and Dr. Cheveau has had a similar case, where the girl perceived a swelling soon after the first appearance of the catamenia. It is necessary to remark that Morgagni and the older writers, as well as Meckel, Boyer, B. Cooper, Chelius, and others, thought it sufficiently proved that the left ovary, which is the most frequently attacked by acute ovaritis, was also most liable to chronic disease; but statistics seem to prove the contrary; for, by adding to those we have seen the number of cases given by Chereau, Lee, Kilgour, Bluff, Tavignot, we find 475 wherein the side affected was specified: thus—

The right side	260
The left	173
Both sides	42

Dr. Clay also mentions that nearly four-fifths of the cases he has seen have been on the right side.

Professor Owen says that, as a general rule, the right and left oviducts and testicles of birds are at first equal in

size, but that the left organs alone attain to that state of development which qualifies them for several functions. It was probably to the unknown cause of this singular fact that Carius alluded when he explained the *supposed* greater frequency of ovarian disease in the left ovary by what he called "the ruling productivity of the left ovary." German transcendentalists will now be obliged to explain the greater liability of the right ovary to disease by admitting that the ruling productivity has changed sides.

Some facts may be adduced to show that this disease may be hereditary: thus Meissner mentions "that Alt lost his daughter at ten years of age, and that, on opening the body, an ovarian degeneration was found, similar to that which had caused the death of her mother;" and Campbell (*Midwifery*, p. 176) says that he was informed by one of his pupils that nine sisters were all attacked with ovarian disease!

Constitutional diathesis.

Cystic growths have been often considered the expression of some constitutional perversion of the fluids, either cancerous or scrofulous. Ledran first started this opinion, considering ovarian dropsy as the epiphenomenon of scirrhus, —a term not very definite now, but most indefinite in the time of Ledran. This opinion has been admitted by Delpech, and partially so by Cruveilhier and Mad. Boivin; while in our own country Dr. Bright and Mr. Bransby Cooper have allowed the same explanation. That certain fibro-cartilaginous tumors shall have been mistaken for scirrhus is not surprising; but the denomination of *malignant*, applied by Dr. Bright to many forms of ovarian disease, is neither warranted by the general structure of the tissues, nor by their microscopical investigation. Having met with several remarkable instances of real cancerous ovarian cysts, and finding it difficult to distinguish between these and the simple forms of ovarian cysts, it seems as if Dr. Bright tried to elude the difficulty by calling the latter *malignant* tumors of a mild nature. Even the little innocent pendulous cysts are thus termed malignant. If this were true, we should more frequently see ovarian dropsy of long standing complicated by the cancerous lesion of some other organs, whereas the complication is rare. Professor Reclamier, during his long practice, can only

remember two cases of cancer coinciding with ovarian disease. Again, if this were tenable, we should, in the numerous cases of rupture of the cysts with effusion of their contents into the peritoneum or other organs, have at least sometimes seen the propagation of cancer to those organs. As this is not the case, we may dismiss the subject; admitting, however, the liability of the ovary to cancer,—a form of ovarian cysts which we have already described. If only a small proportion of ovarian growths derive their existence and peculiar nature from a cancerous diathesis, we are convinced that a large number owe their origin to a scrofulous modification of the tumors. That delicate lymphatic subjects are the most liable to these forms of disease, has been long remarked; and we believe Mad. Bevin was the first to consider them as accompaniments of this temperament: St. Jefferson, Copland, and Cruveilhier, adopt the same view, and we hold it to be correct. If we consider the topographical situation of the disease, an etiological element which ought to stand first in every similar investigation, we find that in hot climates, where the scrofulous taint is little observed, ovarian disease is likewise of rare occurrence. It is not known in India. Dr. Davey, of the Ceylon medical service, has assured me that during his five years' residence in Ceylon, and attendance at a dispensary in Colombo, where the native women, out of a population of 200,000, willingly sought advice, he never met with a case of ovarian dropsy, although ascites and anasarca were not uncommon. During our residence in Egypt and in Syria, our own observation, and the experience of those who practised in those countries, taught us that it was not to be met with there. We have been assured by our friend Dr. Thompson, who practised for four years in Damascus, which boasts of a population of 250,000 inhabitants, that he never saw a single instance of this disease. Dr. Ross has also told us that, during his ten years' practice in Madeira, it has been very seldom observed by him; and during a long residence in various parts of Italy, we never met with a case, and the native practitioners assert that it is not of very frequent occurrence.

In answer to some questions which I addressed to him through the Colonial

Office, Mr. Ferguson, Government Surgeon of Western Australia, assures me that ovarian dropsy is commonly met with amongst European settlers, whereas it is unknown to occur in the native women of Western Australia,—an immunity which may be the effect of race as well as that of less sophisticated habits.

We must turn from these sunny climes to those temperate regions where, for the greatest part of the year, the sun has not sufficient power to warm into healthful life the dull fluids which circulate in our veins. We must come to France, England, and Germany, where, to retain their warmth, the inhabitants must submit, the greatest part of the year, to be cooped up in a small quantity of more or less contaminated air. We must turn from countries for the most part thinly peopled by those who, amidst all their failings, still follow some salutary principles for the perpetuation of their race, to those highly civilized countries where, in certain spots, millions are assembled, and are so huddled together as to obstruct the vivifying rays of the sun, to contaminate the air and poison the water,—enlightened races, most anxious about the purity of the blood that flows in the veins of bulls, or sheep, or horses, but who do not care by what impure blood they may contaminate their own perhaps not less impure, reckless as to whether their descendants are destined to inherit scrofula, gout, cancer, or even madness—that still more terrible canker of the mind. Shall we then wonder when such blood becomes more and more vitiated in a population, when it is prohibited, by act of parliament, to revivify it by the regenerative influence of air, light, and water? can we wonder that scrofula should be common amongst us, and that from this heteromorphous mass of fluid there should spring, in various parts of the body, heteromorphous growths? Now,

* We do not seem to be sufficiently aware of the fact that the modern researches into the nature of the fluids of the human body by physiologists and chemists lead us far away from the exclusive dogmas of solidism, and will soon force us again to adopt the long-despised doctrines of the humorists. It is true the microscope is supposed by some to give us, in the knowledge of the cell, the ultima thule of physiology and pathology; but the investigation of the cell is only a more minute species of anatomy, and those who think to find therein the ultimatum ratio of health or disease will be sadly deceived.

Already do some of the most distinguished

although ovarian encysted tumors may be met with in all constitutions, the scrofulous generally coincide with the heteromorphous growths I here describe. If, however, from speculations we turn to facts, we find that in many of the cases recorded in medical journals, the strongly lymphatic tendency of the patient has been noted. For instance, the ten cases given by M^d. Berwin (*Mémoires sur une cause de Favorisements*) occurred in women of a similar constitutional tendency; and thirty out of the fifty-three cases we have treated, or whose treatment we have carefully followed, had the attributes of a lymphatic constitution; and had been affected by glandular swelling, swellings of the joints, scrofulous ulcers, or by phthisis.

ON THE OPERATION FOR THE REMOVAL OF SCROTAL TUMORS, &c.,

THE EFFECTS OF MESMERISM AND CHLORO-
FORM COMPARED.

By JAMES ESDALE, M.D.
Marine Surgeon, Calcutta.

I AM induced to address you in consequence of having read, in the February No. of the MEDICAL GAZETTE, an account of a fatal operation performed at Alexandria for the removal of a large scrotal tumor. As I have probably performed more operations of this description than have taken place all over the world, you and your readers will not be displeased perhaps to learn the practical results of so extensive an experience.

From the rarity of this disease in temperate climates, this operation is

microscopists of the North acknowledge that they must look beyond the cell, and more closely study in the different tissues those molecules and granules of which cell-growths are the perfect development. But beyond the granules there are the fluids, in the midst of which, and out of which they were elaborated, and in which they will again be dissolved; and although these do rank below the solids in vital capacity, it is now well proved that even from the fluids the vital principle can build up fibres and membranes, whether in the normal or the morbid state, without the intervention of either molecule, granule, or cell. Thus we are again brought back to the fluids, and to the universal fluid, the blood, which still maintains its unity of composition, despite the elements incessantly withdrawn from it by so many organs, and the refuse with which they continually pollute the purity of its stream.

When chemical poisons are introduced into

comparatively a novelty to European surgeons.

These singular and often prodigious tumors are so common in Bengal, that they may be considered as an endemic curse of the climate. The disease in a great measure is confined to Bengal and the sea-board of India. A hot, moist, malarious, and relaxing climate seems to favour its invasion, as it is rarely met with in Upper India. In Egypt it is also principally confined to the Delta of the Nile, and is seldom seen above Gairo. It is also prevalent in the West Indian Islands. All these tumors are generally called "elephantiasis of the scrotum;" but this is incorrect, as simple hydrocele is probably the origin of nine-tenths of those here, the remainder being divided between syphilis, leprosy, and elephantiasis. There is a most remarkable predisposition to hydrocele prevalent among this population, the extent of which may be imagined from the fact that about 2000 cases are operated upon yearly in the hospitals of Calcutta; and this is the perennial source of the inexhaustible supply of this kind of surgery.

The native treatment, which is worse than useless, is to make deep eschars in the tumor with red-hot charcoal balls, which often brings on an intense inflammation, deep sloughing, and fatal hemorrhage, and never, as far as I know, causing a resolution of the tumor. On the contrary, I am convinced that it accelerates its growth by the local irritation; and it causes a most vexatious complication of the case to the surgeon operating, as the testes are always adherent to these cicatrices, and often completely involved in them. The disease sometimes only attacks the penis,

the body, they can be detected in the blood by chemical reagents; but as yet we have no pathological reagents by which we can prove the presence in the blood of those pathological poisons, which evidently circulate in the blood long before they are deposited in the tissues. Professors Andral and Forget have detected encephaloid and tuberculous masses in the blood; but such cases are rare, and only occur in the last stages of a super-saturation of the system by these pathological poisons.

The evident tendency of modern pathologists is to discover pathological poisons in the fluids, when there is yet the possibility of eliminating them. Such is the direction of the labours of Dr. Frost and Dr. Bence Jones, of Professor Bennett and Mr. Quekett, and others; they do but continue the researches of the older humorists with a more advanced chemistry, and a new mode of investigation; and if they do so it is because they hope to find in the knowledge of the fluids the secrets of health and of disease.

which assumes the appearance of an elephant's trunk, hanging down to the knees, and ending in a wart bigger than a man's fist. But however fantastic or monstrous the shape may be, the penis is always found unchanged, and *in situ*; and in these proboscis-like productions it may be at once found, by cutting down upon a cane pushed up to the pubes, or, still better by slitting the tube up with a very long strong *bistouri caché* thrust up to the symphysis pubis. The operation for the removal of scrotal tumors, till of late, was considered so formidable, that few surgeons cared to deal with large cases; and Dr. Goolens, of the Medical College, Calcutta, who had considerable experience in this line, thus speaks of it, in Vol. viii. of the Transactions of the Medical and Physical Society of Calcutta:—

"It is clear that this operation is one of imminent danger, in which, hitherto, it has appeared that the chances for and against the recovery of the patient are evenly balanced."

The practical conclusions I have come to from 161 operations are—

I. That, so far from this being the case, the results are marvellously satisfactory, even when the tumors weigh 40, 50, 60, 70, 80, 90, 100lbs. and more, provided the operation can be got through quick enough,—in from three to six minutes, and in the mesmeric trance. In 161 operations for the removal of tumors, from the size of a man's head to 103lbs., the mortality has only been *five per cent.*; but in no instance has death been caused directly by the operation, or followed soon after it, but at the distance of many days, weeks, or months, in consequence of lock-jaw, fever, cholera, diarrhoea, and exhaustion of the system;—from what may be called hospital diseases, and accidents in the course of the cure; and this has been going on with all the regularity of a law of nature for the last five years.

II. That, when the constitution is good, the patient not above 40 years of age, and the tumor does not exceed 40lbs., or possibly 50lbs., it is quite practicable to save all the organs without much danger.

III. That when the age of the man and the size of the tumor exceed this, no attempt should be made to save the testes; because the patient will either bleed to death in the attempt, or, if not, he testes will be found to be useless

after all, and requiring to be removed. But, even supposing the patient to escape with his life, and the testes to be sound, his constitution would not be able to perform the process of covering them again; they would slough, and require a second operation for their removal, or the man would die exhausted in the effort at reparation, as we have more than once seen. I therefore never attempt to preserve the testes when the tumor is above 50lbs. (unless the man is strong and robust); but the penis, with one exception, has been always saved, however large the mass.

I will now present your readers with some illustrative cases, taken from the beginning, middle, and end of my practice, and will conclude with some practical remarks on the propriety or otherwise of administering chloroform in such operations.

The following is among the first mesmeric operations for this disease:—

Sept. 2d, 1845.—I was stopped on the road to-day, and requested to go into a temple to see a sick Fu Queen. On entering, I found a healthy-looking man of 60; but he was nearly blind from cataracts, and had a scrotal tumor of about 30lbs., weight, which he begged me to remove. I examined it, said I would consider of it, and then drove on to Chinsurah to see Dr. Ross, the surgeon in charge of the troops there, to whom I said, that if he chose to return with me I would endeavour to show him a mesmeric operation. He willingly consented; and, in passing the Fu Queen's house, we carried him to the hospital along with us. The cook of the hospital, one of my best mesmerisers, was set upon him, and in half an hour made him insensible. Considering the man's age, and the size of the tumor, we did not think it worth while to save the testes, and the operation was therefore very soon effected. The man never moved, and did not awake till after the last artery was tied. Seeing him about to awake, he was covered with a sheet, and asked, when he came to his senses, if he had been in any way disturbed in his sleep: he said "No." He was then desired to sit up and show the tumor, as I wished to examine it. He did so, and actually put his hands down to raise the mass as usual; and his look of amazement on missing it was something not to be forgotten.

Dr. Ross published an account of this

and other operations in which he assisted me.

Oct. 25th, 1846.—Gooroochurn Shah, a shopkeeper, aged 40. He has got a monster tumor, which prevents him moving: its great weight, and his having used it as a writing-desk for many years, has pressed it into the shape of a gigantic cooked hat. His pulse is very weak, and his feet oedematous, which will make it very hazardous to attempt its removal; but life is literally a burden with such an appendage, and he is willing to risk life for the chance of getting rid of it. Having no precedent to guide me, and knowing that expedition was his only safety, I determined to make it a case of decollation as much as possible, without attempting to save the penis even. He became insensible on the fourth day of mesmerising, and was drawn on the mattress to the end of the bed. Two men held up the mass in a sheet, extending its neck, and I took it off by a circular incision. The loss of blood was immense, and he immediately fainted, but recovered without great difficulty in a short time. After tying the last vessel, the mattress was again pulled back upon the bed with him upon it; and at this moment he awoke, and said that nothing had disturbed him. We had no time to spare, certainly; but I saw that the penis might have been spared, and in every subsequent case it has been so.

At the end of a year I reported to government that one hundred operations of all kinds had been performed by me in the mesmeric trance, without the patients knowing anything about it; and I offered to demonstrate the truth of my statement to any number of persons in whom the government had confidence. Upon this a Mesmeric Committee was organized by order of Sir Herbert Maddock, the Deputy Governor of Bengal, to witness and report upon operations to be performed by me before them. For this purpose I came down to Calcutta from Hooghly, and in the course of fourteen days performed seven mesmeric operations in the presence of the Committee; and this is the summary of the Committee's observations:—

"In all these cases, without exception, after the operation was completed, the patients expressed no knowledge or recollection of what had occurred, denied having dreamed, and denied having pain till their act"

rested to the place where the operation was performed."—(Mesmeric Committee's Report).

Among the operations performed before the Committee was a completely successful one of amputation of the thigh: but as this paper is devoted to scrotal tumors, I will proceed to the first very remarkable case that occurred after I came to Calcutta:—

A few days after the Committee had ceased to sit, a man came to the Native Hospital, where the Committee held their meetings, with an enormous tumor, and I put him under treatment.

Oct. 9th, 1846.—Horoondo Saha, aged 27, hearing that I was in Calcutta, came to the Native Hospital to-day with an immense tumor. It measured seven feet in circumference, and two feet round its neck. The disease began seven years ago with hydrocele, and its progress has been very rapid for the last three years. Although the tumor is actually as heavy as his whole body (he appears to be under 8 stone weight), his person is in tolerable condition, and his constitution does not seem much broken.

10th.—He was mesmerised to-day for the first time, for two hours, by two persons, taking one hour each. He slept profoundly, and was partially cataleptic. The best mode of managing the mass, described hereafter, was rehearsed without awaking him.

11th.—No mesmeric effects to-day, on account of his system being deranged by the periodic (?)* that very generally attends these tumors.

12th.—The mesmeric phenomena are less marked than on the first day. He is still feverish.

13th.—This day being excessively stormy I did not go to the hospital, thinking that the gentlemen interested in the progress of the case would not venture out in such bad weather. At 2 o'clock P.M. I received a note from Mr. Hume, a magistrate of Calcutta, and a member of the late Mesmeric Committee, informing me that the patient had that day exhibited the most perfect catalepsy, and that "I might have made minced meat of him."

14th.—The same appearances being present to-day, I proceeded to operate upon him. The tumor had been daily tied up in a sheet, to which a rope was attached rove through a pulley in a

* Omission in MS.

rafter. The penis was dissected out. The mattress was then hauled down to the end of the bed; his legs were held asunder, and the pulley was put in motion to support the mass and develope its neck. It was transfixed with the longest two-edged knife, which was found to be too short, as I had to dig the haft of the mass to make the point appear below, and it was removed by two semicircular incisions right and left. The flow of venous blood was prodigious, but soon moderated under pressure of the hands, the vessels being picked up as fast as possible. The mass, after half an hour, weighed 108lbs., and was as heavy as the man's whole body. During the whole operation I was not sensible of a quiver of his flesh. Dr. Duncan Stewart, Residency surgeon, and a member of the late Mesmeric Committee, held his pulse all the time, and had the best opportunities of observation: he has kindly furnished me with the following notes:—

Dr. Stewart's Notes.

"The time occupied in the operation was six minutes, including the application of ligatures to the spermatic arteries, and three or four other vessels that spouted. The arterial hæmorrhage was very small indeed, but the welling of blood at the moment of each transverse cut was appalling. The loss could not have been less than ten or twelve pounds. The patient remained throughout most perfectly still and motionless. I held his pulse the whole time, and counted it carefully. Immediately on the removal of the tumor it sank to zero; his face became pale, and cold sweat bedewed his forehead: it was not till his head was lowered that he recovered from the collapse caused by so sudden and great a withdrawal of vital stimulus from the heart and brain. The pulse gradually returned, and was found, when first counted, to be 120, very small, compressible, and intermittent, but there was not the slightest evidence of consciousness or pain.

"It was now found necessary by Dr. Taylor and myself to pour some wine and hartshorn down his throat; but, as he could not swallow in this state, it was allowed us to dash cold water in his face, blow in his eyes, and fan him; by which means he awoke from his trance, recovered sufficient sensibility to drink some brandy and water, and presently

subsided into perfect repose: the pulse, however, remained very weak, and settled at 100. The patient was then carefully removed to a clean bed: he passed a good night; the wound was stitched and strapped the following day; and, on visiting him this morning, I found him looking composed, and sleeping soundly, the parts looking well, and with every prospect of a most successful cure.

(Signed) "D. STEWART, M.D."

The operation was performed in a room full of spectators, including the Secretaries to Government, and many medical and unprofessional gentlemen. Although this operation was not performed before the Committee, it was ordered to be printed in their report by the Government.

The man is now well and fat.

On receipt of the Mesmeric Committee's report, which was printed by order of Government, the Deputy-governor determined to give me an experimental hospital for a year, which intention was thus intimated:—"So far has the possibility of rendering the most severe surgical operations painless to the subject of them been, in his honour's opinion, established by the late experiments performed under the eye of a committee appointed for the purpose, as to render it incumbent on the Government to afford to the meritorious and zealous officer by whom the subject was first brought to its notice, such assistance as may facilitate his investigations, and enable him to prosecute his interesting experiments under the most favourable and promising circumstances." (Letter of the Secretary to the Government of Bengal to the President of the Mesmeric Committee).

Forty-six capital operations were performed within the year in the experimental hospital; and, as the last operation has the novelty of having been performed by a native surgeon, I shall only give it,

Dec. 23, 1847.—Luckynaram Day, aged 37, mesmerised for the first time to-day. He bore pricking and pinching very well, and only a slight shiver of the skin took place when he was touched with a bit of live charcoal. He resisted all the usual means of demesmerising, but awoke instantly on having cold water squirted into his eyes.

4th.—Found deeply entranced to-

day: he was pricked and pinched with the same results as yesterday, and could not be roused by the loudest noises. Some sulphate of magnesia was put into his mouth with difficulty, as his jaws were firmly clenched, as usually happens. The tongue remained quite passive, and the mouth gradually closed again. His nose was also put into a bottle of carbonate of ammonia, and he inhaled the fumes like common air. Repeated attempts were made to mesmerise him: his eyes were rubbed, opened, and sharply blown into, without the least effect. He was at last awakened by his eyes being syringed with cold water; and the moment he came to his senses, he began to retch, and said that his mouth was disgustingly bitter—why, he could not tell.

5th.—In the same state to-day. This man was quite ready the first day, but the operation was put off at the request of some persons who desired to be present.

6th.—The operation was performed to-day by my assistant, Baboo Budurichanda, in the presence of Dr. Morat, and other gentlemen. The penis, having been first freed, an incision was made in the course of the right cord, which exposed a large hydrocele sac, on opening which a great quantity of bloody pus escaped. As this convinced us that the testis was useless, it was no farther sought for, but was removed with the mass, after the left one had been dissected out. The left testis was also found to be involved in a hydrocele; and, as the tunica vaginalis was much thickened and hardened, it was dissected off the testis and cord; all which he bore like a dead body. He was mesmerised a quarter of an hour after all was over; and, when asked, said he had slept soundly, without being in any way disturbed, and was as strong as usual. He could not be convinced that he had been operated on till the part was shown to him. On seeing it he was overjoyed, and begged me to pare down his legs in the same way, as they were both elephantoid.

Mr. O'Shaughnessy, Professor of Surgery in the Medical College, Calcutta, tells us that, since the discovery of ether and chloroform, "Mesmerism no longer deserves the serious consideration of the operating surgeon;" and this brings me to the question of the propriety of

administering chloroform in cases of large scrotal tumors. If my views are correct, they have a much more extensive application.

If chloroform is resorted to at all (and I very rarely require to do so), I see no reason to suppose that it will not be as successful in moderate-sized tumors as in amputations, &c. But in my last half-yearly report of the Mesmeric Hospital I asked:—

"Who would dare to remove scrotal tumors weighing from fifty to one hundred pounds, and, more, under the influence of any narcotic drug whatever? Not I, certainly; and I would earnestly dissuade any one from attempting it, or suffering it to be done to him. We all now knew that chloroform has a tendency to paralyse the heart, lungs, and brain; and it requires for doctors learning to be convinced that such exhausting operations can only be performed with a chance of success in cases where the vital powers are intact. When these tumors weigh above forty pounds, the loss of blood is so profuse that the pulse is usually extinguished on the spot, and it takes a considerable time to revive it; the brain is so exhausted by the sudden withdrawal of blood, that the patient generally faints, and awakes in a half delirious state, and, the stomach sympathising, vomiting also takes place, and hours elapse before the equilibrium of the sanguineous and nervous systems is re-established.

"In the last tumor of four pounds I ordered the solid clots to be gathered, and they weighed two and a half pounds; and, taking into account the serum on the floor, with the blood in the mass, I presume that about five pounds of blood were lost in as many minutes; and we have seen that Dr. Stewart estimated the loss of blood at ten or twelve pounds in the monster tumor.

"It must be evident to every one that persons fainting from this immense hæmorrhage would probably (rather certainly) never revive if the vitality of the heart, lungs, and brain, was lowered by any additional influence whatever."

A girl lately died at the Medical College here a few minutes after the extraction of an eye; and Mr. O'Shaughnessy, the operator, made the following remarks upon the case:—"The unfortunate result in this instance I am dis-

passed to attribute entirely to the insensibility produced by chloroform. The loss of blood was trifling, the time occupied by the operation was short, and there was no important organ implicated by the removal of the diseased parts. The child fainted; and, owing to the general insensibility, the attempt at reaction was imperfect, asphyxia followed, and she expired."

But, though *fainting, asphyxia, and death*, followed the administration of the chloroform, we are plainly told by Mr. O'Shaughnessy that this valuable drug possesses "no poisonous properties!" Mr. O'Shaughnessy more truly says afterwards, however, "that all narcotic drugs are open to the objection that they prevent the reaction necessary for recovery from injuries attended with great exhaustion of the system." Dr. Goodeve, who, as he told me, has known and believed in mesmerism for the last twenty years, and fully appreciates its practical and philosophic interest, on seeing me remove a seventy-pound tumor lately under the mesmeric influence, said, on witnessing the state the man was reduced to, that, if there had been a drop of chloroform in the man's blood, he would never have breathed again. In the very last operation on a worn-out man of fifty (weight of tumor eighty pounds), I emphatically pronounced him *dead*, although the operation was performed in two and a half minutes. He at last gave a gasp, however, and in ten minutes was out of all danger, and is about to quit the hospital. Does any one believe that this man would have ever gasped again if his blood had been saturated with chloroform, or his vitality in any way depressed? Mesmerism is not open to the same objections; *for it not only protects, but exalts the nervous system*, as we know from extensive experience both in medical and surgical cases.

My warning against the use of chloroform in large tumor cases no sooner reached Madras than, as if to establish my character of a true prophet, we immediately heard of a fatal case there; and the occurrence of another at Alexandria goes to confirm my predictions. I would on no account be aiding and abetting in giving chloroform in monster cases of scroval tumors, being convinced that the patient, if he cannot be

mesmerised, has a better chance of life if operated on in his natural state. In one case of eighty pounds, the man could not be affected by mesmerism, and I removed the mass in his waking state: he recovered very well. In another refractory case of fifty pounds, in which all the organs were sound, I would not give chloroform, and the man did perfectly well.

Calcutta, May 6th, 1850.

** Want of space has compelled us to omit from this paper some passages which appeared to us to have no immediate relation to the subject, as well as the long list of operations and the large accompanying illustration.

HYDATID DEGENERATION OF OVUM. BY DR. PUTNAM.

THE subject of this case is a young married woman. Her last child born three years ago. Menstruation since that time regular. Last menstruation occurred during last week in November. During the following three months, suffered from pain in the back, bearing down, and copious leucorrhoea. There was also an unusual degree of chilliness, which made warmer clothing necessary. The above symptoms were aggravated at the periods of expected menstruation. She was positive that she was not pregnant, because her sensations were different from those she had usually experienced during gestation.

On examination, at the end of the second month, the body of the uterus was decidedly enlarged. No special change in the neck. At the end of the third month the enlargement was found to have subsided. On the fifteenth of March—at the middle of the fourth month—hæmorrhage occurred, at first very slight, but gradually increasing and attended with pain until the twenty-third, when the hydatids were discharged. Hæmorrhage continued more urgently until checked by the use of ergot, but did not entirely cease for three weeks.

The hydatid cluster would more than fill a half-pint bowl, the separate vesicles being of various sizes, from a pin's head to half an inch in diameter. The usual term hydatid has been employed; but it is well known to be a vesicular disease of the ovum, resembling hydatids only in external form. In this case no fetus was detected, but the deciduous membrane was perfectly distinct.

—*American Journal of Med. Sciences.*

MEDICAL GAZETTE.

FRIDAY, SEPTEMBER 12, 1860.

WITHIN the last few years there has sprung up what appears to us to be a novel and dangerous interpretation of the law regarding charges of manslaughter brought against ignorant pretenders to the healing art. It has been long an admitted principle of law that the possession of a diploma confers no privilege, and that whether a man be a blacksmith or a fully qualified practitioner, the same rule is applied to determine the degree of criminality when death is caused by the ignorant or unskilful administration of medicines.

A new view of the law, and one which we believe to be most dangerous to the public, was, however, taken by Mr. Baron Alderson at the Liverpool Lent Assizes. In the case of the Queen *v.* Winterbottom, indicted for manslaughter, the learned judge is reported to have said, "The question for the jury to consider was, whether the prisoner had done what *he considered most advantageous* for the recovery of the patient? If he had done so, he ought not to be declared guilty of manslaughter. Unless the prosecutor made it plain that he was *not doing his best*, there was an end of the case: and if people employed unqualified persons to dispense medicines, they themselves must take the consequences." We had hitherto thought that one great object of law in a social state was to protect people from the results of ignorance and unskilfulness in cases in which they are incompetent to protect themselves. An uneducated man pretends to a knowledge of physic, promises largely, and prescribes an over dose of medicine, which kills his dupe, who has had no means of testing the validity of his pretensions, or ascer-

ing whether he was or was not qualified to practise medicine. According to this view of the law, no criminality would attach to such a person. It is most likely that the quack did what he thought was most advantageous for the recovery of his patient. The prosecutor cannot show that he was *not doing his best*; hence, according to Mr. Baron Alderson, the charge of manslaughter could not be sustained. These unlicensed practitioners of medicine destroy lives—not that they designedly adopt methods of treatment least advantageous (in their judgment) to the recovery of patients; not that they do not do their best; but because they use means of the proper and safe employment of which they are wholly ignorant. It is not, therefore, so much a question of *bona fides* as of ignorance, unskilfulness, and incompetency. If Mr. Baron Alderson's dictum be correct,—under what circumstances can an unlicensed and ignorant practitioner ever be convicted of manslaughter by the improper administration of medicines? We assert that it is equal to legalizing manslaughter at the hands of quacks. A man may prescribe a strong dose of aconite for another, and thus kill him. Any medical practitioner acquainted with the properties of this vegetable would know, *a priori*, that death would be just as likely to follow as if a loaded pistol had been discharged at the head of the patient. The new test for criminality, however, is, Did the prescriber consider that *he* was adopting a plan of treatment most advantageous for the recovery of the patient? Was he not doing *his best*, although that best involved the worst consequences for his patient? But then the learned judge throws out this piece of consolation to the public. If a person will employ an unqualified individual to prescribe aconite or hemlock for him, he must take the consequences! There is no giving it; this lies in

another man being foolish enough to take it at the recommendation of a person, before he has searched the lists of the different colleges in Great Britain to determine whether he is or is not qualified to practise.

In all other cases except physica, the *bona fides* of the pretender to a particular art, science, or mystery, avails nothing. Take the case of a man not licensed by the Trinity House to act as a river or coast pilot. He has perfect confidence in his own nautical abilities. In a difficult piece of navigation, he takes a course which he believes to be most advantageous for the security of the vessel: he runs the ship on a rock, and causes the loss of twenty or more lives. Would the conviction of such a person for manslaughter rest upon the bare fact, whether he was or was not doing *his best* at the time of the occurrence? If he was, then, according to Mr. Baron Alderson's law, there would be an end of the case. If the captains and crews of ships will employ unqualified persons to act as pilots, they must take the consequences. Let us test this principle by the recent case of the *Orion*. The mate who ran the vessel on the rocks of the Scotch coast did not take this fatal course with malicious intention: it was a course which he considered most advantageous for the vessel; he did his best; but he lost the ship, and with it many lives. According to the law of Mr. Baron Alderson, the mate, it appears to us, could not, under these circumstances, have been fairly held responsible for the fatal results. If passengers will embark on board of steam-vessels in which the captains and mates are unqualified to navigate them, *they must take the consequences.**

* The captain and second mate of the *Orion* were properly convicted, in the recent trial at Edinburgh, of causing death by neglect and unskillfulness. The exculpation of these persons from a charge of manslaughter was not made to rest on whether the mate had done *his best*, and

We do not think we have exaggerated the results to which this view of the law regarding manslaughter from the administration of medicines must lead. Our belief is, that it would not meet with the concurrence of the other learned members of the Bench. It is, however, exerting a pernicious influence, inasmuch as it is now quoted as a reason for the acquittal of every quack who is charged with manslaughter in poisoning another by an over-dose of some dangerous medicine. It was thus recently brought before a jury at Carlisle in a case recently noticed in our pages,* and no doubt influenced their verdict of Acquittal. With the dictum of Mr. Baron Alderson, and with the numerous acquittals before us, where there could be no doubt whatever that death was entirely owing to ignorance and unskillfulness, we may reverse the old adage, and proclaim, SALUS POPULI NON EST SUPREMA LEX.

PHYSICIANS AND APOTHECARIES.

At the late meeting of the Pennsylvania State Medical Society, among the many resolutions offered we find the following:—“Whereas it is asserted and confidently believed by a portion of the public that it is the practice of some physicians and apothecaries to enter into a collusive arrangement, by which the former are to receive a per centage upon all prescriptions sent to the latter, and in this way bring dishonour upon the medical profession: therefore Resolved,—That this Society regards all collusion between physicians and apothecaries, whether with a view to peculiar profit or patronage, as opposed to every principle of that moral code which the profession have adopted; and that no physician known to be guilty of such a collusion should be entitled to the confidence and professional intercourse of medical men.”—*Boston Medical Journal*.

whether he had taken that course which he thought most advantageous for the safe navigation of the ship! On these points there would have been a verdict in his favour. We quite agree with our contemporary the *Times* in the opinion that, had these men been tried by an English instead of a Scotch jury, they would have been acquitted, or, if found guilty, discharged with a reprimand and a caution to take more care at another time!

* See MED. GAZ. for August 30, p. 384.

Reblets.

Annales d'Hygiène Publique et de Médecine Légale. 1850. 1. *Sanitary condition of buildings for the poor.* 2. *Railway Accidents.* 3. *Bread.* 4. *Child-murder by blows on the head.* 5. *Compound poisoning.*

1. In the April number of the "Annales," the department devoted to medical police opens with a paper by M. Villermé, on the subject of *cités ouvrières*, or erections on the plan of the model dwelling-houses of the London "Society for the Improvement of the Labouring Classes." Such schemes for the elevation of the condition of the lower orders in towns have of late been occupying the public attention in France, Belgium, Hanover, and Prussia. Several establishments of this kind have already been opened in Berlin; and one is in course of erection in Paris. M. Villermé deprecates the form which these structures have assumed with us, and advises that of a series of small separate houses, along the side of a street, in preference to single barrack-like buildings; and with this the reports from Prussia agree. For reasons which, unhappily, are but too applicable to the majority of continental workmen, he proposes altogether to exclude unmarried persons from such establishments, in order thereby to avoid the risk of having them speedily converted into foci of turbulence, or nests of sedition.

2. This paper is followed by a report to the Council of Health of the Northern department of France, from a mixed commission, on the best means of lessening the number of accidents from the employment of steam as the motive power in manufactures and trades. The steam machinery in Lille, the great industrial centre of the North, gives occupation to 8170 work-people, about one half females, and one-sixth children. Amongst these, from the returns of three years, the commissioners calculate the annual number of accidents at 12·5 for every 1000; of which 10 are curable, 1·25 fatal, and 1·15 (1·25 ?) are found to involve the loss of one or more limbs;—an average which we must regard as a high one, when we consider that in 1840 the deaths by machinery, of all kinds, for the population of England,

only amounted to 00·94 in the 1000 (vide Sixth Annual Report of the Registrar-General).

The preventive measures recommended by the commission are confessedly the same as those which our own legislature has made imperative on the owners of silk, cotton, and woollen mills in Britain, in the Factory Acts of 1844 (7 Vict. cap. 16), in which, for once at least, where the public safety is concerned, we can reverse Sterne's boast, that "they manage these things better in France."

3. The paper which comes next is the conclusion of a report continued from the previous number, which embodies the labours of a commission of the Council of Health of Paris, appointed by the Minister of Commerce, for the purpose of determining the quantity of bread of the different sorts which a sack of flour should yield to the baker.

By the present communication it appears, that from the early part of the fourteenth century downwards, several successive administrations in France have instituted inquiries into this matter, with the intention of thereby legislating for the public interest, although without arriving at any very satisfactory results. Nor has the commission in question been altogether successful, having failed in coming to a unanimous conclusion on the principal point referred to its decision. As was, perhaps, to have been expected, the experiments undertaken by the commissioners were found only to yield them approximative results, while they showed conclusively that the yield of bread from a given weight of flour will vary usually with circumstances;—such as the quality of the flour itself; the relative proportions of gluten and starch contained in it; its hygrometric state; the amount of water employed in the working of it up; the temperature of the oven, &c.; and all this, independently of the loss in the transport of the article, and from the mixing of the different sorts essential to the manufacture of bread of good quality. The size and shape of the loaves have also been shown to affect considerably their ultimate weight.

Two other papers complete this portion of the Annals—the first on the insalubrity of rice fields; the other commences an inquiry into the effects of copper and its alloys on the health of the workmen employed in their fabrica-

tion. To this last we shall return when we have the completion of the inquiry before us.

4. The department of the journal appropriated to medical jurisprudence commences with an instructive narrative, by M. Orfila, of the proceedings originating in the case of the suspected murder, from blows on its head, of a female infant of three years, by its mother, Elizabeth Prat, or Sarret, at La Rochelle.

The infant, born before marriage, was known to have suffered much harsh treatment from its mother, who had denied it the ordinary comforts of life. Its death took place on the 9th of August, 1849, after an illness of thirty-three days, (during which it had no regular medical or other attendance), and was attributed to violence, on the part of its mother, on the 7th of the previous month. This woman, when judicially examined, ascribed the death of her child to the effects of an accidental fall from a child's chair, when it struck its head against a stone floor, the seat of the chair in question being 21 centimètres (8·267 inches) in height. The leading symptoms of the illness, as described by her, were occasional acute pains in the head, nearly constant stupor, irritability of stomach, and loss of power over the bladder. A woman who had seen the child during life had drawn the mother's attention to an unusual softness of the occiput.

On the 30th of August the body was disinterred, and inspected by MM. Chesnet, Meyer, and Gaudin, one of whom had visited the infant before its death. The corpse was considerably attenuated, and decomposition had made some progress. The head was inclined to the left side: the cranial bones were unusually moveable: over the parietal, and a part of the occipital bones, the scalp was thickened, softened, and infiltrated with a blackish fluid. There was a vertical fracture of from six to seven centimètres (2·36 to 2·75 inches) in length on the right side of the occipital bone, without any corresponding injury of the scalp: the brain was a dark, fetid pulp: the dura mater, which was uninjured, was deeply coloured in its interior over the whole of its left side.

The report of the inspectors was favourable to the accused. The condition of the abdominal viscera, which

were natural, negatived the charge of starvation. The appearances about the head were, they conceived, as likely to have been caused by a fall as by a blow. The fracture of the occiput was, in their opinion, indirectly the cause of the child's death, by giving rise to effusion of blood, and consecutive inflammation of the brain, the symptoms attending which tallied with the mother's statements.

On the case being subsequently referred to the same parties separately, for further consideration, and their attention having been called to a point which it seems they at first had overlooked, their evidence came to disagree to some extent. M. Meyer stated, that while the injuries on the child's head were not cadaveric, but the effects of direct violence during life, they were as likely to have been caused by blows as by a fall. The injury in the occipital bone he considered to have been a counter-fracture. On examining the skull, both with and without a lens, he found that portions (of the outer surfaces?) of the left parietal, and of the left side of the occipital bones, had lost their natural smoothness and polish, which he attributed to the effects of disease. M. Gaudin's opinion was so far different, that while he agreed with M. M. that the occipital injury was a counter and not a direct fracture, he was positive that it could not have been caused by a fall from the child's chair, and considered that the roughness of the bones on the left side of the skull had been the effect of violence, such as a blow or fall on the head. M. Chesnet, who, in doubts of his own competency to form a correct judgment, had, in the interim, seen and consulted with MM. Orfila, Velpeau, and Roux, in Paris, now deposed that he found a difficulty in admitting a fracture of the occiput by a counter-stroke, and that the loss of polish of the parietal and occipital bones was most likely a purely morbid change.

In these circumstances the legal authorities requested the opinions of MM. Orfila, Roux, and Velpeau, on the leading points in the case. These gentlemen did not, however, coincide in their views any more than the local practitioners. Thus, while Roux and Velpeau viewed the occipito-parietal lesions on the left side of the head as merely cadaveric, and as the effect of putrefaction, Orfila's opinion was, that they had been produced during life. Again, Roux and

Velpéau decided that the occipital fracture had been caused by a direct blow or fall on the part, and not by a counter-stroke; whereas Orfila thought it by no means impossible that forcible violence applied to the left side of the head would have fractured the right side of the occipital bone. Once more Roux and Velpéau considered that the fracture might have been occasioned by the fall, and in the circumstances indicated by the accused; while Orfila could hardly admit that it could have resulted from so slight a cause.

At the trial in February last Sarret was acquitted.

In the remarks which he has appended to the case, Orfila contends that, from the circumstances adduced on the trial, the injuries on the head of Sarret's child could not have had an earlier date than the 30th of July, and that his positions in opposition to his colleagues are thus very much strengthened.

One or two points in the above narrative appear to us to call for a passing comment. The softened and infiltrated scalp, and the dyeing of the dura mater in a body twenty days buried, and in the month of July, are but equivocal tokens of the mode in which the injuries on the left side of the child's head were produced, and far more so of the period when they originated, whether during life or after death. But if we connect with these appearances the change which the bones had undergone on the same side of the head, we are led to refer the whole to the effects of disease, in part at least, if not entirely. We cannot, certainly, admit, with some of the witnesses, that a degree of roughness (un chagrinement), and a loss of polish (un dépoli) of the cranial bones, visible to the naked eye, could be produced by putrefaction alone. The opinion of Orfila, running counter to that of his two eminent colleagues and one of the provincial reporters, to the effect that the fracture at the occiput could hardly have resulted from a mere fall of eight inches, especially when we are aware that no additional impetus was communicated to the body in falling, is undoubtedly in harmony with the teachings of experience without a single exception. We are, however, compelled to differ from this veteran medical jurist when he assumes that the fracture might have been a counter one. The question here mooted as to the likelihood of violence

to the head of a child giving rise to a counter-fracture of the skull, appears to us to be a new one in legal medicine, and one which leaves room for difference of opinion, in the entire absence of facts in our surgical works bearing directly on this point. But the very silence of surgical writers on such an occurrence as counter-fracture of the cranium in children, seems to us strongly hostile to Orfila's decision, and to favour that of his opponents. Besides, the flexible character of the infantile skull, and the unfused state of its separate bones, must be considered as highly unfavourable to the transmission of such an impulse from one side of the cranial vault to the one opposite to it as takes place in the production of counter-fractures. Chaussier's experiments, where direct fractures only were produced on dropping dead children from a height of eighteen inches, point in the same direction: and we may be permitted to add to the same purport, in so far as it goes, the fact, that in ten cases of fractured cranial bones, resulting from blows or falls in persons under puberty, which we have had occasion to examine, each was found to have been a direct fracture.

5. Passing over a discussion on the legality or illegality of omitting to register the births of still-born infants,—a point on which there have been of late, in France, some conflicting legal decisions,—we come to two cases of compound poisoning. The first of these is a mere notice to the following effect;—viz. that a person was tried and found guilty of the crime of poisoning a female child with alum and sulphate of iron, at Toulouse; and that M. Filhol, professor of chemistry there, detected in the matters submitted to him for examination a sufficient quantity of these substances to have caused the infant's death. The other case is one of alleged poisoning, tried at the Orne assizes, in July, 1849. The deceased, a labourer, died in four days with all the symptoms of irritant poisoning; and after seven months' interment, M.M. Chevallier and Lassaigue procured 0·21 grammes (8·26 grains) of arsenic from portions only of the stomach, liver, kidneys, and intestines, forwarded to them. The man's wife and paramour, besides being charged with administering arsenic to their victim, were also believed to have given him a few weeks before his death a quantity

of a substance sold under the name of "phosphoric paste for destroying rats," and which Chevallier and Lassaigue discovered to be a preparation of ordinary phosphorus diffused through flour paste. This substance seems, however, to have occasioned but a temporary illness, though the remains of the soup in which it had been administered, when thrown out, had destroyed some fowls who had eaten it.

The accused parties were acquitted by the court, but on what grounds we are not informed.

Amongst the "varieties" at the end of the journal we find a very instructive analysis, by M. Briere de Boismont, of the report of a commission lately appointed by the king of Sardinia for the investigation of the subject of cretinism as it exists in that country.

Descriptive and Illustrated Catalogue of the Histological Series contained in the Museum of the Royal College of Surgeons of England. Volume I. Elementary Tissues of Vegetables and Animals. 4to. pp 305; 18 Plates. London. 1850.

The medical profession are under deep obligations to the Royal College of Surgeons of England for having collected a most excellent library of medical books, and for having, by their fostering care, raised the Hunterian Museum to be the most noble establishment of the kind the science of medicine can boast of.

By the application of the microscope to anatomy and physiology, the domain of these sciences has of late years been much extended. A museum of large objects only being, therefore, no longer a complete exposition of their present state, the Royal College of Surgeons have wisely and liberally created a microscopical department in the Hunterian Museum. The nucleus of this was a few preparations made by Mr. Hewson, originally belonging to Mr. John Hunter, and some purchased a few years ago from the executors of the late Dr. Todd, of Brighton. By the addition of Mr. Quekett's valuable collection, and by presents from various sources, the series has now become, under the auspices of Mr. Quekett, very extensive, and may be considered invaluable.

All the great facts in the minute structure of vegetables and animals are

bodily presented in the bare and simple severity of nature.

To Mr. Quekett, and to the liberality of the Royal College of Surgeons, who have enabled him to carry on his scientific labours, the sciences of anatomy and physiology are thus especially indebted.

The present illustrated catalogue, prepared by Mr. Quekett, descriptive of the results of a part of these labours, will be particularly acceptable to those who, living at a distance, have not the opportunity of directly examining the microscopical collection. The pictorial illustrations may be almost considered as duplicates of the principal preparations.

Report of the Committee of Internal Health on the Asiatic Cholera; together with a Report of the City Physician on the Cholera Hospital. 8vo. pp. 180. Boston. 1849.

This report opens with an account of the very efficient sanitary measures actively put into execution by the municipal authorities of Boston previously to and during the visitation of the cholera in 1849. Among the first steps taken were the fitting up of a cholera hospital, and the organisation of a medical staff for domiciliary visitation. The description given of some of the worst localities of Boston will prove the need of these sanitary measures. Several woodcuts exhibit courts and alleys, and places of the most wretched condition, as the residences of American and Irish human pigs.

From the 29th of June to the 15th of November there were under treatment in the hospital two hundred and sixty-two patients: of these, one hundred and sixty-six died. The opinion of the medical officers of the hospital was against the contagious nature of the cholera. With regard to treatment, we find the same statements as are contained in every other report, that all depended upon the early use of remedies: of these a list is mentioned, with the commentary upon each that it was *useless*! The pathological changes found after death were carefully noted in thirty-three cases, and presented the same morbid appearances that have been observed elsewhere.

We consider this Report as a useful contribution to the facts of which the history of cholera consists, and as a

document which will doubtless be of service on some future occasion.

Remarks on Epilepsy, or Puerperal Convulsions. By GEORGE KING, Esq. Pamphlet, pp. 16. Worcester: Deighton. 1850.

THIS is a reprint, from the Provincial Medical and Surgical Journal, of a paper in which the author shows that puerperal convulsions are more frequently the result of "mental impressions and uterine irritability than of active congestion of, or determination of blood to, the brain; and, therefore, that the abstraction of large quantities of blood in such cases is highly injurious, and more calculated to destroy the energies of the system than to restore or to relieve them." We fully concur in the author's views. We have met with many cases of puerperal convulsions in which depletion would have inevitably aggravated symptoms which were relieved by an opposite plan of treatment.

Mr. King's pamphlet, though short, is to the purpose, and on both accounts merits perusal.

Every-day Wonders; or, Facts in Physiology which all should know. 12mo. pp. 137. Van Voorst. 1850.

THIS is a novel attempt to convey a general knowledge of the structure and physiology of the human body to the mind of a child. The facts are well selected, and put in such a form as to be adapted to the capacity of a child under the guidance of a teacher. All technicality is avoided, and the chapters contain anecdotes bearing on the subjects to which they refer. The descriptions are made clear by engravings on wood. Some may object that it is absurd to attempt to teach a child such subjects; but, whether expressly taught or not, children grow up with some loose ideas respecting the uses of a heart, lungs, stomach, and liver. This little volume does not attempt too much, but it has the excellent use of giving correct views of physiology; and it will thus serve to prevent the growth of false and erroneous ideas in the youthful mind. We recommend it as a useful and instructive book for children.

Introductory Lecture to a Course of Military Surgery; delivered in the University of Edinburgh, May 1st, 1850. By SIR GEORGE BALLINGALL, M.D., F.R.S.E., Professor of Military Surgery in the University of Edinburgh. Pamphlet, 8vo. pp. 8. Edinburgh. 1850.

Hospital; an article written for the Cyclopædia of Practical Surgery. By SIR GEORGE BALLINGALL, M.D. &c.

THESE papers contain much interesting information; the former more especially with reference to the history of military, the latter with regard to that of civil hospitals. In the last-named of these two pamphlets the reader will find very full and judicious observations on the construction of hospitals, and on their internal arrangements and general economy. We trust that Sir George Ballingall's suggestions will receive from the founders and architects of hospitals that attentive consideration which they fully deserve.

CASE OF POISONING BY CORROSIVE SUBIMATE. BY B. W. MCCREADY, M.D.

THE patient was a young woman, who swallowed about 3 oz. of a solution of corrosive sublimate of the strength of one drachm of the salt to the pint of alcohol, and had consequently taken about 22 grains. The case, according to Dr. M'C., is worthy of note for the following reasons:—

1st. The general symptoms bore no relation to the amount of local disease. After the first few hours the pulse was moderately full, and not above 90, and the temperature not unusual. This might be connected with the state of the blood after the occurrence of the suppression of urine; but it was present previous to the coming on of the suppression.

2d. The amount of inflammation and ulceration of the large intestines was such as occurs commonly with severe dysentery, and yet the dysenteric symptoms were slight, much slighter, indeed, than commonly occur in cases of poisoning by corrosive sublimate.

3d. Suppression of urine continued from Monday until Saturday, the day of her death, and yet the patient retained her senses, and was easily roused, exhibiting no signs of cerebral disturbance except slight drowsiness and some tendency to wandering.—*New York Journal of Medicine*, July.

Proceedings of Societies.

BIOLOGICAL SOCIETY OF PARIS.

Monthly Summary, May and June 1880.

Existence of a Rhythmical Movement in the Crop of Birds.

M. BROWN-SEQUARD stated that he had observed a regular rhythmical movement in the crop and oesophagus of birds, and that these movements become more energetic when the bird is asphyxiated. The average normal rate of movement is from ten to twenty in the minute.

Pathological Anatomy.

M. BOUCHUT related the particulars of a case of osteosarcoma of the pelvis occurring in a woman forty-two years of age, which had run its course to a fatal termination in four months from the first appearance of any symptoms. The effects were manifested in the interruption to the functions of the lower extremities proceeding from implication of the structures of the hip-joint.

M. GUBLER detailed a case of intra-cranial encephaloid tumor. The chief symptoms were hemiplegia, with contraction of the limbs. The tumor, of the size of a hen's egg, was seated in the middle lobe of the right hemisphere of the brain.

M. MAZIER related a case of fibrous tumor of the cerebellum, producing convulsive movements and paralysis on the side corresponding to the seat of the tumor.

M. DAVAINÉ related a case of compression of the thoracic portion of the oesophagus by a tubercular mass in the posterior mediastinum of an Ape.

M. VERNEUIL presented two specimens of abnormal *diverticula* attached to the intestines.

Malformation of the Hands.

M. GUBLER exhibited a plaster-cast of a hand presenting a deficiency of the last phalanges. The fingers were united by a web-like extension of the integuments.

On Hematozoa.

M. FILLON stated that he had detected *filaria* in the blood of nine individuals of the *corvus frugilegus*. They presented the aspect of minute elongated filaments slightly flattened at their extremities, one of which was more rounded than the other; but an anterior and posterior extremity could not be distinguished. Their transparent bodies exhibited no trace of internal organization. These *filaria* were found in the heart and large vessels.

M. FILLON regarded them as the young of the *strongylus* met with in the stomach, liver, and lungs.

Anencephalic Fetus.

M. OLIER transmitted the description of an anencephalic fetus in which spina bifida also existed through the whole length of the canal. The spinal cord was flattened and bifurcated at its superior extremity; the spinal nerves were not in connection with the cord; the limbs were well formed; the weight of the fetus was from three, to three and a half pounds; the neck was entirely wanting, and the small head was attached to a depression on the trunk; the thorax was well formed; the lower jaw, which was disproportionately large, extended to the epigastrium; the nose was short; the eyes closed, and the globes directed upwards; these were extremely small, and unprovided with retine; the upper part of the head was wanting from above the orbits, and was replaced by a fibrous membrane, extending backwards to the trunk at the third lumbar vertebra; the viscera were normal as to structure and position. No motion or sign of life was manifested by this fetus after birth, although they had been felt previously.

FATAL CONSEQUENCES OF AN APPARENTLY SLIGHT INJURY TO THREE FINGERS OF THE LEFT HAND. BY DR. KRAMER, OF ASCHERSLEBEN.

A YOUTH, aged 12 years, had his hand crushed by a piece of wood falling upon it from a cart as he lay upon the ground, having just before fallen down.

The last joint of the index, middle, and ring fingers were much crushed, the nails nearly detached, and the capsular ligaments of the second and third phalanx of the middle finger ruptured. The fingers were carefully bandaged, &c., and the patient directed to apply cold water, and attend daily on Dr. Kramer.

Such accidents as this are of common occurrence, and in most cases the cure is complete in two or three weeks, with, at the worst, the loss of the nail, and perhaps the last joint of the finger. The prognosis to this effect was here given. For a few days the case went on well; the nails of the index and ring fingers were detached, the febrile reaction was moderate. Granulations took place as in ordinary cases.

Things did not go on so well, however, with the middle finger. The last joint became gangrenous. The finger was amputated, but the gangrene extended.

Nine days after the receipt of the injury he felt pain in his neck. On the following day tetanus manifested itself, and he died two days later.—*Casper's Wochenschrift*. x

Hospital and Infirmary Reports.

KING'S COLLEGE HOSPITAL.

*Case of Lithotrixy—Stone of large size—
Steele symptoms after operation—Death.*

AN operation for crushing the stone is one which is now so frequently put in force, that an ordinary case of this nature would hardly be worthy of special report; but it happens every now and then that circumstances arise, in connection with the operation of lithotrixy, which change this mode of treatment from a somewhat simple and easy process into a complicated and difficult affair. Under such circumstances, he who carefully watches a case of lithotrixy from beginning to end, or who in any other way makes himself acquainted with its particular features, will gain most useful instruction.

We have selected for this report a case of stone in the bladder, in which lithotrixy has been performed by Mr. Fergusson, and which will afford both instruction and interest. The case is as follows:—The patient, a stout man, aged about sixty, came into hospital, at the last week of June, with symptoms of stone in the bladder, from which, in a somewhat severe form, he had suffered for several years. It was never discovered, however, that he had a stone, until Mr. Fergusson examined him, when it was readily detected. Mr. Fergusson considered the calculus to be of considerable size, and thought it probable that there were more than one present. There was no stricture of the urethra, and no irritability of the urinary passages when the sound was used, and there was no evidence of disease of the bladder. Under these circumstances, then, Mr. Fergusson deemed it proper to attempt the operation of lithotrixy, although the stone was doubtless of considerable size; yet as the parts were so little sensitive, he considered it a fair case for that mode of proceeding which must be looked upon as giving the patient less hazard than that of lithotomy.

Consequently, on Saturday, June 29th, the patient was put under the influence of chloroform, and the stone, which was evidently of considerable size, was without difficulty broken up; Mr. Fergusson using a modification of that most simple and effectual lithotrite worked with the rack and pinion.

On the following day the patient had much pain and difficulty in passing his water; several fragments came away, but with difficulty and irritation; he was ordered opiates and fomentations.

On the fourth day, July 2nd, he was suffering terribly, being continually seized with the desire to pass his water, which came away with excessive difficulty; and it was evident that it was hindered from passing by a piece of calculus blocking up the urethra. He had obtained no rest, and was in a high state of febrile excitement; his skin being hot, his tongue covered with a thick white fur, and his pulse running upwards of 120. He also complained of great tenderness at the lower part of the belly. Mr. Fergusson at his visit passed a catheter down the urethra; but it was stopped about the membranous portion by fragments of stone sticking there: he endeavoured, as far as he was justified, to push them back into the bladder, and thus relieve the man from the retention he was evidently suffering under; but after a cautious trial he was foiled. He then introduced the straight instrument with three blades for the removal of fragments from the urethra, and by this means, after some long and patient manipulation, he was enabled to remove a considerable quantity of broken-up fragments which had conglomerated together, and thus hindered the free passage of the urine. A catheter was now passed, and a considerable quantity of bloody and ammoniacal urine drawn off, to the great relief of the patient. He was ordered to have hot fomentations, with laudanum, and to take a morphia draught at night time. On the next day he was much easier, although he was troubled still with a frequent desire of making water, which comes away with difficulty: he has passed more fragments.

At Mr. Fergusson's visit on the 4th he was still suffering somewhat, but not nearly to the extent that he had been; he could pass his water with more freedom, and the febrile excitement was not so great, and he did not complain of pain in the abdomen. Mr. Fergusson did not use any instrument. The patient continued to suffer a great deal of inconvenience from the irritability of the urinary passages, and the pain which the fragments of stone produced in their way through the urethra. On the 7th he passed a very large and rough fragment, which caused great pain.

On the 9th he had passed a very good night previously, from the opiate which he had taken; but when Mr. Fergusson visited him he was suffering greatly from pain and irritation, and there was considerable difficulty in passing water; the pulse was 120; tongue still much furred; appetite bad. A catheter was passed down the urethra, but was again stopped by fragments of calculi sticking. Mr. Fergusson therefore used the urethra forceps and extracted them, and afterwards drew

off some urine. Chloroform was then used, and the lithotrite was introduced, and more of the stone broken up: he was ordered a warm bath.

15th.—This poor man has been suffering very much since the last report; some small fragments have passed away from him, but not many; he still has great irritation of the bladder and urethra, and his urine comes away with difficulty. His general condition is very bad; he has lost flesh, and has become very feeble; his pulse amounts to 120, and is weak; tongue very much coated with a dirty fur; appetite very bad; takes only fluid nourishment, in the shape of beef tea, &c.; he has been ordered some gin and water, which he relishes, and which gives some relief to the irritability of his urinary organs. He gets sleep at night by powerful opiates, but he is delirious at times.

16th.—Mr. Fergusson, at his visit to-day, finding that no fragments were coming away, and that the man was suffering so much from their retention, had him placed under the influence of chloroform, and injected the bladder with tepid water: this viscus was in a very irritable condition, however, and the injected fluid was repelled immediately, and it was found necessary to inject more, and compress the urethra, thus preventing its exit. The lithotrite was then introduced, and several fragments were broken up: the patient was ordered to be well fomented.

19th.—The patient has had no relief since this operation; on the contrary, he has been in great pain and distress, and it is evident that he is getting weaker day by day; he has passed but very few fragments, and these have caused great irritation and difficulty in urination: independent of this he suffers severely from the irritation of the skin of the scrotum and perineum, and an abscess has formed on the right groin, and has been opened. The pulse to-day is very feeble, upwards of 120; tongue is much furred; the patient has been slightly delirious; he is only able to take fluid nourishment, which consists of beef tea, brandy, and gin; he is also taking plenty of diluents; and a draught every six hours, containing, Infus. Lini, ʒiss.; Liq. Pt. Mxx.; Tr. Camph. ʒss.

23d.—This poor man has been suffering terribly ever since, in consequence of the great amount of irritation produced by the retention of the fragments of the calculus; at times the pain which he suffered was excessive, causing him to groan out aloud; he was evidently sinking under this protracted irritation; his pulse became very rapid and feeble, his countenance dull and heavy, and he took little nourishment, and that chiefly in the form of stimulants: he

lingered on in this manner, and died last night.

Post-mortem examination.—The bladder and front part of the pelvis, together with a portion of the urethra, were removed; in clearing the bladder from its connections on the left side, a considerable quantity of pus was seen to issue after a stroke of the knife, and on closer examination it was found to come from out of the left urter, which was distended nearly to the size of one of the fingers; its lining membrane was excessively inflamed, and here and there coated with pus; the pelvis of the left kidney was also found to be greatly dilated and inflamed. The right kidney was inflamed, and in addition there were two or three purulent deposits in its substance.

The bladder was considerably contracted, much thickened, and its mucous membrane in a state of inflammation; the viscus contained upwards of an ounce of urine freely mixed with pus. The fragments of the calculus were numerous; some of them of small size, whilst others were large; one piece was as big as a chestnut; they were coated with phosphatic deposit. The urethra was very much inflamed.

This case, which has turned out so unfortunately, tends to show the uncertainty which surrounds the operation of lithotritry, and the difficulties the surgeon labours under in his decision as to what case of stone is or is not fit for such a treatment to be pursued; for here was an instance in which there was every reason to expect a successful termination. There was no evidence of disease of the bladder or kidneys; there was no obstruction in the urethra, and the urinary passages altogether were not by any means sensitive to pain after the introduction of the sound. It is true that the calculus was of considerable size; but its size was not such as to prevent its being broken up, and Mr. Fergusson, as he stated at the time of the operation, was chiefly induced to try lithotritry in this case, although the stone was large, in consequence of his having just before most successfully broken up a calculus of the same calibre in the person of an elderly gentleman: only two sittings were required in this instance, but all the fragments, which, when collected, filled a two-ounce phial, were passed with scarcely any irritation, although some of them were large and became so impacted in the urethra that Mr. Fergusson found it necessary to remove them by the scoop. It was the success of this case, together with the great similarity between the one and the other, that induced Mr. Fergusson to try the same procedure in his hospital patient; and there was nothing to lead one to

the supposition that it would not turn out as favourably: the event, however, proves that it is impossible to estimate beforehand what will be the result after lithotomy, even in cases to which the operation seems to be well adapted, and when the greatest amount of care and skill has been used: it is this very uncertainty which has made some surgeons shy of performing it. We unfortunately in this country possess no extended statistics of lithotomy, and therefore it is impossible with accuracy to ascertain whether the mortality is greater after this proceeding, or after the apparently more formidable operation of lithotomy: there can be no doubt that death not unfrequently follows lithotomy,—in cases, too, when everything appears likely to do well; and, when death does ensue, it is generally preceded by far greater sufferings than when it takes place after lithotomy.

Stricture.

In our report of July 12, we related part of the history and progress of a case of impermeable stricture, which had been complicated with stone in front of the neck of the bladder, and with fistulous openings in the perineum and abdominal wall, through which openings most of the urine came away. We shall now continue the case from June 1, on which day, as stated in the last report, Mr. Fergusson had been able, after dividing the stricture from within, to introduce a No. 2 silver catheter into the bladder.

June 3d.—No. 4 was introduced into the bladder without difficulty.

5th.—No. 6 was passed, but it gave the patient some pain. He is still low and weak, although he is much better than he was; his appetite is not very good, and what he takes is chiefly fluid nourishment.

18th.—This poor man has been improving so much since the last report, that he has been able to go out and walk about the yard at the hospital. The wound which existed in the perineum is not yet healed up, but that in the abdominal wall is now almost entirely closed up, and does not allow urine to come away. Although the passage along the urethra is pretty well opened, the urine does not pass in a stream, but only dribbles away. With the object of keeping the back part of the canal well patent, Mr. Fergusson, at his visit to-day, passed the elastic bougie through the perineal opening into the bladder, and at the same time passed a No. 4 silver catheter down the urethra.

27th.—There has been some difficulty in still further dilating the urethra. Since the last report, Mr. Fergusson was scarcely able to pass a No. 5 catheter through the

stricture, which has evidently contracted somewhat since the internal incision was made: to-day, both No. 4 and No. 5 were passed without difficulty, but still there was considerable resistance in all other respects: the patient is much improved. The fistulous opening in the belly has entirely healed up for some days; the opening in the perineum is not yet quite closed; the general health of the patient is much improved, although he is still weakly, but so well as to be able to go out and take a trip down the river in a steam-boat.

July 5th.—As there has still been some difficulty in getting larger instruments through the stricture, Mr. Fergusson had the patient placed under the influence of chloroform, and by means of the *urethrotome* before mentioned, divided the stricture much more freely than he had done on the previous occasion, so that he was enabled to pass a No. 8 instrument, which was ordered to be kept in the bladder.

9th.—The patient suffered some slight uneasiness after this operation; but the symptoms subsided in a day or two; and as the instrument last introduced was found to be quite loose in the urethra, and producing no irritation, it was taken out and a No. 10 was introduced with the greatest ease, and then left. From this period everything went on satisfactorily; the instrument was kept in the urethra so that it should become fully dilated; the hardness which was some time ago so marked in front of the urethra gradually disappeared; and the fistulous opening in the perineum, although it did not quite close, nevertheless much diminished in size. Under these circumstances, the patient was discharged on the 22d, as he was anxious to breathe his native air, which in his weak state was considered to be the best thing for him to do.

ST. GEORGE'S HOSPITAL.

REPORTED BY DR. BARCLAY,
Medical Registrar.

Extra-uterine Fotation.

SUCH cases are by no means of very rare occurrence; and the feature of peculiar interest in the present instance is the age which the fetus had attained before fatal hemorrhage occurred, which seems to have been in great measure due to the occurrence of partial hemorrhage at an earlier period, when coagula were formed round the sac in such a manner as to arrest any further hemorrhage for a considerable period.

Hannah ———, *et. 29*, admitted into

St. George's Hospital, August 10th, 1850, under the care of Dr. Bence Jones: is very anemic-looking; the catamenia have not appeared since May; she has been married ten years; had one child nine years ago, and has not been pregnant since. Her present illness commenced in March last with an attack of pain in the abdomen and sickness, accompanied by rigors followed by hot flushings and perspirations; the bowels are generally confined, often as much as three or four days, and were so at that time. This pain recurred with severity about five weeks ago, and she has never been entirely free from it since, although paroxysms sometimes occur of greater severity than others. She stated that she had an illness of precisely similar nature two years before, in which her medical attendant informed her that an abscess had formed and discharged itself *per vaginam*. The attacks of pain now last generally some three or four hours, and are often accompanied by vomiting of greenish and yellow matters.

On admission, she was much blanched in appearance; had a countenance expressive of much anxiety. She complained of pain almost constant in the abdomen, and lay with her legs bent; her tongue was rather coated; pulse 108, not strong nor sharp; bowels costive; the whole abdomen was tender, but not extremely so; was not tympanitic or generally hard, but a distinct fulness and firmness were observed at its lower part on the right side; and a short distance to the right, and below the level of the umbilicus, a hard, roundish, firm tumor was easily discovered; but the tenderness on pressure over this was extreme. The bowels have acted this morning, but with pain and forcing. Eight leeches were ordered to be applied to the right side of the abdomen, and afterwards a hot fomentation. She was also ordered to have 2 grains of Calomel, and $\frac{1}{4}$ grain of Opium every six hours, and a saline the alternate six hours. A common injection to be administered to-morrow morning.

11th.—The bowels have acted scantily, with pain and forcing; the passage of the urine also gives pain; the tongue is cleaner; pulse 104; pain easier. The calomel to be repeated three times a-day; to go on with the draught and the fomentation, and to have a dose of castor oil, followed by common injection, to-morrow morning.

12th.—The bowels acted freely before the injection was administered, and she feels much easier now; pulse 100, rather weak; tongue cleaner, and moist; the motions are fluid and darkish. To repeat the calomel twice daily, and to continue the draught.

15th.—Has continued to go on well till this morning, when she had another severe

attack of pain, lasting three or four hours, leaving the abdomen more tender, and a feeling of pain shooting through to the back. She has the appearance of being more blanched since her admission. (She had slight epistaxis yesterday.) The tumor appears to be more superficial, and very tender; the left side of the abdomen pretty flaccid, and free from pain; no tympanitis. To have 3 grains of Grey Powder, and 1 grain of Opium every night; a linseed-meal poultice to the abdomen, and a morphia draught whenever the pain returns.

17th.—Has slept pretty well; a dose of 3 drachms of Castor Oil ordered yesterday has produced a motion full of small lumps, but with little pain or forcing; pulse 92; tongue tolerably clean; much less tenderness to-day. Nothing further observed of the tumor, except that there seemed to be a separate fulness at the hypogastrium, quite distinct from the tumor, near the umbilicus. She was now to take 3 grains of Calomel on alternate nights, and $\frac{1}{2}$ oz. of Castor Oil on alternate mornings, with a morphia draught always at bed-time.

19th.—The motions following the first dose were not lumpy, but dark and bilious-looking. After a tolerable night, she was this morning seized by a return of the pain in the abdomen: the fomentations, which were always found to have given her relief on previous occasions, were immediately had recourse to, and the morphia draught given. In about two hours after the seizure, when the apothecary was sent for, she was already cold and collapsed: stimulants were given without benefit, and she died with all the appearance of internal hæmorrhage.

Post-mortem examination 26 hours after death.—When the abdominal cavity was laid open, a large quantity of dark coagulated blood was found lying superficially to the intestines, besides a considerable amount of dark blood-stained serum which had gravitated to the lower part of the peritoneal sac. In the midst of these coagula, and a very short distance to the right of the mesian line, and rather superficially, was found a very perfect fœtus of about three months old; the umbilical cord was perfect, and led through masses of coagulated blood to the remains, which were still nearly entire, of a sac in which the amnion and the placenta could be made out very perfectly, as well as a very distinctly enlarged Fallopian tube, which formed, to a considerable extent, the investing membrane of the sac: to the outer side, layers of fibrinous, partially decolorized coagula, which were adherent to the surrounding intestines and the uterus, seemed to indicate a previous hæmorrhage which had been arrested; the uterus itself was very considerably enlarged, was of a

eastliſh texture, and was lined by a very well-marked deciduous membrane; the ovary of the right ſide contained a very diſtinct true corpus luteum; the other viſcera were all tolerably healthy, and were only remarkable as being very much blanched, which was moſt eſpecially the caſe with regard to the lungs, which were of a pale aſhy colour.

A queſtion of intereſt ariſes in all ſuch caſes, whether it be at any time poſſible to make a clear diagnoſis, and whether any operation might be reſorted to with a hope of ſaving the patient's life. In the foregoing inſtance ſuch a diagnoſis was ſcarcely poſſible. The circumſtance of the patient's having been nine years without giving birth to a child, rendered her pregnancy at that time highly problematical, while her anæmic ſtate was perfectly ſufficient to have accounted for the abſence of the catamenia. In addition to this, her own account is to be taken into conſideration, that her illneſs began in March, and that ſhe had continued to menſtrate till May, even ſuppoſing no account is taken of a former illneſs, of the particulars of which ſhe was probably miſinformed. Of theſe former illneſſes it is now difficult to form any idea, as no poſt-mortem traces of them were left; but it is very eaſy to conceive that the period of the recurrence of the ſymptoms which ſhe eſpecially marked as five weeks before her admission, very probably correſponded to the firſt hæmorrhage which had been arreſted; and it is highly probable that at that period the fœtus eſcaped into the abdominal cavity, where its growth continued, from the circumſtance of the placentas and cord remaining entire.

There can be no hope of ſaving the patient's life by any other means than an operation; and the development of the uterus, ſimultaneouſly with the growth of the fœtus, while it ſtill does not acquire the ſame ſize as the normal uterus which contains the fœtus within its walls, is perhaps almoſt the only means we have of aſcertaining the exact nature of the caſe, which muſt, under any circumſtances, be ſurrounded by difficulties, and the more ſo as they generally terminate fatally at a very early period.

APOTHECARIES' HALL.

NAMES of gentlemen who paſſed their examination in the ſcience and practice of medicine, and received certificates to practice, on Thursday, 5th September, 1850:—John Lee Jardine—John White Keyworth, Axton Tirrold, Berks—Walter Monday, Olveſten, Glouceſtershire—John Fourness Brice, Pontefract, Yorkſhire.

Medical Trials and Inquiries.

SUPREME JUDICIAL COURT, BOSTON.

March 19, 1850.

REPORT OF THE TRIAL OF DR. WEBSTER
FOR THE MURDER OF DR. PARKMAN.

*Before Chief Juſtice Shaw and Juſtices
Wilde, Dewey, and Metcalf.*

[Continued from page 436.]

DR. WOODBRIDGE STRONG, *ſworn*.—I have been in practice ſince 1820, in Boſton. While I was a ſtudent of medicine I accepted every opportunity I had to practice diſſection. I have ſince I came to Boſton diſſected many a body, and meant always to be thorough in my work. I have been at different inſtitutions alſo, and am more diſpoſed towards ſurgery than to anything elſe. I have been obliged ſometimes to burn ſome remains. Once I had a pirate given to me by the Maſter, when I was living in Cornhill. It was very warm weather. I wanted the bones, and deſired to get the fleſh deſtroyed. I had a common fire-place. The body was not a large one, but muſcular, and had ſome fat. I made a fire of wood and fleſh, and kept poking and keeping up a roaring fire. It burned all night, and up to three in the morning, and the maſs was not half burned up. Coal is bad to burn it with; but wood is good; though little fleſh can be burnt at a time. Any dry wood, ſuch as pitch pine, would do very well to burn. The ſmell is difficult to allay, and the operator would be apt to be found out through it.

[Witness deſcribed the remains as others had done.] The diſſection of the breſt was done in the uſual manner of anatiomiſts; and no one who had not been in the habit of ſeeing diſſections could have done the thing in the way it was. The muſcles between the bones were cut through as far as four inches,—the diſtance the fleſh was taken from off the ribs. The ſkin at the puncture was taken away; and ſome one ſaid, when I inquired about it being a ſtab, that it was not. I noted that the aperture ſeemed as if a ſtab had cauſed it when the muſcle was tight. It takes a very ſharp knife to make a clean cut, when the ſkin is not tight. I ſaw a perfectly clean cut made, ſo as nearly to graze the rib, which I thought muſt have been given when the body was alive. I thought it might have been the means of death. If a perſon was ſtabbed to the heart, ſomething would depend on the poſition of the wound

as to the hæmorrhage. In this case it would have been internal. In the case of these remains, the parts were peculiarly bloodless. They seemed as much so as meat that is seep in the examples. I observed the hair to be gray or whitish. The skin had lost the elasticity belonging to the young subject, and its thickness denoted age. I judged from the remains, from the hair, and the condition of the cartilages, that the person must have been between fifty and sixty. The body was unusual in its formation, from its narrowness across the shoulders, in proportion to the pelvis, and the tenuity in the upper part. It corresponded, in these respects, to the late Dr. Parkman's general appearance when alive. He had a peculiar appearance. There was nothing dissimilar to what I would have expected to have found in his remains; on the contrary, there were many points of resemblance.

Cross-examination.—I resided in Cambridge Street, No. 5, at the time. I went there in 1842, and have had acts of kindness from Dr. Parkman. We communicated together, and visited each other; and I have the pleasure of thinking he was one of my friends. The colour of the hair on the body was the same as that on his head and face. It is not so invariably. I don't recollect whether he had whiskers or no. If there had been any disproportion of the parts found, to indicate that they belonged to different bodies, I should have seen and noticed it. I am in the habit of looking at the human body regularly, and noting beauties or deformities. I did not see the remains the first day I called; but I saw them on the second day. I think I saw Dr. C. T. Jackson there, and I held some conversation with Dr. Lewis. I never burned up a body in a furnace. I think that the intensity of the heat would be as great in a stove as it would have been in the furnace I saw in Dr. Webster's laboratory. The stove I saw in the same room was a better article to use. I saw but one furnace in the College. I have used a common stove, when dissecting, to consume human flesh, with anthracite coal, which I think is not so good as wood. A large coal fire would burn up human flesh. It might be possible that the wound might have led to the heart, and no blood flowed outward. This could be accounted for by the form of the wound, which might collapse; and also by the sudden stopping the circulation of the blood, such as cutting the aorta would effect.

Direct examination.—There is a diversity of opinion among medical men regarding the weight or amount of blood in the human body. It might average forty pounds in a healthy man.

By a Jurymen.—I noticed the body perhaps more particularly, on account of its being supposed to be that of Dr. Parkman.

By Mr. Clifford.—Both the back and front of the body looked as if it might have been Dr. Parkman's.

Dr. FRANKLIN S. ALMSWORTH, sworn.—Am demonstrator of anatomy in the Medical College. All subjects must come through my hands. Keep a record of all anatomical materials. My attention was called to this record at the time of finding these remains; found I had all the subjects and materials I ought to have; all were accounted for. Examined the remains, and came to the conclusion, from the remains themselves, that they had never been sent to me or to the College for dissection. All subjects sent for dissection are injected with fluid, to preserve them from decomposition. I use a solution of arsenious acid, or chloride of zinc, with a saturated solution of alum and saltpetre. Dr. Webster has no connection with the anatomical department. My impression was, that the person who cut up these remains had no anatomical knowledge. The person who did it might have seen a body cut up, but I should doubt whether he ever took a knife in his hand to do it.

Dr. CHARLES T. JACKSON, sworn.—Have given attention to chemistry for several years. Went to the Medical College on Saturday, the 1st of December. Went with Dr. Gay. Dr. Lewis was there, and made some preliminary examinations. Dr. Gay and myself undertook the chemical part. There were shown us the remains of a human body. I took some observation of them; there was nothing to indicate that they had been used for the purpose of dissection. The body indicated some knowledge of anatomy on the part of the person that had divided it. There was no hacking about it; the thigh and hips were disarticulated neatly. I heard the testimony of Drs. Gay and Stone, and coincided with them. Was acquainted with the late Dr. George Parkman; he was very frequently at my office: he was a tall and slender man, and was otherwise peculiar. Did not see anything in the remains dissimilar from Dr. Parkman. There was nothing differing from what I should expect in Dr. Parkman, in the muscular developments of the lower parts. The flesh showed indications of chemical application.

[The report of Dr. Jackson was now read.]

I, Charles T. Jackson, being duly sworn, depose as follows:—I am, by profession, a physician and chemist. On the 1st day of December, 1849, I was requested by James H. Blake to accompany Dr. Martin Gay in

making some chemical and other examinations at the Massachusetts Medical College, in the city of Boston; and at four o'clock in the afternoon of that day I went with Dr. Gay to the Medical College, and there met Dr. Winslow Lewis, jun., and others, with the coroner of the county of Suffolk, and the jury of inquest. We made a general examination on that afternoon, and adjourned until Sunday morning, when we resumed our examination, Dr. Jeffries Wyman being associated with us, and aiding in the examination of the bones found in the furnace of the chemical laboratory; and also took chips of wood on which we had been shown certain brown stains, which were submitted to Dr. Wyman to examine. Dr. F. S. Ainsworth also assisted us in the selection of fragments of bone from the cinders of the furnace. The bones found by us were in a mass of cinders and ashes which had been removed from the furnace by the police officers, and were placed in a box, and had the appearance of having been exposed to fire. They were much broken, and were, in some instances, partially fused into the cinders. We identified at that time the following bones: right os calcis, right astragalus, tibia, and fibula, phalanges resembling those of the ring or middle finger; coronoid process of the lower jaw, and numerous fragments of a human skull; a human tooth with a hole in it, appearing as if it had once been filled by a dentist's operation; three blocks of mineral teeth, with platinum rivets in one of them entire, but wanting the gold plate on which mineral teeth are usually set. A pearl shirt-button was also found in the ashes of the furnace, and was partially calcined. Numerous little copper cups found in one of the laboratory drawers; they did not appear to have been burnt. Many pieces of glass were also found among the slags and cinders of the furnace. Masses of metal were also found, which proved by analysis to consist of, in 25 grains: tin, 12.19; lead, 11.95 = 24.14. Hence, it is evidently tea-chest lead. The cinders of the furnace, pounded and washed, yielded globules of gold, some silver, and a little copper. In the portion of slags and cinders worked by me, thirty grains of gold were found. My attention having been called to the state of parts of the human body which Dr. Lewis was examining, I took portions of the skin and muscles from the thorax, and tested them by reddened litmus paper, and found those parts strongly charged with alkali. I found the discoloured thigh also had been imbued with alkali, and stained by the fire. I took portions of skin from the arm and thigh, and carried them to the laboratory, and ascertained by chem-

that the alkali contained in them was potash, mixed with a very little sea-salt. The skin in several places appeared to have been corroded by the joint action of potash and heat. The thorax had singed hair on it, showing the action of fire, and probably of flame, since the burning was superficial. I found no alkali in the interior of the thighs, nor in the flesh beneath the skin of the thorax. The muscles of the cut surfaces at both ends of the thorax were strongly alkaline. I observed that the skin near an opening near the sixth and seventh ribs was quite tender, and the edges of the opening into the thorax were corroded, as if by potash. I dissected out the arteries and some of the veins of both thighs and of the leg, and gave them to Dr. Martin Gay. I subsequently saw Mr. Richard Crossley in my laboratory, in my presence, examine a portion of one of these vessels, with the adhering muscle, for arsenic and zinc, and saw that no trace of those substances was found. The spots on the walls, floor, and furniture showed us, were committed to Dr. Jeffries Wyman, who cut out chips from them in my presence. A pair of slippers was submitted to us by the officers, and Dr. Jeffries Wyman cut pieces off from them in my presence, and took them away with him. Dr. Martin Gay took portions of the cinders and metals for examination, and his results should be compared with mine, in order to ascertain how much gold was found among the cinders. CHARLES T. JACKSON.

Attest, J. L. ANDREWS.

I was instructed by the Attorney-General, on the occasion of the presentment of the Grand Jury, to take possession of those articles found at the Medical College, which were left with Dr. Gay. Went to his house and got them. Took them to Mr. Crossley. (Had made a previous examination of them with Dr. Gay, and turned them over to him for further examination.) Mr. Crossley is in my employment, and occupies a part of my laboratory. My test, as to whether there was alkali in the body, was satisfactory to me. The action of potash on the human body softens the flesh, and dissolves it after a while; and when heat is supplied, it dissolves it very rapidly. With suitable apparatus, the time taken to dissolve a human body with potash would depend upon circumstances; if it were cut up in pieces and boiled, the flesh would dissolve in two or three hours. For this, it takes of potash nearly half the

; and if the whole were very large kettle. To entirely (he weigh-), it would take of potash. Exa-

mined Dr. Webster's laboratory. The largest kettle that was there was a tin boiler, with a thick copper bottom, such as is used in boiling clothes. If the body was cut up in pieces, it would have been adequate to dissolve it; but it would not hold the whole body, or the thorax. The next best substance to potash for dissolving a body is nitric acid; but potash is the best. Nitric acid would require a double portion; it would take the whole weight of the body, of nitric acid, to dissolve it. The effect of it, when applied to a body, would depend entirely upon caloric: if gently heated, it would give off very little gas; and, if boiled, a very large quantity. The odour of nitric acid is very disagreeable, but it is not necessary that the gas should be confined for the safety of the operator; an open vessel would do. I saw nothing large enough to dissolve any considerable amount of matter at a time in the laboratory. There were in the laboratory several bottles containing one or two pounds each, or perhaps more, some of nitric and some of muriatic acid, nearly full, on the window near the staircase. Did not examine all the bottles, but think there was not more than ten pounds of it altogether. There was on the wall (which was painted white), and on the staircase side of it, drops of green liquid. (The stairs were those leading from the back room in the rear of the lecture room.) I sent to my laboratory and got some filtering paper, which would absorb the green liquid. Dr. Gay took that paper and absorbed some of it, and carried it home with him. Since I received the things that Dr. Gay had, I have examined that paper, which I recognised to be the same paper with the green fluid in it, and found that green fluid to be nitrate of copper. The drops were very abundant in quantity, extending all along the stairs, from top to bottom, and on the adjoining wall. Was satisfied that it was nitrate of copper; think it was spilt on the stairs, and did not run down from the top. The drops were more abundant at the bottom than at the top of the stairs. Nitrate of copper attracts moisture from the air, and remains in a liquid state a long time; the stains were in a fluid state when I was there. Nitrate of copper has an astringent acid taste, like copper. With regard to the spots of blood, I was requested by the Government to make an examination of them; but considering the blood a more proper subject for microscopic observation, I transferred the duty to Dr. Wyman. Have had no experience of the effect of nitrate of copper on blood. A pair of pantaloons was discovered, with spots of blood on them. I think it was on Sunday Dr. Wyman cut out pieces from them. A power-

ful microscope is the only test to discover small spots of blood. Slippers were also found with appearances of blood on them. Dr. Wyman cut pieces from them also. Several punch pieces of copper were found in one of the drawers in Dr. Webster's laboratory. Others were found, much thinner, under the furnace, having upon them nitrate of copper,—showing that the oxide of copper had probably been introduced into nitric acid, for the purpose of making nitrate of copper.

	Grs. Gold-
I found in that given to me . . .	45 $\frac{1}{8}$
Dr. Gay found	47
And in a piece brought to me by Mr. John L. Andrews, the se- cretary of the coroner's inquest, I found	81 $\frac{1}{8}$
Total	173 $\frac{1}{8}$

The market value of this gold would be 6 dollars 94 cents. There is some gold still remaining in the blocks of teeth. Think I did not extract all the gold from the furnace; there might have been some left.

Mr. Bemis.—Is there any change in the appearance of the bone by being in fire in connection with gold, giving any indication of the proximity of the gold to the teeth?

Dr. Jackson [examining the bone].—It is the same colour that we see in the slag where the globules of gold are found—a pink colour produced by oxide of gold. It is on a block of artificial teeth. The contents of the ash-pit were taken out,—a part of them while I was there, the remainder not. The bones indicated that intense heat had been applied. Anthracite coal had been used at a high temperature. I have known the defendant for twenty-five years; have studied with him, and been on terms of communication with him. I have noticed the knife [exhibiting the sheath-knife] in Dr. Webster's laboratory, when I studied in the old Medical College. The College was removed to Grove Street in 1846. We found whiting and fresh oil upon the knife, as if there had been an attempt to clean it. This was on Monday or Tuesday. One of the officers called my attention to it. I scraped it off, carried it home, and analysed it. The whiting was not dry, but moist, and soft as putty. I did not notice the handle. Dr. Parkman was about my height, which was five feet and eleven inches.

Cross-examination.—The back, one side, and the two ends of the remains, were covered with potash, but no other part of the thorax. The thigh-bone was smoked, and the skin was softened, as if by potash and heat. If the bones had been separated

from the flesh. I think they could have been dissolved in half a day. There was nothing at all in the large boiler I mentioned before, when I saw it. I suppose the whiting had been used in cleaning the knife-handle. Tried the effects of nitrate of copper on wood, and it produced the same stains that we saw on the stair-case. [Here a piece of wood, with the above stains on it, was shown to the Court.]

RICHARD CROSSLEY, *sworn*.—Have given attention to chemistry for thirteen years. Have examined the blood-vessels, at the request of Dr. Gay, to ascertain whether they were, as usual, injected with arsenic acid and chloride of zinc, and found neither of these substances present. These are the injecting substances used at the Medical College. I coincide with Dr. Jackson in his opinion about the nitrate of copper.

DR. NATHAN C. KEEF, *sworn*.—Have been in the practice of dentistry for thirty years. Give attention both to natural and artificial teeth. Knew Dr. George Parkman as early as 1823. When I was a student to Dr. John Randal, Dr. Parkman was frequently there; and on one occasion was quite sick, and Dr. Randal attended him. When he recovered he was at Dr. Randal's very frequently, and I was acquainted with him at that time. As early, I think, as 1826, he employed me as his family dentist; and since that time, whenever he needed assistance, I have been the person on whom he called. Was shown the block of mineral teeth by Dr. Lewis. This was on my arrival from Springfield in the cars, the Monday after Thanksgiving, about one or two o'clock. I recognised them as the teeth I had made for Dr. Parkman in 1846. Dr. Parkman's mouth was a very peculiar mouth in many respects; differing in the relation that existed between the upper and lower jaw so peculiarly, that the impression left upon my mind was very distinct. I remember the peculiarity of the lower jaw with great exactness. The circumstances connected with the teeth being ordered were somewhat peculiar. The first question asked by Dr. Parkman, when the teeth were ordered, was, "How long will it take to make them?" I took the liberty to ask why he was so particular to know. He told me that the Medical College was to be opened, and that it was necessary for him to be there, and perhaps to speak; and he wanted them by that time, or else he did not want them at all. That time was a very short one. The peculiarity of the mouth made it a case requiring as much skill as could be used. I began to do it as soon as possible; gave a large part of my attention to it from day to day. In consequence of these circumstances, and the

shortness of the time, and the close application I gave to it, I remember very distinctly what was done, more than in ordinary cases. I proceeded in my usual mode to take the impression. The first step was, to take an exact *facies* of each jaw with wax, metal, liquid plaster, &c. A plate was made from that; and the next step was, of course, to ascertain the relation between the upper and the lower jaw. A model of the lower jaw was made from an impression taken with wax, while in a plastic state; and by means of this the lower plate was fitted. The upper plate was fitted in the same manner. [Dr. Keef exhibited the original plates, which fitted to the models.] These plates were made before the gold plates, to ascertain if there were any defect in the models. When the plates were fitted to his mouth, I requested him to close it until I satisfied myself as to the suitable distance. A great irregularity on the left side of the lower jaw of Dr. Parkman gave me great trouble in getting this up. Each set of teeth was made in three blocks, and then joined to the gold plate. There were spiral springs that connected the two sets of teeth, to enable the patient to open his mouth and close it with less danger of the teeth being displaced, as they would have been without the springs. There was an accident which injured one of the teeth in the front block, and delayed the finishing of them until near the end of the night before the opening of the Medical College. My assistant was Dr. Noble. When I next saw Dr. Parkman, he said that he did not feel that he had room for his tongue. In order to obviate that difficulty, I ground the block of the lower jaw on the inside, to make it lighter, and furnish more room for the tongue. This grinding, at that time, was not accomplished with so much ease. The teeth being on the plate, we could not grind on a large wheel. We had to grind on a very small wheel. This grinding removed the pink colour that represented the gums, and also the enamel from the inside of the lower teeth. The beauty of it was defaced by this grinding. The shape left by the grinding was very peculiar, because of its being ground on a small wheel, smaller than itself. I saw Dr. Parkman frequently. The last time I saw him was, as near as I can remember, about two weeks previous to his disappearance. He called late in the evening, about ten o'clock. It so happened that, not being very well, I had retired for the night. The person who went to the door, seeing Dr. Parkman, asked him in, and went up and told me that it was he. I sent word to him that I would come down as soon as I could. He told me his trouble. I took his teeth, both upper and

lower, examined them, and put on a new spring. He staid about half an hour; when he was ready to go home. I had no more professional intercourse with him at all. I went into the country to pass Thanksgiving at Longmeadow, and returned on the Monday morning after Thanksgiving. Arrived home, I was told that Dr. Lewis wanted to see me; and he presented me with these remains of mineral teeth [showing them], with the request that I would examine them. On looking at them I recognised them to be the same teeth I had made for Dr. Parkman. The most of the upper portion that remained was the block belonging to the left side of the lower jaw. Several other parts had been very much injured by fire. I proceeded to look for the mould upon which these teeth were made, put the metal upon its proper place, and it fitted exactly. There is sufficient left of these blocks to identify the place where they belonged. There is no mistake. [He then showed the mould and remains of teeth, &c.] All the pieces having been found, there were five pieces, which fitted to their exact places. The only piece that could not be identified might or might not have been right; but it was supposed to be right, as there was no reason that it should not be so. [The blocks of teeth, &c. were here shown to the jury by the witness, and afterwards to the Judges. During the progress of Dr. Keep's testimony, the Court and the witnesses were affected even to tears, and Dr. Keep, particularly, was overcome with emotion.] I found imbedded, more or less, with these mineral teeth, some very minute portions of gold, which is termed cancelled, being peculiar to the jaw-bone. I saw the teeth in the doctor's head the last time I saw him, in conversing with him. The presumption is very strong that these teeth were put in the fire in the head. Such is the nature of these mineral teeth, that, especially if they have been worn, they absorb small particles of water; when suddenly heated, the surface becomes closed, and the water becomes steam, and there would be a report, with an explosion. I have known such explosions to take place on heating teeth that have been worn; and when they have been worn recently, the explosion is always sure to take place if heated rapidly. If, while in the head, they were put into the fire, only a small portion would be exposed to the heat; and as the temperature would be raised so gradually, the water would have time to escape; and this accounts, in my mind, for the teeth not being cracked, excepting the front teeth, which would have been most exposed. I have found fused into the remains of teeth portions of the natural jaw. All these

teeth were exhibited to me at the same time.

Cross-examination.—My first impression, on seeing the teeth shown me by Dr. Lewis, was of the circumstances which I have related. Do not think I have been burlesquing up my recollection since they were shown me. Knew them for myself, without examining the mould; but I did examine them with the mould. The mould of Dr. Parkman was preserved, as moulds usually are, for future use in case of accident to the teeth. I heard of Dr. Parkman being missing before I went into the country. First time I heard of his being missing was the first night it was advertised in the papers.

Dr. LESTER NOBLE, sworn.—I was an assistant of Dr. Keep, in 1846, and remained in his service until 1849. Am now pursuing my studies in Baltimore; am a student at the Baltimore College. Recollect working upon teeth for Dr. Parkman; it was in the autumn of 1846. [Here he was shown the mould of Dr. Parkman's teeth, and recognised his handwriting on it.] Yes, this is my handwriting.—“Dr. Parkman, in October, 1846.” I did recognise these teeth. Was called to the Attorney-General's room; the blocks were brought in under seal, and there I examined them. They were delivered to me for safe keeping; and I have kept them in my pocket since, until they were called for to-day. The circumstances accompanying my recognition of them were, in the first place, the general shade of the block, which was the same that I remember to have worked upon for Dr. Parkman. Also I found on the inside a surface which appeared to be ground. At the moment I saw it, I recollected that this block of Dr. Parkman's teeth had been ground in the same way; that I saw Dr. Keep grind them. It was after the doctor had worn them, after he had been to the Medical College with them, that he said his tongue was incommoded. I have every reason to believe that the blocks were Dr. Parkman's teeth, and no reason to believe that they were not; have as good reason to believe it as any other fact. I have not the slightest doubt that they were the blocks I worked upon for Dr. Parkman. We were obliged to be very prompt in making the teeth to the time of an appointment by Dr. Parkman. They had to be ready at the time, because Dr. Parkman was sure to be there at the very moment he appointed. The time of the appointment of their being finished was when he wished to attend a meeting at the Medical College. An accident which occurred in blocking spoiled a part of the front block; it occasioned the necessity of remaking it, which of course caused delay, so that we had to work upon

hem a large portion of the night; remember that I worked upon them almost all night. We got them finished just in time for Dr. Parkman to go over to the College with them. I want myself, in order that I might see, when the doctor had occasion to peak, how well he used them. Think it was in the early part of November that the Medical College was opened. I rather think he did not speak; merely, when complimented for his generosity by Governor Everitt, he acknowledged it with a bow; yet I am not confident but what he said a few words. I understood that he had been liberal in a donation, by furnishing the ground, &c. The Medical College was opened in the early part of November. It was in the early part of my studies that I worked on Dr. Parkman's teeth; I began about the 11th of September. We first make the impression in wax. I made the moulds of these teeth in the wood, and made the metal casts. Could not tell precisely how much time was spent on them: it must have been quite a number of days. Have put blocks of teeth into the fire, to see how easily they would crack, and I have never known them not to crack; they may be heated up gradually and cooled with perfect safety. Coincided with Dr. Keep as to his opinion. Some time after the fitting of the teeth—about a year, I think—an accident happened to them; they bent together, and they had to be re-annealed to the pattern. I had to heat them for this purpose, and the mark of the blow-pipe still remains.

[To be continued.]

Correspondence.

INFLAMMATION AND ABSCESS OF THE UTERINE APPENDAGES.

SIR,—Since the publication in your valuable weekly journal of my paper on "Inflammation and Abscess of the Uterine Appendages," my attention has been directed to some very interesting cases which appeared in the Transactions of the Provincial Medical and Surgical Association for 1841, contributed by Mr. Wainwright, of Liverpool. Although, in the remarks accompanying them, the writer considered any observations he could make "must necessarily be of a speculative character," yet, as he apprehended that such "cases have usually been regarded as examples of either pueral abscess, or some other disease of this region already brought under the notice of the profession," and published them merely "in the hope that some attention may be

attracted to the subject," I think that, although he failed in developing the subject, and that Deherty in 1842, Churchill in 1843, and others subsequently, have been more successful; yet he having, of British writers, been, as I believe, the first to call professional attention thereto, is in my opinion deserving of honourable notice; and I accordingly regret I did not see that volume of the Transactions before.

I am

Your obedient servant,

M. JENNETTE,

Surgeon to the Birkenhead Hospital.

Sept. 9, 1850.

Medical Intelligence.

EPIDEMIOLOGICAL SOCIETY.

Dr. Babington's Speech, made when he proposed the Establishment of the Epidemiological Society at the General Meeting held at the Hanover Square Rooms on the 30th of July, 1850.

I FEEL that I should best consult the interest of the Society which we have met to form this evening, if, after the excellent, the admirable speech which has just been delivered by his lordship, I were simply to read the motion placed in my hands, and say nothing; but I have undertaken a task, and, in justice to myself, I must go through with it.

Gentlemen—Before I proceed further I wish to make one observation. It has been said and believed by some that I am the founder of this Society. Certain expressions in some of the medical journals have given rise to this impression. I wish to set myself right with the public on this head. I have no claim to be considered as the founder of this body: that merit belongs to our worthy Secretary, Mr. Tucker.

In common with others, when he called upon me I responded to his call, and was anxious to aid, as much as in me lay, to forward the views of such a valuable institution. I think no better time than the present could have been chosen for the formation of this Society. With the exception of some few sporadic cases, we are at present free from cholera, or any other epidemic—free from that fearful proximity which is so apt to disturb our powers of reflection.

Physical science, too, has made remarkable progress within the last half century; so that we can approach the investigation of epidemic diseases, as it were, under new circumstances. Animal chemistry is a new science. Fifty years ago it was almost un-

known. Meteorology, too, is quite a new science. The microscope, also, has done wonders; and such are the improvements in its construction, that its powers may be said to have only lately been developed: while physiology, human and comparative, has made most astonishing progress. This, then, is the time for the establishment of such a Society as ours; for, with these new instruments to aid our researches, we can hardly fail to add to our knowledge.

Gentlemen—It has been said, and by clever men too, that great discoveries are not to be made by bodies of even the most scientific men; that they do not result from the combined efforts of many, but that they rather spring from the genius of single labourers, who feel themselves called by the force of enthusiasm to the work, or by their peculiar circumstances are led to concentrate some thoughts upon particular subjects; that the formation, therefore, of a Society such as this is superfluous, seeing that individuals will of themselves come forth and elucidate those matters for the investigation of which it is formed.

This is partially true, but not wholly so. There are investigations which it requires the combined efforts of many to make on a sufficiently grand scale to be useful. Besides, we do not purpose to interfere with individual exertions, but rather to aid them, by placing materials at their disposal, such as books and instruments,—by publishing their productions, and by making them acquainted with the scientific researches of men of other countries.

Granting that individuals for the most part *make* discoveries, societies *arrange* them, and render them practically useful. The advantages of combination are almost too well known to need illustration. Take, for example, the variation of the compass, a knowledge of which is so essential to safe navigation. How could it have been determined but by the combined observations of many simultaneously made? Take, again, the theory of storms: Col. Reed could never have demonstrated the truth of this had it not been for the information and assistance afforded by numerous observers stationed in all parts of a great circle on the ocean, and simultaneously recording physical facts.

Gentlemen—We have a large subject before us in the investigation of the causes of epidemics, and especially difficult is that branch of it which relates to contagion. If we pronounce an opinion in favour of the doctrine of non-contagion, and that opinion be correct, we are enabled at once to take off the fetters of commerce imposed by quarantine, and thus to grant a great boon to mankind; but if that opinion be erroneous, how fearful is our responsibility in

so doing! We admit the seeds of death into the heart of populous cities, and the most awful results—the destruction of myriads of human beings—may follow.

This, then, is a momentous question, and one about which there is a vast difference of opinion among medical men; and it will be for us, aided by the new light of science, to elicit the truth from the conflicting testimony submitted to our judgment. One gentleman to whom I wrote, requesting him to join our Society, felt so strongly on this very question that he declined doing so,—on the sole ground that it would engross all our attention, yet without any hope that it would after all be settled. We must, therefore, be cautious, being thus warned, not to suffer it to claim more than its due share of our labours.

There is another almost equally important question,—that of the incubation of diseases,—of how long, for instance, scarlet fever remains in the affected before it makes its outward appearance: again, as connected with this subject, how long a habitation where such disease has prevailed remains capable of imparting it to newcomers. Were I called upon to answer such a question in the case of scarlet fever, I really could not speak with anything like certainty. Others may no doubt have much more experience than myself in this matter; but I candidly own that I could not determine the point, even within the wide limits of a month or two.

Then, with regard to the treatment of epidemic diseases, we have a wide field open to us. If we instance cholera, we know that a vast amount of thought has been expended upon the subject; but still we have no leading principle,—no guiding circumstances,—no collection of facts sufficiently ascertained to enable us to come to any safe and certain conclusion.

It by no means follows, however, that we never shall do so. There was a time when it was a question quite unsettled whether bleeding in yellow fever was beneficial or otherwise. It is now determined, from the testimony borne by a vast collection of facts, that bleeding is fatal in that disease. There is not a recent book that you can take up on the subject in which you do not find it condemned. And how was this question set at rest?—Why, by individual medical men carefully marking the results of bleeding in numerous cases, and comparing their observations. Had we but a single fact in the treatment of cholera as certain as this, it would be interesting and valuable; but we cannot affirm that even this is the case.

The learned Doctor then dwelt upon the necessity of the Society having ample funds at its disposal, in order to enable it to make

inquiries in all parts of the world, to organise scientific investigations in different departments and in various parts of the globe, and to give publicity to the results of its manifold labours.

One good at least (continued the Doctor) must arise from our exertions under any circumstances—namely, that the public will feel that the medical profession are not apathetic, but actively employed in using their best exertions for the benefit of their fellow men.

I may mention, before I sit down, that it is proposed to fix the annual subscription at one guinea. Of course it is not intended to limit members to that sum. If the laity who may not be able to assist in any other way feel inclined to increase that amount to any extent which their means, and the great objects to be accomplished, may warrant, we shall feel most grateful for such aid.

The Doctor concluded by moving the first resolution.

UNIVERSITY OF ST. ANDREWS—MEDICAL
EXAMINATION PAPERS, AUGUST 1860.

First Examination.

1. Explain shortly the nature of chemical affinity, and give two or more examples.
2. In what manner is phosphorus usually obtained, and what compounds does it form with oxygen?
3. Explain the nature of the change effected upon sugar during the vinous fermentation.
4. Give one of the usual processes for the preparation of iodide of potassium.
5. Classify the most important mineral waters according to their composition, giving one or two examples of each variety. Describe briefly their actions and uses.
6. Mention the effects, uses, and mode of administration of elaterium, strychnia, monkshood, aconite [aconita, *Ph. Ed.*], gallic acid, nitrate of silver, the iodides of mercury, and the arsenious solution of the British Pharmacopoeias.
7. What is copaiva? Describe its actions and uses. Write (without using abbreviations) a Latin prescription suitable for an ordinary case of gonorrhoea, and containing copaiva and spirit of nitrous ether.

To be translated into English.

Sonorum immensa varietas est; quippe quorum vel levissimae mutationes et conjunctiones ab acuta et docta aere percipiuntur. Insignis quoque eorum vis est in animum hominis, et ideo in corpus. Variae animi affectus, tristes insuperis et laetæ, inspirant; neque hominum ex hoc inexhausto fonte puræ et suavissimæ velup-

tates hauriunt. Auris vero, quæ vocatur musica, non omnibus datur; cujus rei ratio hæcenus latet. Nullo modo pendet ab acutiore vel obtusiore auditu. Semisurdi nonnunquam musicæ sâtis periti sunt et amantissimi, quæ bene audientes parum sæpe frutuntur: et vir, auditu integro manente, aurem suam musicam, quam eximiam habebat, subito et sine nota causa amisit. Neque ratio ulla est cur defectum talem inæquali per bimbas aures auditui imputemus. Facultas ipsa Naturæ donum est, nullo studio acquirenda: cultu vero et usu, non secus ac reliquæ hominis facultates, mirum in modum acuitur et roboratur. Auditus fere omnium sensuum sæpius vitatur: quod profecto nil mirum; scilicet, quia organum habet delicatissimum, et ex plurimis et valde minutis partibus compositum.

Second Examination.

1. Give a classification of the different kinds of food, stating the uses of each class in the animal economy. What do you consider to be the quantity of solid food necessary for a healthy adult? Give the symptoms, and appearances after death, in cases of starvation.
2. Describe the course of the left subclavian artery, mentioning its most important connections, and the branches which it gives off.
3. Describe briefly the anatomy of the pneumogastric nerve, pointing out the difference of its course on the right and left side. Mention the most important functions of this nerve, and name some of the diseases or morbid conditions of the system in which you suppose it to be involved.
4. Describe the duodenum, the nature of the secretions which are poured into it, and their influence in the process of digestion.
5. Give a sketch of the physical and chemical characters of healthy urine, mentioning the average amount of urea and uric acid excreted in twenty-four hours. What effect is produced on the urine by the administration of benzoic acid?
6. Mention the peculiarities of the fetal circulation. State what you conceive to be the principal office of the placenta. Describe the nature and causes of the sounds which are heard on auscultating the uterine region during pregnancy.

Third Examination.

1. Describe the different modes of death, mentioning their symptoms and some of the diseases in which they respectively occur. What are the most frequent causes of sudden death?
2. Give a description of the paroxysm in hysteria, and contrast it with that in

epilepsy. Mention the diseases most commonly simulated by hysteria, and show how the true nature of these cases may be detected. Give a sketch of the general principles of treatment, both during the hysterical paroxysm and in the interval.

3. Describe the organic causes and the treatment of pulmonary hæmorrhage.

4. Describe the causes, symptoms, and treatment of abscess of the liver; and mention some of the courses which the pus may take.

5. Give a sketch of the causes, symptoms, and treatment of erysipelas of the face and head. Mention the different ways in which it may prove fatal.

6. What are the causes, symptoms, and treatment of stricture of the rectum?

7. Mention briefly the symptoms and treatment of secondary syphilis.

8. Describe the symptoms of abortion, and the treatment of them.

QUEEN'S COLLEGE, BIRMINGHAM.

At the recent annual meeting of the Governors of this College, the following report was read on the medical department:—

"The Council have the satisfaction of witnessing, from year to year, the entrance upon life of the students who have received their education in this department of the College; and they are now able thankfully to point to many in extensive and lucrative practice, and holding public appointments, who were formerly students in this department. During the present year, Mr. P. H. Bird, who quitted the College in the year 1847, has obtained the Jacksonian Prize, awarded by the College of Surgeons, and open to all its members, for his Essay on the Nature and Treatment of Erysipelas. The Council hope that this success augurs for that gentleman a distinguished professional course. The Gold Medals on Dr. Warneford's foundations were adjudged, in October last, to—Stead and—Mills, for their Essays on the Brain. On the same occasion gentlemen received medals and prizes in the classes—*Materia Medica*, *Botany*, *Chemistry*, *Medicine*, *Midwifery*, *Surgery*, *Anatomy*, *Forensic Medicine*, &c. &c.

"The number of resident students in the College in Paradise Street has been greater during the present than any former year. The number of out-students is also satisfactory.

"The professors and tutors have delivered to the Council satisfactory reports of the conduct and progress of the students in their several classes. The Theological Lecture, endowed by Dr. Warneford, has been delivered during the present year by the warden, who reports favourably of the attendance and improvement of the students."

THE CHOLERA ON THE CONTINENT.

The cholera is still prevalent in various parts of Germany, especially in the Austrian empire. At Lubeck, from the 17th of July to the 24th of August, there were 918 cases and 451 deaths. In 1832, the deaths from cholera in this city were 800; and in 1848, from the 18th of September to the 30th of November, there were 575 cases and 301 deaths. At Malmo, in Sweden, the disease is also very prevalent. There were more than 300 cases in less than a month; and about one-fourth proved fatal.

Accounts of the 13th of August from Mexico mention the disappearance of cholera after 100 days' duration, and 18,000 deaths in that city alone.

PROFESSOR WEBSTER.

PROFESSOR WEBSTER was executed at Boston, on the 30th August, for the murder of Dr. Parkman.

DINNER GIVEN IN DUBLIN TO SIR BENJAMIN BRODIE.

THIS eminent surgeon was, on August 29th, entertained at dinner by the President and Council of the Royal College of Surgeons. In proposing the toast of the Royal College of Surgeons of England, coupled with the name of its most distinguished member then present, the President observed that he might upon such an occasion correctly allude to the great names which adorned its lists, and the benefits conferred upon the profession and the public by their exertions. He would not, however, do more than express, through Sir Benjamin Brodie, the feelings of gratitude which were entertained by the Irish College of Surgeons for the never-failing hospitality and kindness shown to its members by their brethren of the College of Surgeons of England, and the great gratification they felt in being able in the present instance to mark that feeling by paying a tribute of respect to the most distinguished living member of that body. It was difficult—very difficult, he said, to express in proper terms sentiments of admiration for a man who was good as well as great, when that man was present; but he felt that he was guilty of no flattery when he stated that he hailed their honoured guest as the first surgeon in the metropolis of the world.

SIR BENJAMIN BRODIE returned thanks for the honour done to himself and the College of Surgeons of England, and in doing so begged to assure those present of the high opinion entertained by English surgeons for the professional exertions and merits of the members of the Irish College, and of their endeavours to elevate their science from the position of a trade into that of a noble profession. He had

for more than forty years been acquainted with many of its members, and he could safely say that no body of men (in his judgment) more truly answered the characters of a scientific and gentlemanly profession than the members of the Irish College. It was not then the time to speak of the many improvements in surgery made by gentlemen of its body; but he could not refrain from referring to one of the latest—viz., the introduction of a bloodless mode of cure for the formidable disease of aneurism. The proofs he had witnessed of its efficacy during his stay in Dublin convinced him that it must soon supersede every other plan of cure. Such triumphs as these had secured the pre-eminence for the school of surgery in Ireland, and he trusted that it might long enjoy the honour.

Sir PHILIP CRAMPTON rose, and observed that, allusion having been made by their eminent guest to the treatment of aneurism by compression, he begged, in the name of the Council, to present to Sir Benjamin Brodie a trifling gift, but one that he might be gratified to receive—viz., a set of the instruments used in the treatment of the disease, fitted with the elastic spring, the valuable improvement of Dr. Carte; and to these he begged to add, from the author, the work of Dr. Bellingham on the subject.

OBITUARY.

DR. BENJAMIN WILLIAMSON, ABERDEEN.

WE have this week to record the death of Dr. Williamson, of Aberdeen. The deceased, who had reached his 62d year, was born on the 25th of January, 1789. In 1804, he became an articled pupil of the late Dr. French, then one of the medical attendants in the Aberdeen Infirmary, and Professor of Chemistry in the University. In the autumn of 1808 he proceeded to London, where he attended the surgical practice at Guy's and St. Thomas's Hospitals. Subsequently to this he became assistant to a surgeon in Cheltenham, returning to London, in November 1811, to enter the different classes then taught by Mr. Cline, Mr. Cooper, afterwards Sir Astley, Sir Benjamin, then Mr. Brodie, and Dr. Haighton. Early in 1812 he presented himself for the usual examination at the Royal College of Surgeons, with the view of entering the naval service as assistant-surgeon. Ultimately, however, he determined on returning to his native place, which he did about the close of 1812, after spending a few months at the Medical School in Edinburgh.

In Aberdeen, Mr. Williamson's first

charge was that of a public dispensary. In 1814 he became one of the medical attendants at the Royal Infirmary, the important duties of which office he continued to discharge, with much credit to himself and benefit to the institution; for the long period of nearly a quarter of a century. During his incumbency the present division of the duties of the medical officers of the Infirmary had not been effected; and, consequently, Mr. Williamson had to act there in the capacities both of surgeon and physician. Rare as such a double qualification is usually found to be, it would be difficult to say in which of these two spheres of action the services of the deceased were the more appreciated by the managers and the public. While he did much to sustain the previously well-earned medical fame of the institution, to him the merit is justly due of having been the first to originate the improved surgical practice which has since placed it on a level in this respect with the most efficient metropolitan establishments. In 1838 he resigned his situation at the Infirmary, and about the same time withdrew altogether from the operative department of surgery, in order to devote his time entirely to his extensive private practice, and to his numerous engagements as a consulting physician. His withdrawal from the Infirmary was deemed a fitting occasion on which to confer on him an honorary degree in medicine, which was done by Marischal College soon after. In general and in consulting practice, Dr. Williamson's career was most active and laborious till about two years ago, when, his health giving way, he almost wholly retired into private life.

We have already alluded to the rare union in Dr. Williamson of the best qualities both of the surgeon and the physician. For the former branch of his profession he was admirably qualified. As an operative surgeon, he had few equals amongst his contemporaries here or elsewhere. As a physician, however, it is that he will, perhaps, be longest remembered. His quiet and gentlemanly manners, his highly prepossessing appearance, and his undeviating cheerfulness, coming strikingly in aid of his ready resources, his never-failing tact and precision, and his long and varied experience, generally sufficed to secure for him the entire confidence of his patients, most of whom became in time his most attached friends. By his medical brethren Dr. Williamson had been placed, for several years before his death, at the head of the profession in that part of Scotland—a position considered by them as due to him, both from his sterling uprightness of conduct and his high medical requirements.

Selections from Journals.

ON THE POST-MORTEM APPEARANCES IN DEATH BY DROWNING. BY DR. RIEDEL.

The following are the results of a series of experiments by Dr. Riedell:—

1. The cadaveric rigidity does not occur more slowly than in other modes of death. This phenomenon is accelerated by high temperature of the water.

2. The accumulation of blood within the cranium was inconsiderable, and effusion of blood had not occurred in any case.

3. The epiglottis was constantly found erect.

4. The diaphragm was always found relaxed, its convexity being towards the thorax. The intestines were not pressed against the abdominal walls. The lungs were usually found in the condition of an imperfect expiration,—owing, doubtless, to the distension of these organs by fluid, the presence of which presented an obstacle to the collapse of the thoracic parietes.

5. In half the number of instances the two sides of the heart contained equal quantities of blood; in the other half the right side contained the larger proportion. In one case only the emptiness of the left side contrasted strongly with the fulness of the right. The quantity of blood in the liver varied greatly.

6. The bladder was sometimes empty, and sometimes full. It was almost always distended with fluid in animals drowned in a state of stupor.

7. The blood in the heart and large vessels contained coagula, in autopsies made from two hours to five days after death. Coagula were found three-quarters of an hour after death, in the heart of a cat drowned in boiling water (!), while in animals drowned in ice-cold water the blood was still fluid and cadaveric rigidity was absent.

8. In the majority of cases the fluid had passed into the stomach. In animals previously killed, and placed for twenty-four hours in water with the mouth wide open, no fluid penetrated to the stomach.

9. In every case the trachea, bronchi, and lungs contained a frothy fluid. This was a constant sign of death by submersion. After death, this frothy fluid gradually disappeared from the air-tubes by exosmosis, but not from the lungs. The fluidity of this froth is diagnostic of death by drowning, and is not met with in any other mode of death. Another equally important character was observed in the constantly flabby condition of the lungs, which are dilated and preserve the impression of the

finger on their surfaces. The lungs are also three or four times as heavy as natural. This diagnosis, however, is open to the objection that the same phenomena may be produced by the forcible injection of fluid into the lungs.—*L'Union Médicale*.

X

THE SIGNS OF DEATH.—DR. DOWLER'S THERMOMETRICAL TEST.

DR. DOWLER, of New Orleans, proposes the thermometer as a means of testing death, possessing as it does superior certainty over the stethoscope. The latter method takes for granted that in apparent death the heart's action still continues,—that it cannot be for a time suspended, and that it can always be heard! The very analogies of apparent or temporary death seem to oppose or contradict these assumptions. The analogies and the positive facts known of animal temperature teach that during life the body is not heated and cooled like inert matter. Place two or three thermometers in the arm-pits,—in the bend of the arm (the forearm being flexed),—in the mouth and within the sphincters, to ascertain the heat of the surface and of the centres (the rectum is the best and most accessible centre). The application of the thermometer requires no skill, and is open to the inspection of all; and is a test for all the warm-blooded animals,—at least for man. While the auscultatory test takes for granted that there can be no temporary inaction of the heart, and that all its motions can be heard, the thermometrical test takes nothing for granted without the most indubitable proof. Its great axiom is, that man in his living state maintains an uniform temperature independent of the surrounding media; while a dead man, like other inert matter, has no independence of this kind, but steadily responds to, and is governed by, caloric conditions altogether physical—heating and being heated, receiving and radiating caloric. This is not the result of speculation, but of prolonged and varied experimental research.

The refrigeration of the body before death in cholera, congestive, and the like, is not physical refrigeration responding to the caloric condition of the surrounding media; it is a morbid or physiological calorificity, which for a time augments or continues stationary after death, until it shall be replaced by physical refrigeration, as its phenomenal history clearly shows.

"The facts," observes Dr. Dowler, "which I have published concerning post-mortem calorificity do not invalidate this thermometrical test; for soon or late the physical refrigeration must take place. I may here add that the speculative opinion which pre-

vails among those who do not take the trouble to make experiments,—namely, that these calorific movements are the effects of putrefaction, is wholly unfounded (so far as it regards the human subject), how much soever it may be countenanced by certain analogies derived from other inert matter. The calorific and putrefactive periods, so far from coinciding, antagonize each other, so long as the heat is not in accordance with the ordinary physical laws of calorific. The point of coincidence and equilibrium is really the point of putrefaction, unless the circumstances be of an extraordinary character, such as involve the freezing point, or that of torrefaction. But the predominance of the invariable law of refrigeration is a criterion always attainable, and may be proved as to its times, distances, and velocities, by arithmetical calculation:—ascertain the temperatures of the media and of the heated body, the velocity of the refrigeration will be proportioned to the times and distances, and will proceed from the surface to the centre until the equilibrium be attained. The only objection that lies against this rule relates to the calorific conditions where the differences between the heated body and the media are very slight; but this is of no importance in practice, because there is always a marked difference between the average temperature of the air in the shade and that of a living or recently dead person.”—*British American Journal*, May 1850.

REPORT OF MEDICAL CASES OCCURRING IN THE NEW YORK HOSPITAL. BY V. P. COLTON, M.D., RESIDENT PHYSICIAN.

Case of Lead Paralysis.

MARCH 12th, 1850.—Henry Dorigan, *et. 28*, Ireland, clothier: is of sanguine temperament; has red hair, fair skin, and blue eyes. About six months ago he was seized with colicky pains in the epigastrium, which were quite severe, and which continued more or less acute for six days. His bowels at the time were quite costive: he took medicine, which produced free emesis and catharsis. In about half an hour from the accession of pain he began to feel a strange weakness in both hands and arms. Since that time he has had no return of pain; his bowels have been quite regular; appetite and general feelings have been natural; the weakness in hands and arms alone remains. At the present time the paralysis chiefly, if not entirely, affects the extensor muscles of hands and fingers, the flexors being about as strong as usual. A bluish tinge is observed along the edge of the gums. The only cause which a rigid examination can find to account for the symptoms is, the habit of drinking freely of

water, conveyed from a fountain half a mile distant through a lead pipe.

April 28th.—Has used internally, since his admission, Acid. Sulph.; Quin. Sulph.; Potass. Iod. Arms and hands have been freely rubbed with stimulating liniments, and fingers have been kept extended for a part of the time on the hand and finger splint; all without much benefit. Can use right hand rather more freely than when admitted. Discharged relieved.

Observation.—There would seem to be a peculiar tendency in many of the mineral poisons, in their remote effects, to produce cramps and paralysis of the second and third divisions of extremities—the face, arm, and hand, the leg and foot: and in several cases these affections have commenced in the fingers and toes, and travelled up the limbs. Thus, in a case of arsenical paralysis now in the Hospital, the pain, cramps, and paralysis commenced in the fingers of right hand; then in those of left; then in the toes, first of one foot, and, shortly after, in those of the other; the whole gradually extending up the limbs.

Another case of paralysis and cramps, from the chronic effect of arsenic, with similar phenomena, occurred here in 1847.

In lead paralysis, and in that produced by copper, the same order is sometimes observed. Also in a case of tremor mercurialis which lately fell under my notice: the weakness and peculiar trembling commenced in left fore-arm; then attacked left leg; then the right leg; and, lastly, the right fore-arm. The reason why the second and third divisions of extremities should be primarily affected, does not appear. It seems as if all cramps and paralysis proceeding from gastro-intestinal irritation showed the same preference. Cholera cramps certainly do; and the pain in the calves of the legs, in dysenteric patients, is a matter of daily observation.—*New York Journal of Medicine*, July 1850.

ON THE EMPLOYMENT OF NUX VOMICA IN IMPOTENCY AND SPERMATORRHEA.

DR. DUCLOS states that he has found the exhibition of doses of one-eighth to one-sixth of a grain of the alcoholic extract of nux vomica attended with beneficial results in cases of impotency resulting from long continence, and in those dependent on nervous excitability. In spermatorrhoea attended with general debility the nux vomica has produced an improvement of the constitutional condition, with cure of the spermatorrhoea. The dose above named is given three or four times a day, being gradually increased, even to the extent of two or three grains, without injury.—*L'Union Médicale*.

BOOKS & PERIODICALS RECEIVED

FOR REVIEW

DURING THE LAST FIVE WEEKS.

Memoirs on the Nervous System. Parts 1 and 2. By Marshall Hall, M.D.

Synopsis of the Diastaltic Nervous System. By Marshall Hall, M.D.

The Cyclopædia of Anatomy and Physiology. By R. B. Todd, M.D. F.R.S. Part 40.

A Practical Treatise on the Therapeutic Uses of Terebinthine Medicines. By Thomas Smith, M.D.

Corpulence; or, Excess of Fat in the Human Body: its Relations to Chemistry and Physiology. By T. K. Chambers, D.M. &c.

Lectures on Inflammation, delivered at the Royal College of Surgeons. By James Paget, Professor of Anatomy and Surgery to the College.

Practical Suggestions on the Prevention of Consumption. By G. Calvert Holland, M.D.

A Practical Synopsis of Diseases of the Chest and Air-passages, &c. By James Bright, M.D.

Results of the Treatment of Fever in the Glasgow Hospitals and Out-door Practice contrasted. By Jas. Adams, M.D.

Manual of Materia Medica. By Edward Chapman.

Oratio Harveiana habita die Junii XIX. MDCCCL. A Jacobo Arturo Wilson, M.D.

Court Patronage and Professional Jealousy in England.

On the Varieties of Man. By R. G. Latham, M.D.

Twentieth Annual Report of the Belfast District Lunatic Asylum. 1850.

The Metropolitan Interments Act. By G. H. H. Oliphant.

Report of the Medical Benevolent Fund for the year 1849-50.

The Quarterly Journal of the Chemical Society. Vol. 3, No. 3.

London Journal of Medicine. September.

The Pharmaceutical Journal. Sept. 1850.

Edinburgh Monthly Journal of Medical Science. September 1850.

American Quarterly Journal and Library of Dental Science. January and April.

Eleventh Annual Announcement of the Baltimore College of Dental Surgery.

New York Medical Gazette. Nos. 1 & 2.

The Philadelphia Medical Examiner. Nos. 3, 4, 5, 6, 7, and 8, March to August.

The Boston Medical and Surgical Journal. August 1850.

Recherches sur l'État de la Contractilité, etc. Par le Docteur Duchenne.

Comptes Rendus. Nos. 5, 6, 7, 8, 9, 29th July to 26th August.

Journal de Chimie Médicale. Septembre.

Das Kramphafte Asthma der Erwachsenen. Von Dr. F. Bergson Nordhausen. 1850. Casper's Wochenschrift für die gesammte Heilkunde. Nos. 27 to 31, 6th July to 3d August.

Every-day Wonders; or, Facts in Physiology which all should know.

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer 30.19
 " " " Thermometer 56.5
 Self-registering do. Max. 75° Min. 24°
 " From 12 observations daily. " Sun.

RAIN, in inches, .08.—Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was about 1° below the mean of the month of September.

BIRTHS & DEATHS IN THE METROPOLIS

During the Week ending Saturday, Sept. 7.

BIRTHS.		DEATHS.	
Males.....	734	Males....	466
Females..	750	Females..	433
	1474		899

CAUSES OF DEATH.

ALL CAUSES	899
SPECIFIED CAUSES	899
1. Zymotic (or Epidemic, Endemic, Contagious) Diseases....	251
<i>Sporadic Diseases, viz.—</i>	
1. Dropsy, Cancer, &c.	46
2. Brain, Spinal Marrow, Nerves, and Senses	94
4. Heart and Bloodvessels	26
5. Lungs and organs of Respiration	80
6. Stomach, Liver, &c.	60
7. Diseases of the Kidneys, &c.	12
8. Childbirth, Diseases of Uterus, &c.	6
9. Rheumatism, Diseases of Bones, Joints, &c.	8
10. Skin.....	1
11. Premature Birth	24
12. Old Age	20
13. Sudden Deaths	9
14. Violence, Privation, Cold, &c.	24

The following is a selection of the numbers of Deaths from the most important special causes :

Small-pox.....	8	Convulsions.....	30
Measles.....	13	Bronchitis.....	33
Scarlatina.....	35	Pneumonia.....	31
Whooping-cough.....	25	Phthisis.....	119
Diarrhoea.....	75	Lungs.....	2
Cholera.....	8	Teething.....	10
Typhus.....	48	Stomach.....	4
Dropsy.....	16	Liver.....	9
Hydrocephalus.....	12	Childbirth.....	4
Apoplexy.....	20	Uterus.....	1
Paralysis.....	24		

REMARKS.—The total number of deaths was 239 below the average mortality of the 36th week of ten previous years.

NOTICES TO CORRESPONDENTS.

Communications have been received from Dr. W. Hamilton, Dr. Addison, Mr. Lonsdale, Dr. Bucknill, and Mr. Bate (of Swansea). These will have early insertion.

We beg to assure Mr. W. J. Orr that his Essay, with six other works on Cholera, is now in the hands of a gentleman for review.

Lectures.

CLINICAL LECTURE
ON
CARDIAC DROPSY,
(Delivered at King's College Hospital.)

By R. B. TODD, M.D. F.R.S.
Physician to the Hospital.

[Reported by H. H. SALTER, B.A., Med. Schol.
K.C.L.]

LECTURE X.

GENTLEMEN,—In accordance with my two-fold plan of selecting for the subject of my remarks cases under your immediate observation, and at the same time giving you a consecutive account of the varied, allied, and contrasted forms of some particular disease (which one, as you know, at present is dropsy), I shall offer you to-day some observations on a case of *cardiac dropsy* now in the Fisk ward. The dropsy in this case is mainly dependent on heart disease; but there are present various other phenomena, which partly tend to complicate, and partly to elucidate the diagnosis, but which are all interesting and instructive in a high degree. These are pericarditis, renal disease, a congested state of the liver, dependent, no doubt, on the disease of the heart, and other slighter pathological conditions, which I shall advert to in the history of the case.

The patient, Alfred Baylis, is a young man, *æt.* 30. His occupation has been that of a housepainter, a most unhealthy one, and he bears, in the blue line round his gums, the badge of his trade. Like many of his occupation, he has been intemperate in his habits; and to this, most probably, his present condition is mainly referable.

He was admitted on the 11th of November, suffering from palpitation, shortness of breath, and emaciation, which quickly supervened upon an attack of hæmoptysis to a considerable extent, that occurred two months previously. He had been an out-patient of one of the hospitals in town; and five weeks before his admission here dropsy of the lower extremities came on, and has been steadily increasing ever since. Under these circumstances he applied for admission. Shortly afterwards he suffered another and very violent access of hæmoptysis. These two attacks of hæmoptysis have produced great pallor and anæmia; and at the same time the patient

is suffering from distressing dyspnoea, amounting to *orthopnoea*. Upon any attempt to assume the recumbent posture he is at once compelled, by the increased difficulty of respiration, to sit up, and in this posture, as you may have remarked, he remains day and night. This orthopnoea has been from the commencement a prominent symptom.

The great loss of blood which he has suffered has given rise to another symptom, —amaurosis: this seems to vary with the condition of the heart. When he suffers much from palpitation his sight is worse; when the palpitation is less it is better; showing the dependence of the symptom on the state of the circulation. The fact is, the blood has become poor, and this impairment of quality causes any slight deficiency in the quantity sent to an organ, from inefficient action of the heart, to be felt by it more readily than if the blood were in its normal state. Associated with this symptom is another curious one; namely, a remarkable prominence of the eye-balls. I mention it because it seems to have some connection with cardiac disease. I have seen it particularly in women. I have also met with it in connection with enlarged thyroid gland. The cause of these associations I do not attempt to explain.

In looking into the previous history of our patient, to ascertain if there be anything to explain the existence of cardiac disease at so early an age, we can only find that at the age of 19 he suffered a slight attack of rheumatism. We can get no distinct evidence that the heart was at that time affected: but it is highly probable that, at the same time with the rheumatic attack, there took place some slight organic affection of the heart, either pericardial or endocardial, which his subsequent unhealthy occupation and intemperance have tended to increase.

His state on admission into the hospital was as follows:—Great exsanguineousness; breathlessness, particularly orthopnoea. Heart's action natural and regular, with no preternatural impulse; pulse 84, small, regular. There was an unnatural extent of dullness over the region of the heart, and moreover the pulsations of the heart were visible and sensible in the region of the scrobiculus cordis: they were felt also in the cardiac region. In some cases the heart's action is felt only in the scrobiculus cordis, and beneath the left mamma; and in such there is no evidence of enlargement; for there may be, and probably is, some cause pressing the heart to the right side, so that the altered seat of the pulsations is due to change in the position of the entire organ. But when the pulsation of the heart is felt in both regions we have evidence of

enlargement, and of its being, at least, the right ventricle that is enlarged; for while the left remains stationary in its normal situation, the right has passed over still further to the right side. Those of you who accompany me regularly through the wards of the hospital may notice, that in all cases of dyspnoea I make it a rule to place my hand over the scrobiculus cordis to feel if I can detect any cardiac pulsations there. My attention is particularly directed to this point in cases of long-standing asthma, and rarely in these cases do I fail to find it; never, indeed, if the asthma have produced emphysema, where there is not only displacement of the right ventricle by hypertrophy, but a pushing of the whole heart to the right side from the enlargement of the lungs.

We noticed also in this case a friction sound accompanying the systole and diastole, heard below and to the left of the nipple, and very circumscribed in extent. There was no bellows murmur either at base or apex; but we observed a phenomenon, connected in its indications with the beating of the heart, in the scrobiculus cordis,—viz., venous pulsation in the neck. This man has very large jugulars, and you may see them pulsating distinctly even at some distance from the bed. This phenomenon, as you know, is due to a reflux of blood at the systole from the ventricle through the auricle into the venous system. In consequence of the obstruction in the lungs the ventricle cannot completely empty itself, and so becoming distended the tricuspid valve allows some of the blood to regurgitate, and thus to escape through the auricle into the veins. The proper source of supply to these veins is from their distal extremity. If then you put your finger in the course of that supply,—that is, to the distal point where the pulsation is observed,—the vein would, if healthy, become empty; but it does not; it continues full, and its pulsations persist: hence the waves of blood which produce the phenomenon of pulsation must be propagated from the cardiac extremity of the vein. There are many cases of heart affection where there are no pulsations, but where the vein does not empty itself on the cardiac side of the point pressed. Now in such cases there is regurgitation, but not to a sufficient degree to cause the pulsation. In perfect health the vein will be quite empty below the point pressed: but it is possible that a slight enlargement may suffice to produce the opposite state; and it is very possible that, even in health, it may exist to a slight extent under the prolonged though temporary occurrence of some disturbing circumstances, such as great efforts, violent actions, &c. When the regurgitation

exists in a great degree, you may see all the superficial veins of the neck and chest pulsating.

We found also in this case evidence of great pulmonary congestion, crepitus and rhonchus being heard over various parts of the chest; and there was abdominal dropsy, caused by obstructed circulation through the liver. On examining the urine we found it acid, of specific gravity 1015, and heat and nitric acid gave evidence of the existence of albumen. When the fresh urine was allowed to stand, a scanty and flocculent deposit was formed, which, under the microscope, was found to consist of blood-corpuscles, epithelium (chiefly renal), a few pus globules, and a great many casts, for the most part granular and transparent, some containing cells loaded with oil globules; but these were not very abundant. Now this is a state of things which we should fancy our patient would not be likely long to survive; yet here we see him remaining very much in *status quo*, if any thing a little improved, until the present time, Jan. 1863, a space of two months.

Let us now analyse these symptoms more minutely, and see their nature and indications, and what conclusions we can deduce from them.

The *palpitation* was no doubt an irregular rate of action. When the heart beat quickly he had what he called palpitation; when more slowly, the palpitation ceased. When patients speak of palpitation they often mean different things; sometimes they signify by it deranged rhythm, either complete intermission, or irregularity of action; and sometimes, as in the present case, an increased rate of action without any derangement of rhythm. Our patient has not suffered from any remarkable rapidity of the heart's action, except in the above temporary way.

Orthopnoea.—This is almost always connected with imperfect circulation through the lungs: but this imperfection of the pulmonary circulation may either be primary, as in asthma, or secondary, from imperfect action of the heart. In asthma no doubt there is, frequently, an anatomical change in the lungs; but we must guard against the idea that this is essentially the disease: the primary affection is in the nervous system, or in the digestive organs, or both, and any irritation there will produce the asthmatic spasm. But this state of spasm may give rise to anatomical change of the lungs (emphysema); and its frequent recurrence is sufficient to render such a state to a certain degree permanent. When the heart is primarily affected, the embarrassment of the respiration, which is secondary, has received the name of cardiac asthma.

In connection with this we found a *state*

of congestion of the lungs. Now this no doubt was dependent on the heart, through the imperfect action of which the blood was thrown back on the lungs. To this the hæmoptysis is to be attributed; and no doubt, if we could examine the lungs, we should find the remains of pulmonary apoplexy, and of rupture of pulmonary vessels, in the characteristic currant jelly-like spots.

The beating of the heart in the scrobiculus cordis, and its interpretation, I have already referred to.

The *dropsy* is a result of which I shall say more by and by.

Diagnosis.—Now can we, from these general symptoms, make a diagnosis without reference to the physical signs? I think we can form a diagnosis which shall be sufficient for all practical purposes; and I recommend you, as an important exercise, to endeavour to diagnose from symptoms before you have recourse to auscultation; because by so doing your attention will be directed to the disturbed *functions*, and you will thus receive important suggestions for treatment. Those who direct their attention exclusively to physical signs are apt to overlook these derangements of functions, and therefore miss some valuable indications of treatment corrective of them.

Now if I were to proceed on the data afforded by the *general* symptoms alone in this case, I should diagnose first, *dilatation of the right side of the heart*: the pulsations of the jugulars, and the beating of the heart in the scrobiculus cordis, both indicate this. The pulsation of the jugulars, if proved to be regurgitant, is conclusive evidence of dilatation of the right ventricle: no other condition of heart is capable of producing this phenomenon. But we must be careful to prove that the venous pulsation is regurgitant, as it may be due to other causes: it may depend on the impulse of the heart being transmitted through the capillaries; or it may depend on the contiguity of a large artery which communicates its pulsations to the adjacent vein; therefore we must be careful in proving its nature before we attach importance to its indications. Now, by making pressure on the vein, we obtain sufficiently conclusive evidence as to the nature of the pulsation. If the vein empty itself on the cardiac side of the point pressed, and yet the pulsation continues in the empty portion of it, we know it arises, with the highest probability, from some contiguous artery; and this conclusion is rendered certain, if the pulsation ceases on stopping the circulation in the artery. If, on making pressure, the pulsation continues only on the distal side of the point pressed, we may infer that the pulsa-

tion is due to the transmission of the pulse from the heart throughout the circulation, and especially if it be perfectly synchronous with the heart. But if the vein continues full between the point pressed and the heart, then we know, whether there be pulsation or not, that that vein must receive blood from its cardiac side, and this it can only do by regurgitation from the right side of the heart.

You are doubtless aware that a slight regurgitation always takes place from the auricles into the veins in the normal state, and that that regurgitation is synchronous with the auricular systole. But when the ventricle and auricle are dilated, the amount of regurgitation is increased in proportion to the degree of dilatation of the cavities: blood regurgitates from the ventricle into the auricle, and from the auricle into the veins.

The beating of the heart in the scrobiculus cordis is also indicative of dilatation of the right ventricle, because by its dilatation that cavity extends to the apex, and the apex thus enlarged extends to the scrobiculus. But, before we can affirm that the beating of the heart in the scrobiculus is due to the dilatation of the ventricle, we must be careful to ascertain that there is no displacement of the heart, which may be easily determined by seeing that its impulse is felt in the normal, as well as in the abnormal, region.

So far, then, we may affirm, that there is dilatation of the right side of the heart. But such a lesion as dilatation does not arise spontaneously. There must be some dilating force, and that force is the backward pressure of the blood. When the muscular tissue of the heart is in a weak state, we can conceive that the walls of the ventricle may yield readily, without supposing any great increase in the ordinary pressure of the blood. But, in general, dilatation arises from some obstacle to the flow of blood from the ventricle; and in proportion to the resistance which that obstacle affords will be the backward pressure of the blood on the surface of the ventricle,—in other words, the amount of obstacle will give the amount of dilating force. In every case of dilatation of one of the heart's cavities, then, you must look for this dilating force,—this obstacle; and you will find it either at the orifice through which the cavity expels its blood, or in the arteries, or in the capillaries of some important organ, or in some defect in the valves or the orifices of the heart. Dilatation of the right cavities of the heart rarely arises from diseases of their own orifices; for obstructive disease of these orifices is amongst the rarest of cardiac lesions. Experience tells us that the obstructions which the right ventricle

has to encounter are either from the circulation through the lungs, or from some derangement of the circulation through the left side of the heart.

And there is another cause which may produce dilatation as well as hypertrophy of the right ventricle, because it may produce the same change in all the cavities of the heart; that is, extensive or universal adhesion of the opposed surfaces of the pericardium. How it happens that obliteration of the pericardial sac can produce these changes, I do not attempt to explain; but that it does so, there can be no doubt.

Let us now inquire what may cause a dilated right ventricle in this case. I have said that the most common causes of dilatation of the right ventricle are obstructed pulmonary circulation, or obstructed circulation through the left side of the heart. The most common causes of obstructed pulmonary circulation are the asthmatic state, or that which is a frequent consequence of the asthmatic state—emphysema of the lungs. We have no indications in this case of either of these conditions. The history of the patient shows the absence of the one, and the physical signs demonstrate the absence of the other. Is there, then, any disease of the left side of the heart sufficient to create this dilatation?

When I saw the patient first I hazarded a guess, from the character of the pulse, that there was *mitral* disease at the foundation of his malady. The peculiarity of pulse which suggested this notion was, that it was small, produced by a small stream, apparently inadequate to the size of the artery, and not proportioned to the action of the heart. Such a state of pulse does frequently co-exist with mitral disease; but so various are the conditions capable of producing a small feeble pulse, that a diagnosis based upon that sign only must necessarily be only a guess. Now such mitral disease might be either *regurgitant* or *obstructive*,—either of the two would affect the right side of the heart. And how?—By affording an obstacle to the return of the blood from the lungs. Whether the blood is unable to advance in consequence of a *contracted* mitral orifice, or whether it flows back in large quantity into the auricle, it is evident that an impediment is offered to its passage from auricle to ventricle, and, therefore, to its return from the lungs to the auricle; and when this has continued for some time, the right ventricle being exposed to a degree of pressure greater than customary, yields, and gradually expands, till at length it assumes a permanently dilated state.

Now, is the disease here *regurgitant* or *obstructive*? The dyspnoea favours the idea that it is *obstructive*, because there is

no form of heart disease in which the dyspnoea is so great as in obstruction at the mitral valve. Very often, when the disease is of this nature, sudden and terrible dyspnoea is the first evidence of its existence: the patient is suddenly awoken out of sleep by a breathlessness that seems as if it would kill him: he starts up in bed and gasps for breath till the paroxysm gradually subsides. Such a first attack is generally the precursor of similar ones, increasing in severity and frequency, only kept off by constant watching and the upright position, till at length the dyspnoea becomes constant, and the case terminates with pulmonary apoplexy.

But in the absence of this disease of the mitral valves, is there any other way of explaining the occurrence of the dilatation of the right cavities of the heart? The only explanation I can offer is that there is hypertrophy and dilatation of the left cavities connected with obstruction in some part of the systemic circulation, in the aorta or in the renal circulation, or dependent on adherent pericardium, and that this state of the left cavities obstructs the circulation through the right cavities, and causes dilatation and hypertrophy of them.

So far, then, for the indications afforded by the general symptoms: let us now turn to—

The physical signs.—A careful observation of the physical signs will serve to confirm or correct our inferences from the symptoms, and help us to adopt a more decided diagnosis. The physical signs themselves were these: extensive dullness over the præcordial region, indicating enlargement of the heart; the first and second sounds were found to be essentially healthy, and unaccompanied with bellows murmur, as well at the base as at the apex. The absence of bruit at the base shows that there is no aortic disease; also its non-existence at the apex shows that there is no *regurgitant* mitral disease; for if there were, we should inevitably hear a systolic bellows murmur in this situation—*i. e.*, unless the heart's action were very weak: it is only in a state of great feebleness of the ventricular contraction that there would be no bellows sound where there is mitral regurgitation; but with our patient the heart's action is not at all feeble. Now, is there any diastolic bellows sound to indicate obstruction at the mitral orifice? We find none. Are we, then, justified in denying the disease of the mitral valve? Certainly we are not, for those cases of unquestionable obstructive mitral disease in which a diastolic murmur is heard are comparatively few: there must be a certain amount of force in the auricle for its production, and unless this exists the sound will not be heard.

The value of this evidence, therefore, is merely negative: it does not prove that there is no obstruction at the mitral orifice, but only that the impulse given to the blood by the auricle is not sufficient to elicit a sound from the existing obstruction. So far, then, we receive really no aid as to determining the question of the existence of obstructive mitral disease from examining the sounds of the heart. We know, however, that there is no mitral regurgitation; and, as to the question of mitral obstruction, we have no further evidence than what the symptoms give us.

This evidence is undoubted as to the existence of obstruction; but it is not sufficient to determine the exact seat or nature of the obstruction. By the aid of auscultation we can determine that it is not mitral regurgitation: it may, however, arise from contraction of the mitral orifice, or from that general obstruction which the left heart has to encounter in the distal part of the systemic circulation.

Taking it for granted, then, that the circulation through the left side of the heart is obstructed, it is easy to explain all the other symptoms. The obstruction through the left side of the heart throws the blood back on the left auricle, and thence on the lungs, giving rise to the state of extreme pulmonary congestion in which we find our patient, and the consequent hæmoptysis. The obstruction at the lungs is felt by the right ventricle, which, in its efforts to overcome the opposing obstacle, becomes dilated and hypertrophied: the right auricle encounters corresponding difficulties to those which oppose the action of the ventricle, and it becomes similarly dilated and hypertrophied; and, in consequence of the impediment which it has to encounter, the blood is thrown back upon and delayed in the venous system, and regurgitant venous pulsation and dropsy are the result. Thus we see all the symptoms fall in a retrograde succession, as the consequences of the obstructive force beginning at the left side of the heart, or in some part of the systemic circulation.

A feature of this case which practically is of most interest to us is the *dropsy*, because this is the symptom which generally and justly excites the most alarm in the minds of patients and their friends. I have already mentioned that the characteristic of cardiac dropsy is this, that it always takes place in the most dependent parts: it does not begin, as we have seen other forms of dropsy, in the face and upper extremities. An aneurism may cause dropsy of the upper parts of the body from pressure on the descending cava or some of its large tributary veins, but pure cardiac dropsy never comes on thus: the

very fact of its not existing in the upper parts of the body indicates its cardiac nature. We find it apt to vary in amount according to position; the erect or sitting posture favours its accumulation, and hence we find it in increased quantity towards the end of the day, and disappearing or diminishing in the morning from the recumbent position during the night. Still, it *will* appear in the upper extremities, but always late, and often more on one side than another, being most abundant on that side which is the most dependent. This we have seen in our patient: his left hand was the first affected; but lately we find he has been lying very much on his right side, and the consequence is that the dropsy has left his left hand, and accumulated in his right. We could not have a better proof than this of the purely mechanical character of the effusion.

Now what is the immediate cause of this dropsy? We have seen that the obstructing force extends from the left to the right side of the heart, and by its influence in the latter situation a considerable portion of the blood returning from the general circulation is pumped back into the veins at every systole: this obstructs the flow of venous blood throughout the body: the circulation is consequently impeded in the capillaries: these vessels become loaded, and the only relief they can obtain is by the filtration of the serum of the blood, or the liquor sanguinis, into the areolar tissue. If any part is dependent, a greater force is needed to send the blood through its capillaries, because the venous blood has to ascend against gravity: a greater accumulation is apt to take place both in the capillaries and veins; and, therefore, such part becomes affected with a greater amount of dropsy.

The access of dropsy is always a serious symptom, but not necessarily a fatal one. It may come on early or late. In some cases of purely cardiac disease in young persons, not complicated with either kidney or liver disease, dropsy may come on early, from which the patient may recover completely, and life may be prolonged for some time even although the cardiac disease continue. Suppose a patient has had rheumatic endocarditis, and that the impairment of the mitral valve gives rise to regurgitation, the effect would be to obstruct the circulation first at the left, and then at the right side of the heart. Now the right cavities feel the obstruction at first much more than they would do afterwards: the left auricle, being slow to dilate, has not yet adapted itself to the deranged condition, and the whole force of the obstruction is thrown back on the lungs and on the right side, the cavities of which dilate

easily, and you get rapid dropsy. In the meantime the left auricle dilates, the circulation through the lungs becomes less obstructed: there is, therefore, less obstruction in the right side of the heart: it gradually recovers itself, and the dropsy diminishes.

In all cases what seems to be necessary to the production of the dropsy is the *dilatation of the right side of the heart*. I think my friend Dr. Blakiston, of St. Leonard's, was the first to point out clearly the importance of this condition. Dr. Blakiston seems to lay it down as a rule that venous regurgitation always precedes the dropsy. But dilatation may exist without regurgitation being manifest in the external veins: at least, I think that the regurgitation need not exist to such a degree as to produce pulsation. I am now attending a case in private practice in which there is dropsy and undoubted dilatation of the right side of the heart, but no venous pulsation.

But there are certain other circumstances in this case, in addition to the condition of the heart, that predispose to dropsy: these are, a state of the blood in which the water is in large proportion, and its solid ingredients defective, and *renal disease*. When the blood is in such a condition as to favour dropsy, the other causes will act much more speedily than when such is not the case; and, no doubt, in the present instance, the dropsy came on much earlier in consequence of the impoverished state of the blood occasioned by the repeated attacks of hæmoptysis which our patient had suffered.

Reviewing, then, the whole case, we may sum up thus: that the primary cardiac affection consisted in a rheumatic endocarditis, which narrowed the aperture of communication between the auricle and ventricle, and thus obstructed the passage of the blood into the ventricle, throwing it back upon the lungs, causing pulmonary hæmorrhage, and dilatation and hypertrophy of the right cavities; or, in a rheumatic pericarditis, causing an extensive adhesion between the heart and the pericardium, and so far impeding the heart's play as to give rise to hypertrophy with dilatation of its cavities. Or that, from some obstruction to the circulation, either in the large arteries, or in some more distant part of the circulation (in the kidney, for instance, from the chronic disease in that organ), the left heart has suffered dilatation and some hypertrophy; and that, in course of time, the right heart has, in consequence of the obstructed circulation in the left side, become hypertrophied and dilated likewise. On the former supposition, the primary disease would be in the heart itself;

on the latter, it would be in the kidneys, or some other part of the systemic circulation.

I have already alluded to the amaurosis with which our patient is affected. This affection most probably results from an anæmic state of the retina. Last summer we had in the hospital a remarkable case, which illustrated how loss of blood produced amaurosis. The man had suffered from violent hæmatemesis: after the last attack he completely lost his sight, and he remained quite blind afterwards, notwithstanding a variety of treatment to which he was subjected. In a case of this kind the nutrition of the retina is permanently damaged by the altered quality or quantity of the blood after such severe hæmorrhage, and it bears an analogy to those cases in which delirium or epilepsy follow excessive losses of blood, or extreme states of anæmia.

One thing I have omitted to mention, and that is the pericarditis. At the time of our patient's admission there was a very distinct rubbing sound over the region of the heart, which the next day had disappeared, and with its disappearance aggravated dyspnoea, an undulating movement in the region of the scrobiculus cordis synchronous with the action of the heart, increased extent of præcordial dulness, and feebleness of the cardiac sounds, supervened. We set down all these signs to the development of a large quantity of water in the pericardium; and, with the idea of getting rid of it, I applied a blister over the region of the heart, preceding it by a mustard plaster, and purged him well, supporting him at the same time with chloric ether. The good effect of this treatment was manifested by the disappearance of the symptoms just named, and the return of the rubbing sound. Now what was the cause of the pericarditis? This is very doubtful; but this we know, that renal diseases are very apt, by the contamination of the blood by the uneliminated urinary principles, to induce serous inflammations, and that this man was, at the time of the attack of the pericarditis, labouring under such renal disease.

Treatment.—In the treatment of cases of this description your attention should be principally directed to the relief of the urgent symptoms, and to strengthen the power of the heart. For the relief of the dropsy a great deal may be done by attention to the position of the patient, and by the administration of diuretics. With the view of supporting his power we gave our patient Baylis tonics and iron; and, finding the kidneys acting imperfectly, we gave him digitalis. But in such cases it is desirable to be careful in administering this medicine, and it is a good plan to combine it with some tonic. I frequently combine it with

anæmia, or give the tincture of digitalis with the tincture of muriate of iron. However, I cannot say that any great benefit resulted in the present case, either from the digitalis or the iron: that treatment was cut short by the supervention of the pericarditis. This, however, was subdued by very simple means—counter-irritation and copious discharge by the blister.

There was much difficulty in getting the kidneys to act, and the greatest benefit was derived from the bitartrate of potash, either alone or in combination with the pulvis jalapæ, or the compound powder of jalap. We also gave him him lemon-juice, and found it acted freely on the kidneys for some time. The dose of lemon-juice was from half an ounce several times a day.

Elastrum was also given, but without any decided benefit. What, however, has served best to keep down the dropsey of the legs is the acupuncture: the quantity of water that has run away from him, and the relief that he has obtained from it, is surprising. In practising acupuncture in dropsical cases it is important that you should make the punctures at a considerable distance from each other, for each puncture is apt to become the centre of an erysipelatos inflammation, which spreads the more readily when the punctures are too near each other. Indeed, however you make the punctures, the difficulty which you have to encounter arises from this erysipelatos inflammation, which almost invariably follows sooner or later. There is another method of relieving anasarcos legs, lately proposed by Lombard, of Geneva. Instead of pricking the legs at various points with the needle, he advises to make a single incision above the inner malleolus, or in some other convenient position (not too near a vein or artery), cutting through the skin and subcutaneous tissue down to the fascia. The length of the incision may be from half an inch to an inch, and the rapid flow of water that these incisions admit of is further encouraged by sitting the patient on the side of the bed for a certain time during the day with his feet placed in a tub. The patient has by this means the treble advantage of increased rapidity of the flux of the fluid, the prevention of that sloppy and soaking condition which inevitably ensues when the acupuncture are running all day, and the ease to the breath that results from the erect position. There is less danger of sloughing by this process, as a single incision is enough for each leg. If this man does not materially improve soon, we shall adopt this system, and then you will have an opportunity of seeing its effects.

Unhappily we can do nothing to restore the impaired action of the heart, but we

may do much to ameliorate the condition that that impairment entails. The principal indications are to support the patient's strength, to keep open the emunctories, and to adopt all the means in our power to get rid of the superfluous water.

CRIMINAL ABORTIONISM IN NEW YORK.

THE Report of the City Inspector for 1849, shows a large increase in the number of premature and still births.

"In three years we have a mortality from premature births of 400; and from still-born of 3,189; making a total of 3,589 (!) human beings that never breathed.

"Since 1805, when returns were first made to this office, the number of these accidents has steadily and rapidly augmented. With a population at that time of 76,770, the number of still-born and premature births was 47; in 1849, with a population estimated at 450,000, the number swells up to 1,320! Thus, while the population has increased nearly six times since 1805, the annual number of still-born and premature births has multiplied over twenty-seven times!

"The following table shows the rapidity of this increase:—

In 1805 the ratio to population was		
as	1	to 1612.18
1810	1	" 1025.24
1815	1	" 986.46
1820	1	" 654.52
1825	1	" 680.68
1830	1	" 597.60
1835	1	" 569.88
1840	1	" 516.02
1845	1	" 384.68
1849 (estimated)	1	" 340.90"

The facts thus exhibited demonstrably furnish proof that causes have been at work in this city during the last few years, and are steadily increasing, in a frightful ratio beyond the increase of the population, not merely but wholly inexplicable on any principles which do not recognise an amount of guilt at which humanity shudders. The city inspector indicates the truth when he says:—

"This is a state of mortality from these accidents truly alarming, which, while no remedy can be advised, demands our most serious consideration. What of crime and recklessness there is in this sum dare not be expressed, for we cannot refer such a hecatomb of human offspring to natural causes. An honest and fearless expression of the causes or circumstances attending these events, on the part of the medical attendant, would bring into this department an amount of valuable knowledge that might be useful in checking this horrible and increasing waste of life."

—*The New York Medical Gazette*, 1850.

Original Communications.

ON THE
CONTAINING-TEXTURE OF THE
BLOOD.

By WILLIAM ADDISON, M.D., F.R.S., &c.

[Continued from page 321.]

In what texture is inflammation seated? If we examine any soft part of the body, we find it composed of parenchymatous substances (nerves, muscles, and secreting substances), intermixed with blood-vessels and sundry forms of fibrous texture.

§ 1. *Of the parenchyma of different organs.*—The particular substances of different organs, as a consequence of the order of development in the embryo (p. 193), become placed outside the blood-vessels, disposed in groups or masses in the interstices between them. These divide themselves, by their physiological offices or functions, into three well-marked classes—*sentient*, *motor*, and *secreting*; which again, upon physiological grounds, are variously characterised and subdivided. Thus, of the *secreting organs*, the parenchymata and secretions are extremely different; of the *muscles*, some are voluntary, others involuntary; and, in the *nervous system*, the physiology of sentient elements differs in each of the organs of the senses. Those of the ear have a very different function from those of the eye, and so on. But let us review generally the plan of the conformation of the body, with reference to the parenchymata of different organs, in as far as they have relation to inflammation. That which appears simple to the unaided vision becomes exceedingly complex examined microscopically.

The skin and mucous membranes are *secreting organs* so much unfolded and spread out, that the parenchymatous substances upon which the secreting actions depend become disposed in distinct spots or groups, each with its separate outlet or duct, the intervals between being filled in with a corresponding development of fibrous tissue. On the contrary, in the internal secreting organs, the groups or lobules of the parenchyma are so closely folded toge-

ther, that not only is the bulk of the organ very greatly increased, but the ducts unite many times; forming a series of coalescing larger and larger tubes, which demand for their conformation and support a constantly-increasing thickness of dense fibrous texture, the submucous fibrous basis. But then, that no space may be sacrificed, all the lax areolar forms of fibrous tissue are reduced to the smallest possible compass that is consistent with the magnitude and requirements of the secreting mass.

Passing from the secreting parenchymata to the organs of purely animal life, we observe correspondent groupings and relations. *Muscular fibrillæ* (sarcous elements) are associated in great masses in the voluntary muscles, having between them the smallest possible amount of fibrous inter-divisions. Elsewhere the fibrillæ are spread out in thinner sheets, with a larger proportion of fibrous areolar tissue intervening; and in other places unstriped fibrillæ are distributed in almost single threads.

Sentient elements are congregated in prominent and distinct spots, termed *papillæ*, in the skin. They are exposed in thin and delicate sheets upon the organs of hearing, sight, and smell; but in nerves, spinal cord, and brain, they are folded and disposed within the smallest space. It is to be observed that, for the most part, the elements of the special parenchymata retain through life the cell-form. The prominent exceptions are *medullary tubules* belonging to the nervous, and *sarcous elements* belonging to the muscular system. These in the embryo, and for some period of foetal life, are represented by cell or germ-forms; but during growth there is a metamorphosis to the forms by which they are afterwards recognised. Nerves and muscles, therefore, are not primary but secondary forms; and these appear to re-unite after division, provided their dis severed ends be brought sufficiently near together. But this reunion, or rather incorporation, of medullary tubules and muscular fibrillæ, after division by cutting, does not appear to be a phenomenon of repair by the metamorphosis of lymph, but to arise from inherent properties in the substances themselves—a sort of out-growth of nerve and muscle substance: for, first, it does not seem to take place, except during youth; when, as we have remarked, growth is mingled with repair;

and secondly, when it does occur it is only some time after the process of repair as it relates to fibrous texture, or the formation of the cicatrix, has been concluded.

Now the elements of the particular substances of different organs cannot inflame, except in the meaning of secession, absorption, or disappearance, before new and interpolated forms, or of being intruded upon and deranged by the effects of inflammation. Correctly speaking, upon the testimony of microscopical analysis, inflammation is not seated in brain-substance, liver-substance, or muscle-substance; though we speak of such in consequence of the microscopic character of their relations to blood-vessels. No lengthened argument is needed by those accustomed to microscopical observation, to prove this with respect to the cell-particles and medullary tubules peculiar to the nervous system—the fibrillæ, or sarcoous elements of muscles, or the secreting cell-particles of the liver, kidney, and other secreting organs. But if the elements of the parenchymatous substances of different organs do not inflame, inflammation must necessarily be seated in the fibrous textures.

§ 2. *Of the fibrous membranes, fibrous textures, and areolar tissue, skin, and mucous membranes.*—What are the relations of the fibrous membranes?—are they correlatives of blood? Large venous sinuses from the brain traverse the *dura mater*, which are described by anatomists as situated between its laminæ, because of the perfect homogeneity of the elements of the coats of the sinuses and the rest of the membrane. The strength and thickness of the *dura mater* have an evident relation to the magnitude of the blood-streams it conveys. The blood-vessels of the *pia mater* are much more numerous and much smaller, and the membrane is in a corresponding degree thinner and more delicate. There is here the same species of fibrous elements in the coats of the vessels, and in the non-vascular parts stretching in the intervals between them. The *pericardium* is a thick fibrous membrane resembling the *dura mater*, and, like it, coincides in strength and thickness with the magnitude of the great vessels from which it is, as it were, reflected to cover and protect the central moving point of the circulation. The *areolar tissue* is one of the most ex-

tensively diffused of all the elements of organisation. So comprehensive is the association of this tissue with the blood-vessels, that it would be difficult to point out a single instance in which they are not enveloped by it. Even the capillaries of the coats of the larger vessels are invested by a sheathing of this tissue. The *cutis*, or fibrous basis of the skin, cannot be distinguished from areolar tissue except by the greater condensation and more intricate interweavement of the fibrous elements. "However great the difference may seem to be between the dense and closely-woven texture of the *cutis* and the lax areolar tissue, to which it owes its mobility on subjacent organs, they blend insensibly together. Their ultimate texture is essentially the same."* The same may be said of the fibrous basis, or the submucous fibrous tissue of the mucous membranes, excreting ducts, and tubes. This blending insensibly together of the different modifications of fibrous tissue furnishes the grounds of an affirmative answer to the question proposed. And such an answer harmonizes with the pathological facts—viz., the completeness with which all the phases of inflammation are exhibited in fibrous membranes: increased vascularity, lymph-growth, granulation, and pus, as belonging to the primary stage or proximate form; and adventitious fibrous texture of every degree of laxity and condensation, and ossification, as the secondary or more remote. But an affirmative answer embraces a very extensive class of textures. What, therefore, is the history of the growth of fibrous membranes? During growth in the embryo, all the fibrous textures appear from our researches to follow, *pari passu*, the phases of the coats of the blood-vessels. When the latter are corpuscular, the former are corpuscular also; the fibrous type is coetaneous in each. In their growing state these textures are not only greatly more cellular, but also greatly more vascular than when their fibrous type is concluded,—a fact which corresponds in a very striking manner with the cell-structure and high vascularity of "granulations," as compared with the low vascularity and fibrous structure of the "cicatrix." By these facts we trace relations not only between the cell and

* Pathological Anatomy, by Dr. Todd, F.R.S., and Mr. Bowman, F.R.S.

fibrous types of the blood-vessels and the growth of fibrous membranes, but also between these and the granulations and cicatrix of a burn—i. e., between growth and the first and second periods of repair—the rise and cure of inflammation; so that, whether it be that condensed form of fibrous tissue which, limiting and transmitting the streams of blood, we call blood-vessels,—or that closely woven form which backs and supports the glandulæ and papillæ of the skin and mucous membranes,—or that expanded membranous form which incloses the various parenchymata,—or, lastly, the lax and areolar form which admits of motion between contiguous parts, all are subject to the same pathological law: that is to say, the fibrous textures are the theatre of the morphological properties of blood (of repair, inflammation, and scrofulous disease), because they are correlatives of blood.

Thus we arrive at the full meaning of the terms we have employed to head the present inquiry,—*the containing-texture of the blood*,—a meaning not limited to blood-vessels, but comprehending with them all the variously modified forms of simple fibrous tissue. And we have been encouraged and led on to this comprehensive meaning step by step in our researches, considering that inasmuch as the very small vascularity of a tough fibrous cicatrix does not affect its relations to the previous highly vascular cell-granulations, and through these to the elements of blood, so it appears the small vascularity of a tough and condensed, or lax and areolar form of any original fibrous membrane does not deprive it of its physiological correlation and *consanguinity*.

§ 3. *Of inflammation, the first period of repair, abscess, ulceration, and organic disease.*—Inflammation to the ordinary observer appears as a phenomenon of redness, heat, swelling, and pain. To us it appears as a phenomenon of change or *morphology*, affecting, and limited in its definitive character to textures, correlatives of blood in which *cells and cell-growth supplant fibrous forms*. This definition of inflammation is founded upon the facts which have been discussed, and is corroborated by the distinctions recognised by all of us between congestion, hyperæmia, blushing, and inflammation. In the former the coats of the blood-vessels do not experience any morpho-

logical change or essential alteration of type. In the latter they do; the change from fibres to cells adapting them to the supply of new vessels, and to the support of new growths (p. 318).

Whatever be the view entertained by different pathologists respecting the nature of inflammation, whatever the definition they may deem most appropriate, and whether they regard it in its benign and physiological aspect as the first period of a *process of repair which is to follow* (p. 317), or in its morbid aspect as a destructive process, there can be no possible doubt about the appearance or accumulation of cell-forms—termed *lymph-particles*—upon and around the blood-vessels; nor, looking to the general history and physiology of cells in every department of the animal and vegetable kingdoms, do we think there can be any reasonable question raised as to the lymph-particles being, in the examples we are reviewing, the agents which determine the observed changes, which disintegrate and supplant, or *absorb* the fibrous texture in abscess and ulceration. Lymph-particles cannot inflame. These, as we have before said, are microscopic whole organisms, and as such liable themselves to *inherent deteriorating changes*, which modify their character as physiological and pathological agents, diminish their co-ordinated vital properties, and cause them to appear under sundry microscopic aspects, as exudation-cells, pus-particles, &c.

Thus it seems to be the deteriorating changes in the constitution of the cell-particles, in the example of chronic scrofulous abscess already referred to (p. 318), that retard and render chronic, scrofulous, and incomplete, an action which, had the cell-particles been plump, vigorous, and healthy, might have proved simple inflammation followed by the cure. Weakness, incapacity, and death, in the agents of an operation, stop the work, and lymph-particles, which are irregular and effete, render inflammation chronic, and cure tardy. If the microscope brings us physiologically to a point where the vital energies of cell-particles are the necessary antecedents of fibrous structures, so it brings us pathologically to the same point in interpreting the phases of repair and inflammation. *And, as in growth in the embryo, every fibre taking the place of a cell-particle is an element in natural development contributing to the concluded or adult form of fibrous tissue;*

so conversely, when fibrous textures have been established, every cell supplanting a fibre is an element of retrograde metamorphosis; and according to the conditions of the supplanting cells does this assume the physiological, or degenerate to some pathological aspect.

It is the pre-established, the seceding fibrous coats of the blood-vessels, to which we refer as the seat or subject of inflammation: and as regards them in the first instance, the action is the same whether it leads to repair or ulceration, the difference arising in the one case by normal fibrous forms asserting their supremacy; in the other, by the primary cell-action pursuing an unchecked course. The reasons of the difference is the field of speculation (p. 319). Our conclusions are, that textures correlative of blood are the seat, and lymph-particles the material agents; repair, inflammation, and scrofulous disease, the phenomena.

But though the elements of the parenchymatous substances peculiar to different organs do not inflame, they are subject to inherent deteriorating changes. This has been demonstrated with respect to the secreting cell-particles of the liver, and the elements of the vitreous body of the eye, by the observations of Mr. Bowman; and by the researches of others who have employed the microscope with respect to the elements of brain-substance, muscular fibrillæ, and the secreting cell-particles of the kidneys, &c. &c.

And thus we arrive at the anatomical basis of the distinctions recognised in practical medicine and pathology between *organic* or *specific* and *inflammatory* diseases: that is to say, between degeneration or disease of the special elements of an organ, and the changes, interpolations, and intrusions, incidental to the properties of blood and fibrous tissue—phenomena of inflammation.

It is no part of our present purpose to touch upon the subject of organic or specific diseases further than may be necessary to render intelligible the facts connected with inflammation.

In every organ within microscopic areas, there are elements of the common and of the specific.—The elements of the common are blood, blood-vessels, and fibrous tissue; the elements of the specific is the matter of the specific function: therefore there are in every organ of the body, and within

microscopic areas, elements of two kinds of hypertrophy, atrophy, degeneration, and disease—inflammatory and specific. But such is the minute or microscopic scale upon which the different elements of the living structure commingle and incorporate, that before disease can become an object of practical interest disturbed expressions from the *super-added* become mingled with and complicate the *fundamental*: and thus the function of the parenchyma gives complexion to the symptoms of inflammation. For the illustration of these doctrines we go not to the transcendental, but to the practical.

There are assemblages of symptoms in the living, and results to be seen without microscopic aid, in the dead body, which conclusively establish the fact of their being distinguishable diseases, in which the deviation from the standard of health and healthy structure begins in the elements, and progresses from group to group of the parenchyma, the general fibrous basis and the blood-vessels remaining comparatively but little affected, or clearly suffering in a secondary manner; and conversely, that there are other diseases,—scrofulous diseases, for example, in which the persistence of granulations, ulceration, and pus discharges, proclaim a persistent retrograde condition of blood-vessels; fibrous textures overrun with cell-growth constituting the definitive feature of the disorder. In the liver, kidney, heart, and brain, where specific elements are very densely congregated, and fibrous textures reduced to their minimum amount, specific diseases eliminate themselves in a recognised manner from phenomena of inflammation; and in the skin and mucous membranes, where groups of particular substances are comparatively widely separated, and fibrous texture largely developed, the distinctions we are enforcing are not the less well marked and distinguished.

§ 4. *In the skin*.—Small-pox, herpes, lepra, &c. are diseases quite distinct from erythema and erysipelas. The morbid action in the former is circumscribed, limited within a sphere around the glandulæ and follicles, or groups of them; in the latter it is diffused, spreading widely in the common fibrous basis. Evidence to the same effect—and, moreover, we would impress upon the reader, establishing the specific action of poisonous substances—is furnished upon

the application of irritants to the skin. Blisters excite inflammation in the fibrous basis. Croton oil produces a more pimply redness—an eruption: and antimonial ointment causes pustules to arise resembling those of small-pox. In all these instances there is inflammation, a deteriorating change in the coats of the blood-vessels. But in the one class of diseases this is circumscribed about the groups of the particular substances; in the other it spreads widely in the fibrous basis or areolar tissue, irrespective of glandulæ and follicles. In the former examples inflammation wears the aspect of the *superadded*; in the latter, of the *fundamental*.

§ 5. *In the mucous membranes*.—The mucous membranes, composed of a common fibrous basis, and groups of villi, papillæ, glandulæ, and follicles, are analogous to the skin, and are therefore obnoxious, *in virtue of the common*, (bloodvessels and fibrous textures), to congestion, effusion, hyperæmia, anæmia, and all the phases of inflammatory action; and *in virtue of the special*, to specific hypertrophy, wasting, degeneracy, and disease, to pimply eruptions, acuminated pustules, and specific ulcerations. Amongst the diseases of the mucous membranes, Billard distinguishes in infancy the *follicular* from the *erythematic*: and Rokitsky distinguishes in the adult those of the mucous surface from those of the sub-mucous fibrous basis, gelatinous softening of the stomach, and what he denominates the *typhous process*, from inflammation.

"If we consider," says the latter distinguished pathologist, "that gelatinous softening in none of its stages presents, either at the point of softening or in its vicinity, either hyperæmia, injection, or reddening, and that we are still less able to demonstrate upon the inner surface of the stomach, or in the tissue of its coats, the products of inflammation, we are constrained to infer the non-inflammatory nature of the affection." "This conclusion," he says, "gives a key to the various kinds of softening that occur at advanced periods of life under similar circumstances,—viz. in cerebral affections.*"

To follow out physiologically and pathologically the analysis here indicated with respect to the common and

the special in all the organs of the body, microscopic and large, would be, indeed, to enter upon a wide field of anatomical research: enough has been said to indicate the principle, and to eliminate the fibrous textures as the seat of inflammation, repair, and scrofulous disease.

§ 6. *Of repair, and the imperfections or limitations of cure*.—The characteristic pittings of small-pox, and the analogous marks which may be found remaining after a severe application of antimonial ointment to the skin, not only point out upon what texture the morbid action has been concentrated, but they indicate—as do the scars and seams of a burn—that the parenchyma of the glandulæ is not restored or reconstructed by the process of repair, which fills the void, not by restoring the destroyed or lost form, but with simple fibrous tissue. Pathological anatomy has long demonstrated analogous facts in the mucous membranes: and it appears from our observations in these and other examples of repair or cure, where an original or germ-form has been destroyed, that, with the original form and substance, *the principle* governing the natural distribution of the blood-vessels has also departed; for the arrangement or disposition of these in a scar or a cicatrix, in granulations and lymph, appears to us never to be the same as it was in the unimpaired original growth. Such being the facts, we have, in these instances at least, no evidence of a repetition or revival of the "germ force." On the contrary, we see only effects arising from those qualities of blood which, wherever blood extends, originate a limiting or bounding texture, a correlative of blood, the first phase of which is *corpuscular*, and the concluded form *fibrous*. These imperfections or limitations of cure in the skin may be of little consequence to the welfare of the person; but in mucous membranes and internal organs they become of much more importance: and allied to these in their consequences upon the general or constitutional health are the unnatural fibrous adhesions which very frequently attend the cure of severe inflammation on the free surfaces of fibrous membranes. But we have elsewhere spoken at length upon this part of our subject,* and are therefore content with the following summary:—

* Pathological Anatomy: Sydenham Society's edition, vol. ii. p. 33.

* Healthy and Diseased structure, part ii. ch. 2.

The first phenomenon of repair is inflammation, cells and cell-growth interposing fibrous texture. The second phenomenon of repair is the cure of inflammation; natural fibrous forms abolish and exclude the cell-growth period. But there is often a pause between these two periods. Inflammation subsides, but cure does not advance: unnatural cell-growths maintain their footing, but they do not spread. This is *scrofulous disease*,—a persistent form of retrograde metamorphosis.

Finally, the human body, regarded *analytically*, is composed of three great systems of organs—sentient, motor, and secreting—nourished by the circulation of the blood, and sustained by the coherency of textures, correlatives of blood. In this point of view we have sought to determine the seat of inflammation, and to extricate its definitive phenomena from their complications with the elements and physiology of the parenchymatous substances. Desirable as this is on many grounds, particularly as in interpreting pathological appearances, we must remember that analytical views are unsatisfactory in the practical treatment of disease; because, in the first place, such is the minuteness of the scale upon which different physiological substances commingle and are co-ordinated, that there are everywhere in the living structure, and *within microscopic areas*, elements of the common and of the specific: so that if disease fundamentally begin in the one, it affects the other before it can become an object of practical interest or regard: and in the second place, *synthetically* viewed, all the various elements of the living body are so mutually incorporated and interdependent, that the whole of them form but one person. Look at the relations between *sentient* and *motor* elements. Muscles act instantaneously upon the dictates of the will; but disturb sentient matter, and muscles are divorced from the dominion of the will: they remain quiescent, and waste away. Are not muscles, then, exquisitely sensitive? *Sentient* and *motor* elements are both incorporated with the simple fibres of the *fibrous tissues*, and immediately feel the influence of the first inroads of inflammation. In the deep interior of the brain *sentient elements* are brought into such close and mysterious relations with the *elements of blood*, that the ordinary nature and character of the limiting tissue

of the blood is dispensed with, a kind of embryonic type of circulation here prevailing. Blood and medullary matter seem in this instance to have nothing interposed between them. In the *secreting organs*, and *within microscopic areas*, multitudes of the *secreting cell-particles* are attached to the *containing texture of the blood*: and in the liver the same continuous relations are thought by the best anatomists to prevail between the *secreting elements* and the *venous blood*, as appear to exist in the brain between *sentient elements* and *arterial blood*. It is these synthetical relations which prove to us how dependent every part of the body is upon the circulation of blood, and deprive analytical investigations of the extensive influence which otherwise they must have had upon the art of therapeutics. In the embryo, *germ-masses* or groups of sentient, motor, and secreting elements are founded prior to the flowing of the blood current, but the growth of blood vessels of fibrous textures and bones, upon which the relations of forms and functions depend, is posterior to the circulation; and the healthy constitution of these textures hinges upon a normal metamorphosis of the elements of blood. Such being the facts, what, then, replenishes and maintains the circulating fluid?

In answering this question, we enter upon another and a widely different sphere.

Food, drink, and air, incorporate with blood. Here we pass from the department of the living body to the world of external Nature; and it is to be observed, that the elements of the one cannot be said to be *more necessary* to the *phenomena* of life and health than are those of the other; for the body dies as soon deprived of air as it does deprived of blood.

78, Wimpole Street, July 20, 1850.

VESICULAR DISEASE IN A NEW-BORN INFANT.

THE disease appeared on the second or third day, and lasted about a week. Upon the upper extremities the vesicles were quite numerous, and about as large as the head of a pin; but upon the abdomen, where there were only a few, they were probably from four to six lines in diameter. There was little or no redness about them, and they at last dried up.—*American Journal of Medical Sciences* for July, 1850.

AN ESSAY ON
UNHEALTHY INFLAMMATIONS.

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[Continued from p. 411.]

ERYSIPELAS (continued).

A true blood poison disease—Physical peculiarities of the blood—the clot—the serum—Clinical peculiarities—Excess of fibrin in all the phlegmasiæ—In erysipelas—No measure or proof of sthenic action—Proximate cause of the disease—Opinions of the older writers thereon.

But if we turn to the *humoral* aspect of the disease, a far more satisfactory escape from these perplexities will be found, if I mistake not, to await our inquiries; and under this conviction I shall now devote some space to a consideration of the fluids in erysipelas.

Physical characters of the blood in erysipelas.—The blood of an erysipelatous patient, after standing for a while, undergoes the usual separation into serum and crassamentum. But different writers have given different descriptions of the physical appearances of each of these; the clot by some being characterized as loose and large; by others, the reverse of these. This discrepancy would seem to arise from the probability of the blood so examined having been derived from different forms of the disease; for, where the areolar tissue has become involved, and febrile action runs high, the clot, *a priori*, might be expected to be more consistent than in another case where the cutis only was affected, and little constitutional disturbance present. The serum, on the other hand, seems almost by common consent to possess unusual qualities, being described by writers in general as thin, acrid, less glutinous than it should be, and remarkable for its deep yellow dye; or, as some say, for its mixture of yellow and green. Bursarius speaks of the blood as “*rutilans, spumidus, vis coherens*” (Inst. Med. Pract. vol. ii. p. 22). Sennertus as “*tenuior et calidior*” than in phlegmon (De Fabr. lib. ii. c. 16), &c. &c. But, not having the blood of any erysipelatous subject on my own hands, or on my conscience either, I cannot speak from my personal know-

ledge of its state during life. After death, it is in general found to be darker than in those who have died of healthy forms of inflammation, and likewise *semi-fluid*, having from this latter quality escaped into the areolar tissue and cavities of certain organs.

Constitution of the blood in erysipelas.

—Almost all observers who have analysed the blood in this disease are agreed as to an *excess of fibrin* being a prominent feature in its chemical history. Bearing in mind that in healthy blood the mean amount of fibrin is about 2·2* in 1000 parts, the following statement will show a notable increase in its proportion in erysipelatous affections. “Andral and Gavarret have made eight analyses of the blood of five persons, four of whom were suffering from erysipelas of the face, and one from inflammatory erysipelas of the foot. In seven of these cases the fibrin was materially increased; in three instances it amounted to 5·0; in three to 6·0; and in one to 7·0; in a much shorter and milder case, in which there was but little fever, it amounted to only 3·8.”†

Rindskopf, in a case of erysipelas of the hand, found the fibrin increased to as high a point as 7·71‡ in 1000 parts. Heller, in a case of erysipelas of the face, estimated it at 5·45.§

Simon has not analysed the blood himself in erysipelas. Garrod merely repeats the asserted fact, on Andral’s and Gavarret’s testimony, that the amount of fibrin “is very great:” and Thompson only incidentally does the same.||

Andral and Gavarret found a notable diminution in the amount of red globules; and this conclusion would appear to confirm the opinion of Simon, that in proportion to the augmentation of the fibrin is the diminution in the amount of red globules; for he lays it down that the fibrin is derived directly from the constituent elements of the blood-globules,—an opinion to which the former chemists do not assent, who rather maintain that the hæmatoglobulin

* Lectures on the Chemistry of Pathology and Therapeutics, by Alfred B. Garrod, M.D.; Lancet for 1848, vol. ii. p. 86. Lecanu estimates it at something higher than 3; and Christison at 3.

† Animal Chemistry with reference to the Physiology and Pathology of Man, by Dr. J. Franz Simon, vol. i. p. 270.

‡ Simon’s Chemistry, loc. citat.

§ Ibid.

|| Chemistry of Animal Bodies, by Thomas Thomson, M.D., p. 374.

is increased in plethoric conditions of the system.*

In the present unsatisfactory state of our knowledge in relation to the diseased conditions of the blood, I do not think it would be for the interests of my present subject to enter more minutely into the analysis of this fluid in erysipelas; for although we might attain to the knowledge of several individual facts in relation to its other constituents, we know so little at present of the *true interpretation* intended by nature to be placed upon the changes which they undergo in disease, that such inquiry would almost necessarily precipitate me into speculations, and lead us away from the high road of our subject.

"Amicos Cicero, amicus Plato, sed magis amicus Veritas."

Let us confine ourselves, then, to a few observations upon the most remarkable and interesting peculiarity which meets us at the outset in our examination of the animal chemistry of erysipelas,—to wit, the *notable augmentation in the amount of fibrin* which the blood in this disease discloses.

The occurrence of an increase in the amount of fibrin in disease appears, by the common consent of all inquirers at the present day, to be conclusive as to the existence of a state of *hypermosis*, or of an inflammatory condition, in whatsoever circumstances of the system the blood betrays such alteration. "The development of an inflammatory disorder," observe Becquerel and Rodier, "produces remarkable modifications in the composition of the blood, of which the most striking is the increase of fibrin." Simon is equally explicit upon the point. "We find accompanying these physical symptoms" (alluding to the density of the clot) "an excess of fibrin and a diminution of hæmatoglobulin, as well as of the solid constituents of the blood generally; and in proportion to the degree in which these phenomena are observed, we may infer a greater or less amount of inflammatory action."

The direct connection, then, between

* Simon believes that the opposite opinions on this subject between the French chemical physiologists and himself may be traced to the different methods of analysis adopted by them respectively. Nasse has also found that the corpuscles and fibrin are generally in an inverse ratio; and Becquerel and Rodier's experiments show the same in their analysis of blood in a number of cases of well-marked inflammation.

an excess of fibrin in the blood and a state of inflammation, appears to have become a necessary and fundamental canon of our faith—a "golden image" of truth "set up" by the Nebuchadnezzars of our art, before which we are commanded to bow!

But, though indicative of a *certain condition of sanguification*, the important practical question to be solved in connection with its occurrence is, whether this excess of fibrin can be looked upon as a measure of *power* in the system at the time? For, it is the very essence of that truth (if it be a truth) which I am labouring to establish, that one inflammation may be associated with strength, and another with the opposite condition. In these opposite forms of inflammation, however, it seems to be clearly established that an excess of fibrin is equally characteristic of each. It is in marked excess in *phthisis*, as well as in some other diseases of a still more decidedly asthenic nature.

And now for a *fact or two* in connection with the circumstances under which the fibrin discloses itself in excess; for facts, like figures, constitute a stubborn sort of arithmetic.

Chlorosis.—Becquerel and Rodier have furnished a table showing the mean composition of the several constituents of the blood in six chlorotic girls. That of the fibrin they found to be 3.4.

"Andral and Gavarret have made twelve analyses of the blood of nine cases of confirmed chlorosis. In their cases the maximum amount of fibrin was 3.6; the minimum 2.1; the mean 2.9.

"Golding Bird states that the blood in chlorosis forms *just as solid a clot as in inflammatory diseases*: and Jennings (*Lancet*, 1840, p. 887) observed even a *buffy coat* on the clot of chlorotic persons in the absence of all inflammatory symptoms."* Andral and Gavarret have also constantly observed the buffy coat in chlorotic blood. Simon admits that the quantity of fibrin in chlorosis "is normal, or only slightly diminished;" whilst most of the French and German writers declare that it is *in excess*.

With the great diminution in the amount of hæmatoglobulin, and with the excess of water in the blood of chlo-

* Simon's Chemistry.

rotic subjects, I have nothing to do in the present consideration of its constitution. That which now concerns us alone is the fact, that in a disease the nature of which is so much the opposite to that of inflammation that death every now and then overtakes the subject of it from no other apparent cause than a cessation of the heart's action for *lack of the power to go on*, that constituent of the blood which is believed to endow it with the quality of *strength* is generally present in excess. In a word, that the blood in chlorosis is distinguished by a feature, the presence of which in disease is the acknowledged symbol of inflammation,—viz. an excess of fibrin.

Scorbutus.—Dr. Garrod has examined the blood in this disease, and “sometimes found a buffed and cupped state of clot; sometimes a normal clot; but never a dissolved state of the fluid.”* He quotes the results of Mr. Busk's experiments upon scorbutic blood in three cases of the disease, in all of which the amount of fibrin was in excess.† In the first its amount was 6·5; in the second 4·5; and in the third 5·9: and yet scorbutus is a disease in which the vis vitæ is, by general consent, regarded as being below par.

Acute diseases.—In some of the phlegmasiæ an excess of fibrin will be found in the blood after the patient has been almost blooded to the edge of his grave! Thus, in an analysis of the blood of a rheumatic patient by MM. Andral and Gavarret, who “had been bled six times, besides having had 200 leeches applied,” the fibrin was still found in *considerable excess*—i. e. at 4·6!‡

We know this to be a common circumstance in rheumatism—a pre-eminently “*unhealthy*” form of inflammation, as I shall probably endeavour to prove by and by.

Andral and Gavarret, again, have given the analysis of the blood in three cases of pneumonia, in the first and second of which four blood-lettings were practised, and three in the last. It will be interesting to tabulate the changes in the amount of fibrin which were consequent in each of these cases upon the depletion.

	Venesection.	Fibrin.
1st Case	1.	4·0
	2.	5·5
	3.	6·5
	4.	9·0 (?)
2d Case	1.	5·2
	2.	7·3
	3.	6·9
	4.	8·0 (?)
3d Case	1.	5·5
	2.	6·8
	3.	6·4

Mons. Simon (from whose work the foregoing sketch is derived) endeavours, it is true, to explain away this very striking disclosure. “If the bleedings,” says he, “are carried beyond a certain extent, the fibrin as well as the corpuscles are diminished.” But having the clearest evidence that after the *fourth* abstraction of blood the amount of fibrin is in notable excess, and that each depletion augmented its proportion, I think I might be almost justified in the suggestion, whether the diminution in the amount of fibrin in subsequent bleedings—that is, when “carried beyond a certain extent,” as declared by Simon to be the case—might not be satisfactorily accounted for upon the supposition that the fibrin is only diminished from the circumstance of there being *no blood left to furnish it?*

Mons. Andral has some observations upon the various circumstances under which fibrin is found in the blood as well in excess as below the healthy mark, which are so interesting and pertinent to my present subject, that I think I need offer no apology for inserting them here at length.

“The fibrine may be altered either in quantity or in quality. In the first place, there are cases in which this principle is more abundant than usual, or at least in greater proportion relatively to the water and albumen. In such cases, the blood, when drawn from a vein, forms in the vessel that receives it a clot with little or no serum. These are, however, to be *divided into two classes*. In the first class of cases the fibrin constituting the clot still contains a pretty large quantity of serum, which may be separated from it by pressure: in these the coagulum has but little density. In the second, on the contrary, the clot is very dense, and a little fluid albumen can, with difficulty, be squeezed out of it. In the first class;

* On the “Chemistry of Pathology and Therapeutics,” Lancet, 1848, vol. ii. p. 142.

† Mr. Busk has also found the blood in some of his cases both buffed and cupped.

‡ Simon, vol. iii. p. 277.

the relative increase of quantity of the fibrine is only apparent; in the second, it is real. We must take care not to confound them, as they belong to different states of the system. The very fibrinous blood is commonly called *rich blood*: it may either depend simply on a vigorous constitution, or on certain morbid states.

"We learn, then, from observation, that in the same quantity of blood the fibrine may exist in various proportions, being sometimes above and sometimes below the regular complement; how this fibrine is itself composed of determinate quantities of oxygen, hydrogen, azote, and carbon; and, if these elements are derived from the air and food, and are found in more or less considerable proportions in the different excretions, it is natural to suppose that they may also exist in variable proportions in the fibrine, and thus modify its nature: and, indeed, it is possible that their relative excess or deficiency may have some influence in the production of certain morbid states.

"The force that tends to keep the globules of fibrine at a certain distance from each other during life, may be so modified as that they shall have a tendency to run together, as they naturally do after death; and hence may result the spontaneous coagulation of the blood in its vessels during life.

"We have just been considering the cases in which there is a real augmentation of the force of aggregation that keeps together the particles of the fibrine of the blood. In other cases there is, on the contrary, a diminution of this force; and the result is, either a less tendency in the blood to coagulate, or a total absence of coagulum. When there is one, it is remarkably soft, offers scarcely any resistance to the finger, and is converted by a slight agitation into a reddish fluid. In other cases there is no coagulum, and we find the fibrine broken up into small fragments, that remain suspended in the serum, or fall to the bottom of the vessel. Lastly, in other cases there is not any appearance even of these fibrinous particles, but the fibrine is completely mixed with the serum, producing a fluid mass of a reddish or blackish colour. These different appearances presented by the blood when drawn from a vein, may be observed also in the vessels in the dead body: these are sometimes

filled with coagulated blood of greater or less consistence; while on other occasions there is nothing to be found in the heart, arteries, or veins, but a perfectly fluid blood, resembling water charged with a red, brown, or black colouring matter. In such cases it has been ascertained by chemical analysis that *the fibrine is not absent*, but that it is altered in its nature, so as to be no longer spontaneously coagulable. In some few cases I have seen the fibrine assuming still another appearance: instead of a coagulum, there was at the bottom of the vessel a homogeneous stratum, sometimes of a deep brown, and sometimes of a dirty grey, resembling bad pus more than blood."*

In these most interesting observations we discover an explanation of that apparent contradiction in the behaviour of fibrine in diseased states of the blood, which consists in the fact that, while we acknowledge the firmness or looseness of the clot to be dependent upon the greater or less amount of fibrine in the same, that constituent of the vital fluid may still, by analysis, give evidence of its presence in excess, under circumstances in which its entire absence might, *a priori*, have been looked for. The fact that fibrine may part with that property which is thought to be almost distinctive of its existence—namely, its *tendency to coagulate*, is, as far as I know, a new and highly important feature in its history, and one a consideration of which tends to clear up much of the difficulty and obscurity to which I have just adverted,—its actual presence, as shown by analysis, where a *dissolved* state of the humors appears to be a prominent condition.†

It may not be out of place here, per-

* Pathological Anatomy, vol. i., pp. 645-9.

† In his second Lecture on Inflammation, Mr. Paget records the following case, which has an interesting relation to the present subject:—"A man received a compound fracture of the leg, and it was followed by phlegmonous inflammation and abscesses up the limb. As soon as the inflammation had subsided enough, the limb was amputated; and three days afterwards, in examining it, a quantity of serous-looking fluid oozed from the cut through the integument. I collected some of this, and it formed a perfect fibrinous clot; yet the fibrine in this case had remained among the tissues *without coagulating* for three days after the death of the limb, and for many more days during the life of the patient." "One can rarely tell (adds Mr. Paget) why the coagulation of the fibrine in these cases should be delayed. There are here the same difficulties as are in all the exceptions from the general rules of the coagulation of the blood."—MEDICAL GAZETTE, June 14, 1850, pp. 1010-11.

haps, to advert briefly to M. Simon's views of the rationale of the changes superinduced by inflammation in the constituents of the blood.

"Numerous observations" (he observes) "have shown us that blood retained for any length of time in an organ, and thus prevented from meeting with a due supply of oxygen, becomes poorer instead of richer in fibrine; whereas, there is undoubted evidence that in inflammation the fibrine is increased." M. Simon believes that as, in inflammatory conditions of the system, a more rapid circulation of the blood is induced "through the lungs, and other organs that exert a modifying influence on its composition," the blood in consequence gives off a larger amount of carbonic acid than in the normal condition, and imbibes an extraordinary quantum of oxygen; and that, on this account, the development of the blood-corpuscles will be hastened, their vitality heightened, and more corpuscles be consumed than in the normal state; bearing always in mind that M. Simon supposes the fibrine to be directly formed from the blood-corpuscles.

"Hence" (he says) "if we only assume that the circulation is increased by the reaction of the organism in inflammatory affections, an explanation is at once afforded us of the change that occurs in the composition of the blood in hyperionosis, and, at the same time, of its heightened temperature" (vol. i., p. 286).

In brief, that the greater the frequency with which the blood performs its revolution through the lungs, and is submitted to fresh collision with atmospheric air, the greater is the manufacture and subsequent metamorphosis of blood-globules into fibrine. Simon admits that the increased velocity of the circulation cannot be assumed to be the *sole* cause of the change in the blood; by which admission he anticipates the objection to which he would otherwise have exposed his hypothesis—to wit, that in typhus fever, and many other diseases of prostration, there is a great increase in the rapidity of the blood's transit through the lungs, where the fibrine is notably deficient in that fluid.

Reviewing the several conflicting circumstances in connection with the excess or deficiency of fibrine in the blood which I have now imperfectly brought under

review, I think I am warranted in deducing from them the following conclusions:—

1st, That although an excess of fibrine in the blood does appear to be a *constant* accompaniment of inflammation, it is not infrequently met with also in *opposite* conditions of sanguification; whilst its amount is further influenced in some cases by the extent of the orifice through which the blood flows, and by the form of the vessel which receives it.

2ndly, That in the inflammations, strictly so-called, it is a common occurrence to find its amount increased in proportion as we reduce the entire mass of the blood by venesection; wherefrom it would seem that it is not upon a plethoric condition of the individual that its excess, even in these cases, depends.

3dly, That there is absolute proof, in the foregoing circumstances, that its excess in the blood is *not a measure of the amount of power in the system* and, consequently, not a warranty for depletion or for depression.

4thly and lastly, That we know very little either of its natural or morbid history beyond the fact, that its excess or deficiency "*occurs by the will of God*,"* and does not justify us, in the present state of our knowledge, in putting our own interpretation upon its meaning.†

If, in the foregoing analysis of the *actual amount* of information which we possess, as derived from a consideration of the chemical peculiarities of the blood in disease, I shall appear to be setting up the thoughts of a very humble and insignificant pretender in array against those of established authority, and to be seeking to loosen the foundations of those structures which better men have erected as beacons for the ordinary traveller; if I should seem to figure in such light in the eyes of the "master builders of the

* Avicenna.

† "In estimating the quantity of fibrine, chemists have not taken account of the *white* corpuscles of the blood. These cannot, by any mode of analysis yet invented, be separated from the fibrine of mammalian blood; their composition is unknown, but their weight is always included in the estimate of the fibrine. In health, they may, perhaps, add too little to its weight to merit consideration; but in many diseases, especially in inflammatory and other blood diseases, in which the fibrin is said to be increased, these corpuscles become so numerous, that a large proportion of the supposed increase of fibrine must be due to their being weighed with it. On this account all the statements respecting the increase of fibrine in certain diseases need revision."—Kirke's Handbook of Physiology, p. 66.

commonwealth of learning," my exculpation of myself and of my misdeeds must be—

That "I pretend not to publish this essay for the information of men of large thoughts and quick apprehensions. To such masters of knowledge I profess myself a scholar, and therefore warn them beforehand not to suspect anything here but what, being spun out of my own coarse thoughts, is fitted to men of my own size; to whom, perhaps, it will not be unacceptable that I have taken some pains to make plain and familiar to their thoughts some truths which *established prejudice*, or the abstractedness of the ideas themselves, might render difficult. The imputation of novelty is a terrible charge among men who judge of men's heads as they do of their perukes,—by the fashion, and can allow none to be right but the received doctrine." But "he who has raised himself above the *alms-basket*, and, not content to live lazily on scraps of begged opinions, sets his own thoughts on work to find and follow truth, will (whatever he lights on) not miss the hunter's satisfaction; every moment of his pursuit will reward his pains with some delight; and he will have reason to think his time not ill-spent, even when he cannot much boast of any great acquisition."*

Having thus endeavoured to make apparent what this disease is *not*, let us lose no further time in examining into the vexed question of what it is.

I have expended some ink-shed in an endeavour to show that its simple phlogistic nature was not established by the coexistence of an excess of fibrine in the blood; whilst the several peculiarities in its natural history, to which I have briefly also adverted, suffice to point out its *distant relationship only* to the family of the true phlegmasiæ.

In tracing the literary history of erysipelas (vide *Lancet*, Dec. 22, 1849), I dwell upon the interesting fact, that from the earliest periods of medicine this disorder had been regarded by the observant ancients as an *unhealthy form of inflammation*, and not the *product of pure blood*. Thus Galen laid it down that it has its origin in a "*bilious humour*;"† and such strong hold did this

belief take of the minds of his successors, that it has been handed down very nearly to our own day as an established fact in the pathology of the disease. Whilst Paulus Ægineta significantly distinguishes a phlegmon from erysipelas as a disease in which "*good blood* of moderate consistence rushes abundantly to a part," &c., the latter (erysipelas) being known "by the symptoms of very acrid bile putrified along with a deficient blood," Burserius, who has left us an excellent history of erysipelas, attributes its cause to "humor quidam tenuis, calidus, acerque, *biliosæ sanguinis* parti a veteribus dictæ quam similissimus, intus utcumque genitus, et primum nervosum genus, deinde cor, arteriasque repente percollens, a reliquis paulatim liquoribus, motu febrili oborto, secedens, atque ad cutim alicubi propulsus, aut in ea primario etiam defixus, et locum in quo sistitur, vel latebat, *peculiariter* irritans atque inflammandum."*

Hoffmann was convinced that erysipelas has its origin in some acrid poison: but, while in one place he tells us that that poison is "not of a bilious or saline, but rather of a caustic and acrid nature," he immediately afterwards declares it as his opinion that it springs from "*bile in a state of corruption*, to the degeneration of which many causes have contributed."‡

Some colouring might be given to this time-honoured doctrine by a reference to the most recent researches into the mutual action of bile and blood upon each other. I have stated that Andral and Gavarret found a marked diminution in the amount of red globules in erysipematous blood. Now, according to Hunefeld, and to Simon (who confirms the observations of the first), "on the addition of fresh bile the blood immediately becomes clear, and the *corpuscles disappear*."†

Simon experimented upon the blood with pure bilin, which "produces the instantaneous solution of the corpuscles." Some uncertainty, however, still

cap. 1).—"Si ex sanguine et flava bile" (the reader is aware that the ancients attached great importance to the distinction between yellow and black bile) "justo calidioribus fluxio mista fuerit, aut ex sanguine, quidem, sed fervido et substantia tenuissimo" (or even from hot blood alone much attenuated), "erysipelas vocatur ille affectus."

* Institutionum Medicinæ Practicæ, &c. Burserius, cap. II. de Erysipelas, p. 23.

† Febris Erysipelacea.

‡ Simon's Chemistry, vol. i. p. 110-111.

* Locke.

† Galen does not appear to have thought the presence of bile in the blood always necessary to the production of erysipelas; for he says in one of his works (Meth. Med. ad Glaucum, lib. II.

prevails amongst even the highest order of animal chemists as to the effects of the constituents of bile upon the circulatory mass; whilst certain of them are even of opinion that they have never been discovered in the blood at all. Becquerel and Rodier found the serum, in cases of icterus, always tinged with bile pigment: and Tiedemann and Gmelin have drawn the same conclusions from their analysis of the serum of icteric blood, whilst the clot has been of the colour of healthy blood.

Schönbein has found the serum in erysipelas to be invariably tinged yellow with the colouring matter of the bile. But then a bright yellow colour distinguishes the serum also in several other diseases. Such is generally the case in phthisis.* Van Swieten has the following not impertinent remark upon this conceit of the ancients. "As the part invaded with an erysipelas appeared of a yellowish-red colour, therefore the ancient physicians accused the bile as the principal cause; but we at present know that the serum of the blood is naturally yellow; so that if a little cruor stagnates with much serum in the pellucid vessels which are obstructed and inflamed, the affected part will then appear of a reddish-yellow colour."†

"An erysipelas," says Wiseman, "is generated of a hot serum in the blood, and affects the superficies of the skin with a shining, pale, red, or citron colour." "This serum, according to its several degrees of recess from its natural state towards that of cholera, doth produce several diseases. When the recess is less, and the sharpness of it but little, then it doth only cause a reddish blush upon the skin, from which the name of erysipelas is, or at least may be, derived." "That which I propose to treat of is the erysipelas à sanguine bilioso from choleric blood, which affects only the outward parts, none of which escape its tenuity and subtileness." Again, "The matter of this erysipelas is already described to be a thin choleric humour; the fountain of it is the blood, which, by its capillary arteries, dischargeth it upon that part" (the skin) "as often as in

bilious or malignant and pestilential fevers it is provoked thereunto," &c.*

Desault, among the moderns, has inclined somewhat towards the opinion, that at least one form of erysipelas was occasioned by, or associated with, the circulation of bile in the capillaries of the skin. His division of the disease is into phlegmonous, *bilious*, and local. This old doctrine of the congestion of the bile has also been in some measure revived by Tissot, who attributes the cause of erysipelas to a bilious humor diffused through the mass of the blood.†

"The erysipelas," says an old English surgeon, "is often attended with violent itching, the consequence of miliary particles obstructing the cutaneous vessels, and always with some degree of fever. How far," he continues, "the redundancy of bile in the blood during this state of inflammation in any part may confine it to the skin, I will not attempt to determine; but, though the blood, on pressure with the fingers in the inflamed part, immediately retires, and soon returns, nevertheless in that short interval we observe the skin has not its usual whiteness, and the vesicles that attend this disease are always full of a *yellowish serum*."‡

Sauvages considers Sennertus a sufficiently good authority upon the strictly humoral nature of the disease to quote his opinion when classifying the varieties of erysipelas. "Erysipelas juxta Sennertum definitur febris continua a sanguinis parte *tensiore corrupta* et inflammata, quâ ut natura se liberet, eam in aliquam partem externam expellit formâ tumoris aut maculæ rubræ, latæ, quæ à parte in partem serpit."§

Thus we observe from how early a period in the history of medicine, and how continuously down to the present day, the disease which we are considering has been attributed by physicians to the circulation of some impure material in the blood;|| and that this is the origo mali in every form which erysipelas observes (the morbid material or poison being the true agent in the causation of

* Wiseman's *Chirurgical Treatises*, vol. i. p. 55-7.

† Tissot, *Avant un Peuple*.

‡ Bromfield's *Chirurgical Observations and Cases*.

§ Sauvages, p. 446.

|| Van Helmont had a whimsical notion that erysipelas was "apostema maxime igneum, in quo ascendunt spiritus vitales et canit mercurius lacessitus, et valuti in iram provocantur."

* John Hunter observed that the serum of the blood undergoes this change of colour where rhubarb has been taken for any time (vol. iii. p. 63).

† Van Swieten's *Commentaries*, vol. iii. p. 330-1.

its peculiarities) I think so self-evident, that I would lay it down as a canon in our creed.

It is true we are as far at the present day from resolving the real nature of this poison as was Galen himself, who, however, as I have shown, partook of none of our modern doubts and diffidence upon the point. It is to be feared, however, that the present is one of those many instances in which, to use a significant remark of Voltaire's, we are not likely to be permitted "to peep into the bottom of nature's basket." A French writer has well observed (speaking of this disease), "On a beaucoup disserté sur la cause *prochaine* de cette maladie; mais on n'a guère réussi qu'à nous donner des idées vagues ou erronnées, plus propres à embrouiller la matière qu'à l'éclaircir, comme il arrive toujours lorsqu'on veut expliquer ce qui est *inexplicable*."*

[To be continued.]

THE HARDSHIPS OF YOUNG PHYSICIANS.

It has become a proverb, not only in the profession but out of it, that it requires from five to ten years' starvation before the young physician can support himself upon his collected receipts. He may possess all the qualifications requisite to constitute a good physician; he may have rendered excellent service in cases where he has attended; but he cannot be appreciated and patronised, for he is a young man, *inexperienced* in the mysteries of a sick chamber, —and therefore cannot cure disease. How is experience in such a case to be obtained? for the people upon whom he relies for support will not give it until he has had a certain *number of years' practice*. This practice, if obtained at all, is of that peculiar kind which may perhaps benefit his patient, but "enriches not him." He waits upon the poor, whose compensative means are very limited, although at times he receives from them that which is priceless — a heartfelt gratitude and the invoking of heaven's blessings. The better or richer class seldom employ the young physician unless compelled to by accidental circumstances. Their creed is: if we *pay*, we will have what is considered best, —though it is allowable that they are often incompetent to decide upon the question of qualifications. A physician who can repeat every word he has read on medical subjects, and

be able to tell you the number of fibrils in any given muscle, may yet be unable to distinguish one disease from another, or decide upon a proper course of treatment. This same class of patients, who never employ the young physician because they have no confidence in him, do not hesitate, in numberless instances, to purchase secret nostrums, or even to employ the parties who prepare them. If any new *pathy* should be introduced among the people, it only requires its founder to demonstrate its wonderful principles, and promise *impossibilities*, in order to succeed in obtaining advocates and plenty of patronage. The self-vaunted quack gets a large harvest, and that, too, from only a six months' or year's practice. Every one has confidence in him, to be sure, for he *promises* so much, and can tell exactly what *ails* a person at the first glance. Perhaps this wonderful doctor has often to go miles to see a patient; or, may be, his fame is such, that the halt, the lame, and blind come from afar to him to be cured.

In the practice of medicine it would appear, then, that it requires a certain amount of egotism to succeed well. It is one of the weaknesses of our nature to believe in impossibilities; and the more our credulity is imposed upon, the more believing and satisfied are we. Now what encouragement can there possibly be for the young educated physician, fresh from his college, although his brow may be covered with laurels of distinction? He has only to look around him to see that it is not always true merit that makes the practitioner! There is Dr. A., who has been in practice some twelve or fifteen years, but cannot with his earnings pay his office dues. Drs. B. and C. are as badly off; while Drs. D. and E., having been in practice about the same time, are taking the better part of the business. The three first-mentioned doctors are known to be men of science and of rare attainments, —even the public themselves are willing to allow that; while the other medical gentlemen are not so well informed, nor in any respect so well qualified for their posts. It does not require much perception, likewise, for him to see that the arrant quack is not only on a par with the educated physician, but in many cases the palm is given to him. It is true, the success of this class is not permanent, as they stay only long enough in one place to fleece the pockets of their victims there. It would be well if the matter ended here; but it does not, —for as soon as one class of quacks lose caste, there are others, more *pretending*, ready to spring up. —*Boston Medical Journal*.

* *Dictionnaire des Sciences Médicales* — *Encyclopédie*, vol. xxi. p. 264.

MEDICAL GAZETTE.

FRIDAY, SEPTEMBER 20, 1850.

It is with regret we perceive, from the Third Report of the Convention of Poor-law Medical Officers, that the disinterested efforts of the Committee have not received that co-operation from the Poor-law Staff which they had a right to expect, and which the medical officers of Unions were equitably bound to afford. It seems that in order to improve the position of Union surgeons, and to obtain redress for the grievances under which they are well known to labour, the Committee endeavoured to establish a Poor-law Medical League, with Branch Associations in the principal towns and cities of England and Wales. A circular letter was therefore addressed to each Poor-law medical officer, in order that the opinions of all might be ascertained. Letters were thus sent to 8000 persons, and the number of answers received amounted only to 263,—a number comprising about one-twelfth of the whole number of Union officers! Upon examining the answers, it appeared that there were in favour of the proposed Association, 178; doubtful as to the usefulness or probable success of the plan, 36; refusals to co-operate, or to contribute one shilling monthly, 46. The idea of forming such an Association was therefore abandoned.

An interview with Mr. Baines, the President of the Poor-law Board, led to the usual result of official interviews—i. e. the President had neither the disposition nor intention to interfere for the purpose of procuring a better scale of remuneration with permanency of office. He candidly admitted that the question of finance was the great difficulty. "Throughout the country

the demands upon the rates had lately been increasing. He had no reason to believe that any mode of administering medical relief, except as now through the Boards of Guardians, would be acceptable to the country in general. The plan of placing the entire cost on the Consolidated Fund was one which Sir George Grey *might* deal with."

Foiled in this endeavour to enlist the efficient aid of the President, the Committee addressed themselves to the Colleges of Physicians and Surgeons, as well as to the Apothecaries' Society, in the hope that, as medical corporations, they would be induced to memorialize the Poor-law Board in favour of the three leading points of an amended system of medical relief—namely, *permanency of office, responsibility to medical authority, and payment proportionate to duties performed*. Here, again, the Committee were unsuccessful. The Colleges of Physicians and Surgeons declined any further interference in the matter, and the Society of Apothecaries allowed the subject to drop *sub silentio*.

On this the Committee remark:—

"The Poor-law medical officers will not fail to observe that no disposition is shown by any of the existing medical corporations *strenuously* to labour for the amendment of a system admitted by themselves to be oppressive to at least 8000 of their professional brethren in office, and indirectly to every member of the whole profession.*

The subject of remuneration for extra services during the prevalence of malignant cholera has been on several occasions noticed in our columns. It is creditable to the Committee that they devoted all their energies to procure this act of justice for their professional brethren. It is well known, however, that the divided jurisdictions of the

* The apathy of the Medical Corporations cannot be considered deserving of grave censure, when it appears from the Report that out of 8000 Poor-law Medical Officers, 2,500 have shown no disposition to labour in any way for the amendment of an oppressive system!

Board of Health, the Poor-law Board, and the Rural Boards of Guardians, practically defeated the good intentions of the Legislature on this subject. The following extract from the Report gives a short summary of the results attending this application:—

“It appears that the Poor-law Board had power to regulate the *salaries* of medical officers, and also to enforce the performance of such duties on their part, as the guardians, under recent authority of the Health Board, were obliged to see carried out. At the same time the guardians *would not*, and the Board of Health *could not*, award additional recompense for such additional labours, which were evidently beyond the letter, and even the spirit, of the medical officers' contract. It was obvious that the Poor-law Board rather shunned grappling with the question, as it would place them too often in antagonism with the Boards of Guardians. On the other side, should the management of the medical attendance on the sick poor be transferred to the General Board of Health, there was no evidence that the union medical officers would be benefited.”

The answer of the President of the Poor-law Board was, as usual, marked by official politeness, but left the question of increased remuneration in the hands of those patterns of economy, the Boards of Guardians:—

“He admitted that he had the requisite power which on many occasions had been exercised favourably toward the medical officers; but with respect to cholera, and other epidemics, the Poor-law Board had not the power to lay down any scale of remuneration in anticipation of extraordinary services, but the power of granting reasonable remuneration for the same was vested in the guardians; and that, *whenever* an increase of salary or payment was proposed by them, the Poor-law Board had always much pleasure in confirming the same.”

From another paragraph in the Report, it would also appear that when a *diminution* “of salary or payment was proposed by the guardians, the

Poor-law Board had also much pleasure in confirming the same.”

“After the early and repeated assurances of sympathy, and of a desire to ameliorate the status of the Union surgeons made by the Poor-law Board, the Committee were not prepared to receive an account of the treatment exercised by that Board toward the surgeons of the Holborn Union. In this case, not only has a reduction of salary to so low an amount as eightpence per case been *sanctioned*, but it was actually met by a suggestion from the Poor-law Board for even a *further* diminution of the payment, and an *augmentation* of the work of the medical officers.”

This mode of dealing with the Union medical officers is neither candid nor fair. It clearly shows that, while the reasonable demands of the Committee are met by official promises and formal intimations of sympathy, it is the plain and obvious intention of the Poor-law Board to co-operate with the guardians in all financial matters.

Under the present system, it is well known that the screw has been applied to its maximum extent. The order of the day appears to be, increased work and diminished payments. In illustration of the oppressive and impracticable nature of the duties demanded of the medical officers of the Stepney Union, it is stated in the Report that one of them was actually summoned to attend *six* cases of midwifery requiring *immediate* assistance in distant parts of his district in one day! Rural Boards of Guardians have treated with contempt the consolidated order requiring payment for extras, and have handed over the sick poor to any half-starved adventurer who happened to hold a diploma, and who had no chance of succeeding in any more lucrative branch of practice.

It is a great and glaring fact, and one deserving of the severest reprehension, that while Lord Ebrington, a nobleman with comparative independence, receives

as a Secretary of the Poor-law Board a salary of £1,500 per annum for giving up only a moderate portion of his time to the duties of his office, the Union officers are paid for medicines and medical attendance on each sick pauper a sum which is *less than half the cost* of drugs alone in the ordinary treatment of a hospital patient. How can it be explained that, while the salaries of Union surgeons are thus cut down, a noble secretary of the Board receives, for performing not a hundredth part of the labour, an amount equal to the united salaries of about forty medical officers!

The Committee append the following conclusions to their Report:—

"1st. The *disjointed efforts* made by them with much zeal during the last two years, if continued, cannot do more than keep up a teasing agitation, wasting the time and paining the feelings of the few gentlemen who regularly devote themselves to the work.

"2nd. That there is no reason to think that the Poor-law Board will ever take steps to remedy the hardships of the union surgeons, and to correct the misrule under which they serve and the sick poor suffer, *until they can be forced to such a measure*. A cessation of the present undue professional competition must take place; a loud voice, with a resolute purpose, must be raised in the Commons, and well supported on the hustings in every borough and county. It is not too much to state that the medical profession should be equal, by their numbers as well as by their rightful influence, to accomplish what is equally due to themselves, the sick poor, and the rate-payers.

"3rd. That the attempts now made by the Poor-law Board to engage medical officers without private practice for the service of the poor, could not, without much difficulty, be successfully applied to rural districts. The plan of engaging a class of gentlemen for exclusive attendance on the poor is of such very questionable advantage, that the Poor-law Relief Committee, reporting to the House of Commons in August 1838, said 'They felt it to be most important that the poor should be perfectly satisfied with their medical attendants; and with this view it appears to be desirable, as,

indeed, is almost always the case, that the *care of the poor should be confided to the same person who is in the habit of visiting their richer neighbours*.' Such an arrangement as that attempted to be carried out by the Poor-law Board will only work well for the public, or for the profession, when the duties of union surgeons shall be blended with those of Officers of Health, under the control of the General Board of Health, or some other authority independent of the Guardians and Poor-law Board, and adequately paid for by the State.

"4th. That as the public are more convinced than formerly of the false position in which the union surgeon is placed, they will be satisfied that it is their duty to provide *adequate remuneration* and better regulations for the prevention as well as for the cure of disease among the poor. Such provisions are due from the State on the score of justice, humanity, and sound economy.

"THOMAS HODGKIN, M.D., Chairman.
"CHARLES F. J. LORD, Hon. Sec."

There are three serious obstacles with which, it appears to us, the Committee have to contend:—

1. The want of co-operation on the part of the great majority of Union medical officers; 2. The indifference of Government Boards; and 3. The "Guardian" interest in Parliament.

With regard to the first,—it ought not to exist. It is admitted by all that the remuneration of Union officers is insufficient, and the labour excessive. These are assuredly evils which require removal. Is there any Union Officer so selfish that he would profit by the results of efforts, if successful, while he will lend no helping hand to the disinterested few who are now exerting themselves for his benefit? The Report leads us to infer that a large majority are disposed to work on in their present abject condition. If so, there can be no hope of a change for the better. With respect to the two remaining obstacles, we think they would soon cease to exist if the Union surgeons cordially united for the protection of their own interests. Boards of Guardians find a ready justi-

fication for their harsh dealings with their medical officers in the existence of a most degrading and unprofessional system of competition, in which honour is sacrificed for the sake of a paltry salary.

Reviews.

Gout: its Causes, Cure, and Prevention, by an original and most successful treatment, founded on the organic changes in the human solids, and on the functions of the skin; without the use of Colchicum. By ABRAHAM TOULMIN, M.D., &c. 12mo. pp. 112. London: Highley. 1850.

FROM the nature of the above title-page we are led to suppose that this work is addressed rather to the non-professional than to the professional reader. A glance at its contents gives strength to the suspicion. The first three chapters consist of a popular sketch of the chemistry and physiology of food and digestion, assimilation, &c. The two following chapters are on the causes of gout. The next on the functions of the skin. This is succeeded by an exposition of the author's original mode of treatment, which he terms *didermic*. The theory on which the author bases his treatment is, that the blood is to be depurated by increasing the functions of the skin. The practice founded thereon is the use of the *Thermaclin*, or air-tight hot-air bed, to promote profuse diaphoresis. This method the author claims the merit of having introduced into this country, being, it seems, unacquainted with the simple apparatus devised by Dr Copland, in 1832, for the use of cholera patients; and which we have ourselves, since that period, frequently made use of in treating chronic rheumatism and rheumatic gout.

Mr. Toulmin concludes with some common-sense propositions on the prevention of gout; and gives a series of cases in illustration of the benefit of his *thermaclin*, of which, however, we find no particular description, and we are therefore left in doubt whether a special apparatus be indicated, or the

term be employed generally to signify hot air baths or beds.

Ship Fever, so-called: its History, Nature, and best Treatment. By HENRY GRAFTON CLARK, M.D. Pamphlet, 8vo. pp. 48. Boston. 1850.

THE ship fever, as seen on board the emigrant ships, in the quarantine hospitals, and among emigrants newly arrived from England and Ireland, in the ports of the New World, is said by Dr. Clark to be clearly different from anything that has been before known in America. "It is the opinion of the writer that, under the various titles of jail, hospital or camp fever, putrid continual fever, maculated typhus, and English or Irish typhus, is expressed the identical disease known recently and popularly under the name of SHIP FEVER."

The author, in pointing out the differences between typhus and typhoid fever, states that the distinction has not been made in England; but, if not before, it has recently been accurately made by Dr. Jenner in his papers on typhoid fever.

The characters of the disease are briefly and clearly depicted, and the pathological appearances are accurately described. The treatment recorded is judicious.

CAUTION IN THE USE OF ETHER AND CHLOROFORM.

A VERY serious accident happened in surgical practice in Boston, U.S., not long since, by the taking fire of the vapour of the ether which was used to produce insensibility. The circumstances under which the accident occurred are these:—The patient had an operation performed upon the face, near the mouth. There being some considerable hemorrhage, the actual cautery was applied; and it was by contact with the heated iron that the ether vapour was set on fire. Too much caution cannot be used in the application of ether; and this one instance of its inflammability in actual practice should serve as a caution to all. No lighted body should be placed near a patient, while he is under the influence of ether. There never should be a lighted lamp brought into a close or small room while etherization is going on.—*Boston Medical Journal*

*Medical Trials and Inquests.*SUPREME JUDICIAL COURT,
BOSTON.

March 19, 1850.

REPORT OF THE TRIAL OF DR. WEBSTER
FOR THE MURDER OF DR. PARKMAN.*Before Chief Justice Shaw and Justices
Wilde, Dewey, and Metcalf.*

[Concluded from p. 473.]

DR. JEFFRIES WYMAN, sworn. — I am professor of anatomy in Harvard University; have been a teacher of anatomy for the last eight years. First went to the Medical College on Sunday, the 2d of December. I had given to me the charge of the bones found in the furnace; made a catalogue of them. These bones in this box are the bones. I was not called specially to the other parts of the remains. [The witness explained a drawing of a skeleton, in which the bones found were marked in yellow, represented by the shaded portions of the subjoined engraving.] There were no marks of the



body being a subject for dissection: it struck me that the sternum was taken out as it would have been by a physician at

an ordinary post-mortem examination. I was also struck with the separation of the sternum from the clavicles, or collar-bones, and first rib; the route for the knife to pass through is so difficult, that a person having no knowledge of the structure of the parts would not have been likely to direct it in that way. I did not feel myself called upon to examine critically. I should consider the person past the middle age of life. On examining the thorax and turning it over, I was struck with the quantity of hair on the back; I never saw a person before with so much. If a person was killed by a blow, and then stabbed immediately, there would have been a flow of blood according to the vessel wounded, and the depth of the stab. A post-mortem examination may be made without an effusion of blood, if made with care. I made an examination of certain spots on the side of the staircase, near the lower landing; they were not blood but tobacco-stains. This has no reference to the spots found higher up. Saw spots, which I was told by a chemist were nitrate of copper; did not examine them. I performed some experiments on the effect of nitrate of copper on blood. Blood is known by red flattened globules, or discs. I placed some blood under the microscope, and applied nitrate of copper in solution. The action was not immediate; after a few hours it took place, and the blood discs disappeared. I should say that nitrate of copper would destroy all characteristics by which blood could be detected by the microscope. There were no other spots in the building on which were any marks of blood, or what was recognised as such. [Witness was shown some slippers, on which were spots supposed to be blood.] They are the same from which I cut portions having similar spots, which I examined, and found what I considered to be blood. [Pantaloons were shown, marked with the name of Dr. Webster, which the witness identified as the ones from which he cut pieces.] It was the right slipper that was bloody, and the left leg of the pantaloons, on the bottom, on the outside. The slipper had also what looked like Venetian red on the sole. The pantaloons had another spot, which might be Venetian red, but which seemed to be rather a mark of acid. The blood did not seem to have fallen from any height, for it had not trickled down. There was a piece of paper found in the laboratory, with two spots of blood. [The witness explained the classification and arrangement of the bones, referring them to the different parts of the body.] The first bone belonged to the front of the forehead. [Witness explained how he knew it to belong there.]

The next was a piece of the temporal bone, known by the canals that pass through it. The next was behind the ear. [The witness proceeded to demonstrate the places of the bones, and succeeded in putting together three fragments, which form the greater part of the right-half of the lower jaw.]

Attorney-General.—Please state to the Jury whether there is anything remarkable in the correspondence of this jaw with Dr. Keep's model.

Witness.—I made a drawing of the jaw before I saw the model or knew of the condition of Dr Parkman's teeth. There was an absence of the teeth from the coronoid process to the first molar, or bicuspid; I was not absolutely certain which, but my impression was that there were three teeth gone up to the bicuspid. I made a drawing in a pencil of a bicuspid, as a probability. On comparing the fragments of bones with the model, I found that Dr. Parkman had lost these teeth. They seem to correspond in this respect; as the model was taken from the mouth when the gum covered the bone, we cannot expect an exact correspondence. If the curves of this jaw had been carried out, as we should expect, by analogy, it would have given a rising chin. The portion of the left lower jaw had a remarkable depression; have not witnessed the same in any other jaw, having examined a hundred and fifteen or twenty of them, perhaps with particular attention to the jaws. There were several vertebrae, a joint of an elbow, some bones of the wrist, some joints of the fingers, part of the bones of the right leg known unmistakably to be such by their character; there were not duplicates of any bone, nor were there any bones except of the missing parts not found in the vault or tea-chest. There were some fractures, which had the appearance of having been made before calcining. Before calcining, the animal matter adheres; there is a greater disposition to splinter, and to angular forms rather than to crumble. [The witness showed the jury a fracture of a skull-bone, which had the appearance of being fractured before being calcined. He did not think the sign was absolute.]

By the Court.—Does your distinction refer to the fractures made before and after death?

Witness.—Before and after calcination only.

Cross-examination.—In a half-calcined bone there is less disposition to crumble. Thinks nitrate of copper effectual to remove blood, but not so good as water, unless the wood were to be destroyed. Should think muriatic acid a better solvent of blood, as it is of animal tissue. *Physiologist.*—*then nitric.* *that the*

blood is about the fifth part of the weight of the body; thus would be 28lbs. for a body of one hundred and forty pounds. This is by no means a precise statement; but twenty-five pounds have been actually obtained, and probably not all was obtained. There was a thorough examination of the laboratory for blood. I superintended the taking up of the brick floor, which was laid in sand. Found no traces of blood. Took chips from the floor to examine; found no blood on them. Should think the cutting to pieces indicated that the person doing it had some knowledge of anatomy, and knew where the joints were to be found. If blood had fallen from the height of three feet, it would have given an oblong mark, falling on a vertical substance; could not tell from the blood-marks, how long it had been on. After a day or two, blood-marks assume a brown colour; after that, they do not change for years. The microscope distinguishes human blood from the blood of the lower animals, but not from that of the higher, as the ox.

Dr. OLIVER W. HOLMES, sworn.—I am Parkman Professor of Anatomy and Physiology in the Medical School. The professorship is named after Dr. George Parkman. The dedication of the Medical College must have been on the first Wednesday of November. Noticed Dr. Parkman's teeth on that occasion, as new; they were white and long. Dr. Webster lectures to the Medical Class four times a week, on the subject of Chemistry. His lecture-room, laboratory, and small room in the rear, form an establishment entirely distinct from those of the other Professors. He never had any need of anatomical subjects. The remains indicated that the person who separated them knew where to cut; there is no botching about the business. Noticed a discolouration, which seemed to be the effect of heat; was told, and readily believed, that it was the effect of a caustic substance. Was familiar with the form of Dr. Parkman; did not see any particular similarity between the parts and Dr. Parkman, or anything dissimilar. A stab between the sixth and seventh ribs would not necessarily reach the heart. If it did, there might be more or less effusion of blood externally, according to circumstances. If the wounds externally and internally corresponded, there might be external effusion: if the external wound slipped over the internal, the effusion would be chiefly internal. My lecture-room is over that of Dr. Webster; have never heard noises from his room. The students in my room sit on rising seats; I stand on the floor.

Cross-examination.—Could not know what the effusion of blood would be, without knowing the direction of the knife; if the

knife touched the heart, much blood would be inside, but not probably all. Have heard the noise of students below, when in the demonstrator's room, which is at the head of the stairs. Cannot say that the hair had been singed by fire. Am not so familiar with the effects of caustics as to say what effect could be produced by them; but was prepared to believe that all I did see had been so produced. A mortal blow might be struck on the head, and no effusion of blood take place.

[The defence of the prisoner chiefly rested upon an *alibi*; it being alleged that the deceased had been seen by credible witnesses alive and well some hours after the time at which it was stated he must have been murdered. It was further contended that the remains were not identified as those of Dr. Parkman, and that the dental evidence was not trustworthy. Admitting, however, that the jury were satisfied on the question of identity, it was suggested that the real cause of death had not been proved, and that, at all events, there was no evidence of a premeditated design to kill on the part of Dr. Webster; that he and the deceased might have quarrelled concerning the debt, and that in the heat of passion the prisoner might have struck him a fatal blow. The concealment and disposal of the remains might be ascribed to the want of presence of mind on the part of the prisoner how to act in such an emergency. We do not publish the arguments thus ingeniously urged at considerable length, because since his conviction the prisoner has acknowledged that the remains were those of Dr. Parkman, and that he fell by his hand. Even without this confession the arguments were founded on such improbable assumptions, that they only served to indicate the impossibility of giving a clear and satisfactory answer to the evidence for the prosecution.

The prisoner himself addressed the jury, by permission of the Court, before the judge summed up. His address did not touch the terrible charge against him: he did not venture to deny the crime, but merely offered a few remarks explanatory of trivial and unimportant points in the circumstantial evidence.

Some of the medical gentlemen who had given evidence for the prosecution were called for the defence, but their testimony did not in the least alter the force of the evidence in support of the charge. In the closing argument of the Attorney-General there occurs a passage for which we must find room. It involves a high compliment to the profession on scientific evidence, and on the unselfish character of medical researches when required for the purposes of public justice. In speaking of the proofs

of guilt, we find the learned gentleman saying—]

If anything more were needed, it is found in the conformity of the jaw of Dr. Parkman to the mould which Dr. Keep had; which mould corresponded with all the peculiarities of the jaw of Dr. Parkman, picked out from the smouldering ashes, and, by that true lover of science and uncompromising seeker of the truth, Dr. Wyman; put together, and produced here before us. If he had produced here Dr. Parkman's right hand, with a scar upon it which every one of his friends had known, the evidence of identity could not have been more conclusive. When we consider that here is a man in this culprit's dock, with such advantages of education and of culture as he has enjoyed, who is himself a devotee of science,—and we feel that he has so debased and betrayed his high vocation and mission as to have slain, either in anger or in cold blood, whichever it may be, his fellow-man, and his benefactor and friend,—it almost sickens us; we feel that there is no shield for any of us against the commission of great crimes; that culture, science, and all the ennobling and purifying influences of education, are utterly lost upon us. To find them subjected to such base uses as that chemist's laboratory has witnessed, prompts us to exclaim, with the poet,

"Oh, star-eyed science! hast thou wandered there
To waft us back the tidings of despair?"

But we recover and are refreshed only when we go to the other fact which this case discloses, that, although science had been debased to the purpose of destroying those remains, yet science, in its true vocation, in its nobler scope, sifts and penetrates those smouldering ashes, and evokes from them those materials with which it reconstructs almost the entire body which science had vainly attempted to destroy. This gives to us a renewed assurance of respect for science! And I cannot pass from this part of the case without expressing a feeling which has been often in my mind during the solemnities of this trial—the honour that is due to that noble profession through whose ministers this assurance has come to us. When we have welcomed them to our bedside, amid our trials and sufferings, we have loved and honoured them; but when we meet them here, and see them taking the stand, as they do, most reluctantly, against one of their own brotherhood,—forgetting, or rather trampling under foot, all those considerations which arise from caste, from class, and giving themselves unreservedly to the truth, let it strike where it may, let it fall where it will,—they challenge and are wor-

thy of the highest honour; and they have my humble reverence. One of their number, whom we looked to have been here, and whose aid, in another recent capital trial, I had occasion to seek—in which his testimony showed how much he would have added to the impressiveness of this—has passed away from us since these investigations commenced,—a man who honoured the community in which he lived, who honoured the profession to which he belonged, and who, for the cause of science, has been removed from us too soon,—I refer to the late Dr. Martin Gay, whose testimony to that comes down in yonder prison, and over at that Medical College, would have been as valuable to us as would his scientific testimony upon the question of the identification of the remains.

[The only part of the summing up by Chief Justice Shaw which requires notice, is that which refers to the proof of identity from an examination of the jaw and teeth.]

It is scarcely necessary for me (observed the learned judge) to do more than name the witnesses which have been called to testify upon that subject. It is certainly a very interesting inquiry, whether the teeth can be identified or not. It cannot have escaped notice, how great a similarity there is in this to the investigation of what are called fossil remains. Persons have studied the anatomy of the bodies of reptiles, and of the lower orders, to such a minute degree, that from the figure, from the openings, they are able to say, from a single bone even, what class they belong to, and thus trace the inquiry, and ascertain the existence of races and species of animals. But still you are told here by the anatomists,—by Dr. Jeffries Wyman, who is reputed to be excellent,—that by finding a small piece of bone, it is possible to determine to what part of the body that belonged. There are particular parts through which particular nerves or vessels pass, by which it can be determined that they are parts of the temporal bone, the cheek bone, or some other. Dr. Keep was called, and stated that, three years previously, in 1846, he made teeth for Dr. Parkman, to whose teeth and stumps various adjustments were to be made. He testifies that they were adjusted and fitted. Now, the gold having been melted, but the blocks of teeth remaining, with several peculiar angles and points, the question was, whether he could ascertain their identity. It is merely necessary for me to refer you to his testimony. He was of opinion that he could identify them; he was satisfied that they were the teeth of Dr. Parkman. If you are satisfied that that conclusion was right, then this testimony is of a very different character from that of the shape

and size, and has a strong tendency to prove that it was the body of the deceased person. I barely refer to the persons who have testified to this. Dr. Keep, and his assistant, Dr. Noble, think that they can identify these blocks of teeth. Dr. Morton is of opinion that there is not enough to enable an artist to identify them. And with regard to all that, Drs. Harwood, Codman, and Tucker, have testified the other way. You are to determine, by all the testimony, whether those were the teeth of Dr. Parkman, and belonged to the same body as the other parts; and, if so, it has a strong tendency to a proof of death by violence, and then the *corpus delicti* is established; otherwise, not. If this is not proved to the satisfaction of the jury, beyond reasonable doubt, then the dead body is not proved to be that of Dr. Parkman. . . .

If this act of homicide was committed by Professor Webster, and there is not sufficient proof to mitigate the crime to manslaughter, then the conclusion would be that it was murder by implied malice. If the other assertion is proved, that it was intended to decoy him to the College to do this deed, that is express malice. If it is not proved that he was there, then there must be a general verdict of acquittal.

[The trial occupied twelve days. The prisoner was unanimously found GUILTY; and he was executed at Boston, as we have already announced, on the 30th ult. Subsequently to his conviction, he confessed that he had killed Dr. Parkman under circumstances of great provocation. An appeal was made to the Committee on Pardons for a commutation of punishment: they made the subjoined report:—]

"The Committee respectfully report that they cannot, consistently with what they conceive their duty, recommend a commutation of the sentence in the case of John W. Webster, as prayed for in his petition. Nothing now remains for the Committee, in the discharge of this painful duty, but to advise your Excellency in determining upon a time for the Execution; and they name *Friday, the thirtieth day of August next*, as the day; and recommend to your Excellency to decide upon that day as the time for the execution of John W. Webster.

"JOHN REED, Chairman.

"Council Chamber, July 19, 1850."

THE CHOLERA AT MALTA.

LETTERS from Malta of the 8th inst. state that the cholera, which had considerably diminished, had for a day or two been on the increase. Its reappearance was attributed to a whole day of rain, followed by a north-west wind, which occurred last week.

Correspondence.

ON THE SUPPOSED VALUE OF ALCOHOLIC LIQUORS IN MAINTAINING THE HEAT OF THE HUMAN BODY UNDER EXPOSURE TO SEVERE COLD. BY WILLIAM B. CARPENTER, M.D., F.R.S.

I HAVE recently perceived with much regret, that the value of the evidence collected by myself and others, in disproof of the popular opinion which attributes to alcoholic liquors a power of assisting the human system to sustain extremes of cold, has been, in my apprehension, very much underrated; and that the medical profession is less well-informed upon this point, than might have been anticipated from its scientific as well as practical interest. In the review of my "Prize Essay on Alcoholic Liquors," in the *MEDICAL GAZETTE* (p. 203 of the present volume), the following passage occurs:—"Dr. Carpenter argues, from the facts of organic chemistry, that alcohol cannot either enable the human body to endure cold, or contribute to the maintenance of its heat; but 'universal experience' refutes the speculation; and we may observe, that experiments which Liebig performed directly with reference to these very points led him to the very opposite conclusions." It is a little singular that another reviewer of my essay (*Dublin Quarterly Journal* for August, p. 164) should have given a precisely opposite statement of my opinions:—"We have not time to discuss the power of alcohol in enabling the body to resist the depressing influences of cold and heat. This we the less regret, as Dr. Carpenter observes that its power in the former of these cases 'is, perhaps, the best established of all its attributes.'" This reviewer would not seem to have read beyond the first sentence of a section, in which I proceed to demolish this very proposition.

I beg that I may not be understood as now bringing forward this subject with the purpose of complaining of these or of any other of the reviews of my Essay. I was well aware that it would run counter, in many points, to the ideas currently entertained in the profession, and that the production of it would not advance my own scientific reputation—in fact, would probably diminish it: and nothing but the desire to do something, however little, towards the reduction of that great mass of individual and social evil which is clearly traceable to the abuse of intoxicating liquors, leads me now to come forward, for the purpose of again stating, briefly but

explicitly, what are the opinions upon this subject which my own inquiries have led me to embrace, with the nature of the evidence upon which they are founded, and to place before the profession some additional evidence which has come before me since my essay was published.

The question which I had to answer was, what *science* and *experience* teach in regard to the heat-producing powers of alcohol. Now, at the first view, those who adopt (as I do) the chemical theory of animal heat as being true in the main, though not accounting for every phenomenon, would naturally look to alcohol as one of the most potent of all *fuels*. As oleaginous substances produce more heat in combustion than saccharine, so does alcohol (in virtue of its larger proportion of hydrogen) produce more heat than oil or fat. But the experiments of Dr. Prout, and of Vierordt, have shown that the amount of *carbonic acid* exhaled after taking alcoholic liquids is *considerably diminished*; and hence it appears that, whilst alcohol is in the blood, the oxygenation of other combustible materials is retarded (an inference confirmed by the fact ascertained by Bouchardat, as to the venous character of the blood when alcohol has been introduced into the system in excess); so that, notwithstanding the larger proportion of *water* which may be formed by the combusive process, it is doubtful whether more heat is generated by the elimination of alcohol through the respiration, than by the combustion of oleaginous matters. Considered simply as a fuel, then, it has not the advantages it would at first sight appear to possess: but I expressly point out that it has a special value in certain cases, in which there is a deficiency of other combustible material in the blood, and in which it is burnt off so rapidly that it has not time to exert its stimulating property.

Now it is because alcohol is not (like oleaginous matters) a *mere* fuel, but because it exerts a stimulating action on the system, that its effect is considerably different from that of other combusive materials. The pleasant glow which it temporarily produces, seems due, in part, to the increase in the rate and force of the circulation, and partly (it may be) to its stimulating influence on the nervous system. In this way the depressing influence of cold may be effectually resisted *for a time* with its assistance, but *only* for a time; for, as sure as there is stimulation, there is subsequent depression; and, if the exposure to cold be prolonged, its severity will be more felt, and it will be more injurious during that period of depression, than if the alcohol had not been taken.

This I affirm to be the teaching of

science upon the question. It is stated that in so doing I am placing myself in opposition to the dictum of Professor Liebig, who seems to put an unlimited faith in the heat-producing powers of alcohol,—looking at it only in its chemical relations, and ignoring altogether its physiological action. But I am yet to learn what are the experiments on the strength of which it is so confidently asserted that he has been led to opposite conclusions; and, until I shall have been made acquainted with them, I venture to think that the opinion of a physiologist upon a physiological question may be set against that of a chemist, however eminent.

I would not for a moment affirm, however, that any such *a priori* argument should furnish our rule of practice. If “experience” unquestionably led to a different conclusion, I would at once say that our science was imperfect, and must be reconstructed. What science can be good for anything, that is inconsistent with experience? But I find experience here in the fullest harmony with scientific prediction. I cite in my essay the testimony of three distinguished medical officers—Sir John Richardson, Dr. Richard King, and Dr. Joseph D. Hooker, who have been attached to Arctic and Antarctic expeditions. This testimony was kindly given to me in reply to inquiries which I put to these gentlemen. I did not select them as previously knowing their opinions, but because I knew them to be men far above the average in intelligence, as well as to have had special experience on this particular question; and because I happened to have an amount of personal acquaintance with them, which justified me in going to them for information. The statements which I have given on their authority are not selected pieces of evidence, but are all I could get: and they are all most explicit to the same effect—that, whatever may be the temporary warming effect of spirituous liquors, the use of them diminishes the power of resisting cold. I should like to know who has a better claim to be heard on such a question than Sir John Richardson, the companion of Franklin in that disastrous overland journey which put the endurance of every individual to the severest test. Or what experience can be set against that of the servants of the Hudson’s Bay Company, who have for some years past entirely disused spirits, to the great improvement, as Sir John Richardson informs me, of their health and morals?

Surely those who speak so confidently of the refutation of my “speculation” by “universal experience” are bound to get up some sort of case to meet evidence of this kind, and should not be permitted to

cast it on one side as unworthy even of notice. Several other facts, which I have ascertained by personal inquiry, are contained in my Essay;* but rather than cite these, I shall now adduce a body of additional evidence, which will, I think, lead those who will peruse it to question whether the usual notions are quite as well founded as they are accustomed to consider them.

Happening last Easter to receive a visit from Sir John Richardson, I inquired from him how far the experience of his last expedition (undertaken two years ago in search of Sir John Franklin) corresponded with the statement he had given me before he proceeded upon it; and, in reply, he assured me that it had most fully confirmed him in his adhesion to the Total Abstinence system as the best for the endurance of severe and continued cold, the whole party having sustained the full severity of the Arctic winter in a manner in which he was confident (from his former experience) they could not have done, if even a moderate allowance of spirits had been employed. He mentioned, as a proof of his own power of resisting cold, notwithstanding his advancing years, that he was accustomed every evening, during the residence of the party at winter quarters, to cross from the dwelling-house to the observatory at a short distance—going out from a room of the temperature of about 50° into an atmosphere of —50° (that is, enduring a change of a hundred degrees) without even putting on his great coat.

In the narrative of “An Arctic Voyage to Baffin’s Bay and Lancaster Sound in search of friends with Sir John Franklin,” by Mr. R. A. Goodsir (brother of the distinguished Professor of Anatomy in the University of Edinburgh), the following passage occurs at p. 138:—“Well pleased were the men in the evenings if they had a breeze sufficient to bring them back, but generally speaking their weary arms had to supply the motive power. But happy enough they seemed to be when they got on board: the boats were cheerily hoisted up; then each and all betook themselves to the infusions and decoctions of their tea and coffee, the whaling sailor’s greatest luxury and comfort. He has no objection

* My reviewer in the MEDICAL GAZETTE wonders that I have not performed “a new series of experiments” in support of my views. Surely he has not considered that such experiments as have been, and are now in progress, afford all the information required. Could either Professor Liebig or I determine by experiment the heat-producing powers of alcohol in the living human body, by any experiments short of a residence in the Arctic regions? And is not the experience of scientific men who have lived for months at a temperature of —50° as valuable a result as if they had been sent there on purpose to put this question to the test?

to his grog; but I think he has, long ere this, found out that *not strong tea or coffee*, particularly the former, is by far the best beverage he can take in these climates."

Much evidence to the same effect was recently given before the Committee appointed by the Board of Admiralty "to inquire into the expediency of diminishing the present quantity of spirits served out daily to the seamen in the Royal Navy." The witnesses were officers of various grades, medical officers, and seamen,—all of them *practical men*, speaking from their own experience and that of the service generally, and none of them (so far as I can gather) abstainers on principle. Their testimony is, therefore, peculiarly valuable. I cite *all* that I can find bearing upon this point, that I may not be accused of *selecting* facts to suit my purposes. Much of this evidence applies less expressly to *cold*, than to the ordinary exigencies of a seaman's life, of which exposure to cold, wet, and fatigue, is among the most common.

Captain Chadwick, Commander of the American packet-ship "Sir Robert Peel," examined.—Q. 222. "Have you ever been in a ship in which there was a total discontinuance of the use of spirits; and, if so, was it satisfactory to the men?"—A. "For the last twelve years I have sailed on the strict principle of temperance, and have found it work well, and no complaint among the men, and have had men from all European nations among my crews." Q. 225. "Do you think, from your experience, that the temperance system, as it is called, worked well, and without discontent amongst the men?"—A. "I am quite sure that in all our merchant service it works well: even in our *whaling ships*, of which there are near 700 vessels, there is not one in twenty in which spirits are allowed; and thus far they have been very fortunate in their voyages, which are from two to four years, generally cruising in *all climates* where their voyages can best be accomplished."

James Yeo, Esq., shipowner, from Prince Edward's Island, examined.—After mentioning the reasons which had induced him to diminish the allowance of grog in his ships, and to discontinue it entirely for the last two years, he states that the habitual allowance of the men is tea and sugar morning and evening, with an allowance of coffee when there is heavy work, in a wet night, or in a gale; and that no pecuniary compensation is allowed. Nevertheless, he has no difficulty in procuring good hands; and, being asked (Q. 289) whether he finds the same men constantly coming and continuing in his service, he replies, "The same men, and they get better satisfied after a voyage or two without the grog

than with it." The same practice (he says) is adopted in Nova Scotia and New Brunswick generally—localities in which it has been affirmed that the assistance of alcoholic liquors is particularly needed, on account of the cold damp fogs which are so frequent there.

Captain George Denny, brother-in-law of Mr. Frederick Green, the great shipowner of London, and formerly commander of one of his ships for twenty years, examined.—He states that the temperance principle has been tried in Mr. Green's employ, but found not to answer, from the unwillingness of the men.* The following is his opinion as to the effect of the substitution of coffee or cocoa for spirits:—A. 360. "We find that on giving a glass of grog, when any extra exertion is required, the men go more cheerfully through their work at night, such as reefing topsails, and they are very slack if you do not give them a little spirit." Q. 361. "Do you not think that on a cold night, if they had coffee or cocoa made for them, that it would be as useful?"—A. "They would then have to wait for the coffee or cocoa." Q. 362. "But when you expected a heavy night, you might have it in a state of preparation?"—A. "I think it would perhaps be better for their constitutions. I do not think that it would give them the same zest for their work." Q. 363. "Not so exhilarating?"—A. "No."

I would remark that Captain Denny's experience was, by his own showing, derived from a crew which had come into the system *unwillingly*; and it does not appear that the allowance of spirits on such an occasion had any other than a *mental* effect. Captain Chadwick and Mr. Yeo, who worked with *willing* crews, found the men just as well, or better, without it; and even Captain Denny allows that the coffee or cocoa "would perhaps be better for their constitutions."

Captain Cospatrick Baillie Hamilton, R.N., examined.—The evidence of this officer principally went to show that a great decrease in punishments in the Navy might be expected from a discontinuance of the spirit allowance. "I would sum up my opinion (he says) in these few words, that if we would reduce corporal punishments in the Navy, our first step should be to throw overboard the grog." Q. 444. "Have you anything to add to your evidence?"—A. "I would only mention that, on the occasion of my taking a passage in a merchant ship, spirits were not issued. The name of this vessel was the 'Maid of

* It appears that the trial was made, and the attempt abandoned, *some years ago*. The result would be probably different now. See the evidence of Lieut. Brown, R.N.

Mona.' I went in her from the Cape to Rio in 1844. We had severe weather off the Cape, but no spirits were issued. The crew were very cheerful and well conducted, and the master was an abstemious good man, and a thorough sailor. Some of the crew had been man-of-war's men, and one had deserted from the Navy. I spoke much to these men: they said they were well paid, well fed, and had a decent captain, and did not wish for spirits. They said they all liked their glass as well as others, but preferred good wages on the whole. I felt secure and comfortable on board that vessel, on account of their being no spirits. It was on this occasion that I was particularly struck with the advantage of a ship sailing without spirits on board; and from that time I have considered it as idle to say that seamen are necessarily happier or more contented from being supplied with grog."

Captain Peter Richards, R.N., examined.—Q. 750. "Do you suppose that spirits tend to support a man when labouring hard?"—A. "I should think they would stimulate a man." Q. 751. "Do you not think that tea or coffee, or cocoa, would have the same effect?"—A. "I dare say they would equally well, if you could persuade the men to take them." Q. 795. "Should you think it beneficial to the service if there were a discretionary power left with the captain to issue, under peculiar circumstances, coffee by night?"—A. "No doubt of it. I should prefer that greatly to a permission to issue grog." Q. 796. "Would it not be attended with great inconvenience to get fires at night in bad weather?"—A. "It so seldom happens, that I should think that under a necessity the inconvenience of getting fires ought not to be thought of."

Lieutenant J. H. Brown, R.N., Registrar of Seamen, examined.—The evidence of this gentleman, whose position has enabled him to acquire the most accurate and comprehensive knowledge of the habits and feelings of British seamen, shows that a great change is taking place in these in regard to the allowance of spirits. "Formerly they invariably demurred, and it was difficult to get a crew to sign the articles with such conditions; but within the last three or four years a change of feeling has gained ground to such an extent that, with the exception of some of the Steam Companies* and large shipowners, nine-tenths of the ships which sail from the United Kingdom have the stipulation,—either "no

grog" or "grog at the option of the captain." "The Committee will find, by the answers of the shipping agents throughout the United Kingdom, that such is the general feeling of the sailors, that they do not at present demur to sign articles where no spirits are allowed."

It is impossible to suppose that such a change should have taken place,—not suddenly, under the influence of enthusiasm or excitement, but gradually, and almost imperceptibly,—if the "universal experience" of the parties concerned had demonstrated that the discontinuance of the spirit-allowance had been in any respect otherwise than beneficial to them; it being to be borne in mind that the system is repugnant to the *tastes* of the class at large, and of the great majority of individuals.

WILLIAM COLES, gunner's mate in the "Rodney," examined—Q. 1159. "Do you think it would do, if it were to be the custom, instead of 'splicing the main-brace' in heavy gales, to have a cup of coffee given to the men at night who may get wet?"—A. "In a small vessel oftentimes the hands are turned up to shorten sail; they generally get wet before they have done the work. When a man comes down from aloft, after being half an hour shortening sail, I consider that a glass of grog enlivens a man."

Captain EDWARD TATHAM, R.N., examined—Q. 1320. "In bad weather what would your opinion be if coffee were issued to the men?"—A. "I think there would be a difficulty attending the making of it, but that a large portion of the men are very fond of it." Q. 1321. "Supposing any accident happened in a ship, for instance a mast or a yard being carried away, and the whole ship's company were employed, one of the means of refreshing the men that has been mentioned was that of making a small copperfull of coffee, and serving it out to them?"—A. "It is a capital thing if it can be done; giving them coffee or tea would be most popular, and they would like it, I am certain."

JOHN BELLOWES, a quarter-master, who has been in the service twenty-one years, states that he left off grog many years ago, from seeing his partner fall down and hurt himself whilst reefing top-sails. "I saw directly it was through the grog, and I saw that it was an evil; and I said, 'From this time I will have nothing further to do with it.'" Q. 1610. "When there is any severe weather, and on extraordinary occasions, if hot coffee were given at night, instead of 'splicing the main-brace,' would that be more satisfactory to the men?"—A. "It would be much better than grog. Before we came round Cape Horn the officers said that I should be glad to drink grog when I got to Cape Horn; but I found that I could

* The Peninsular and Oriental Steam Navigation Company has recently introduced the temperance, or rather *abstinence*, principle into the numerous ships in its employ.

do my duty without it. I was not on the doctor's list, and I did as well as the rest, and I used to stand four hours at the wheel."

ROBERT W. ROBERTSON, petty officer, 14 years in the naval service, examined—This witness had also served in four merchant vessels in which no spirits were allowed. He states that the system is increasing in the merchant service generally, and that he has heard no complaints in any vessel that he has been in. Q. 1759. "Did they ever give you coffee in bad weather instead of spirits?"—A. "It is customary in many ships to have coffee in lieu of spirits, at night time in particular, when the watch are roused out of their beds." Q. 1761. "Do you think coffee a more comfortable and nourishing beverage for the men than spirits after the sort of weather you have been describing?"—A. "Yes, I think it is; I have had it once or twice at my own expense on the passage home."

GEORGE MURRAY, quartermaster, of 16 years' standing, examined—Q. 2974. "During your late employment in the Arctic regions, was there an additional allowance of spirits occasionally served out?"—A. "Occasionally." Q. 2975. "When the temperature was very severe, or on what occasions?"—A. "Generally when towing. We were towing during the whole of the day, perhaps, from 8 o'clock in the morning until 12 o'clock at night; and Sir James Ross generally used to issue an additional half allowance of spirits, or to some men individually who might fall off the ice." Q. 2977. "But not for mere cold?"—A. "Not for mere cold: I do not think it is of any good against the cold." Q. 2978. "Would the men have liked any hot drink better, do you think, such as hot coffee?"—A. "I doubt much whether seamen would not rather have a glass of grog than hot coffee. I should prefer the coffee myself, and I dare say that a great many others would; I have no doubt of it."

DR. JOHN ROBERTSON, surgeon, R.N., 22 years in the service, examined—Q. 2243. "The present allowance being a quarter of a pint of spirits a day, to what would you recommend it to be reduced—to one half?"—A. "Yes, I should say one half: I should think the more it was reduced the better it would be both for their health, strength, and general welfare, if it cannot be done without altogether." This gentleman served in the "Enterprise," in the last Arctic expedition under Sir James Ross; and states that there were men in that ship who never drank their allowance of grog; also, that seamen generally do not care so much about their grog as they used to do, and that "the better sort of men are much more satisfied

with the allowance of tea."* Q. 2256. "Do you consider that in the ships employed in the Arctic regions, spirits are necessary for the men at all?"—A. "I should think so, certainly. At certain times spirits are highly useful in the Arctic regions, where the men suffer a great deal from wet, cold, and fatigue. I find that it assists in stimulating the nervous system and reviving their energies." Q. 2257. "Would it not be better to give them hot coffee, or cocoa, or tea, after such exertion, in preference to spirits?"—A. "Yes, I think so, undoubtedly; hot coffee or tea would be preferable. What I meant was, that they should have some comforts to recruit their strength." Q. 2258. "You think that would be decidedly preferable to spirits under all circumstances?"—A. "Yes, decidedly; unquestionably they require some indulgence on these occasions." Q. 2259. "What do you call indulgence?"—A. "Some restorative, as tea or coffee, but, where this cannot be had, spirits. It may be observed here that the limited amount of fuel will not admit of frequent hot messes in the Arctic regions."

MR. JAMES DONNET, surgeon, R.N., 10 years in the service, examined—Q. 2323. "After rainy weather, and when the men perhaps have been up all night, or half the night, which would you recommend as preferable, 'splicing the main brace,' or giving them a basin of hot tea or cocoa?"—A. "I would recommend warm tea, it is proved to be the most invigorating beverage in such cases." Q. 2324. "You are no advocate at any time for serving an additional allowance of spirits?"—A. "There may be some particular circumstances where a man may be much benefited; he may be so debilitated from the work that he might require spirits. I have heard one of the officers of the 'Quail,' which was wrecked in 1836 in the Bay of Biscay, say that for ten days she lay as a wreck on the water; the man at the helm was obliged to be lashed, from the sea at times making a clean breach over her. During this time they were offered grog every hour, and warm tea. All of them refused the grog and drank the tea, not that they disliked grog, but tea proved in their case the most invigorating beverage."

This witness is now serving as surgeon on board one of the ships despatched this spring to the Arctic regions in search of Sir John Franklin. All these expeditions are sent

* All the witnesses concur in stating that, since tea has been issued in the evening, scarcely any of the men care for the allowance of grog which is issued at the same time; and this consequently becomes the perquisite of the cook of the mess. To this system most of the witnesses attribute the greater part of the drunkenness and dissipation which occurs on board ship.

out on the "temperance" principle,—a pretty plain proof that the Admiralty (a body not particularly easy to move, as our profession knows to its cost) is well convinced of the special adaptation of this system for crews serving in the coldest climates. I happened to be in company with Mr. Donnet on the afternoon of the day on which he had been engaged in examining and passing the men who had volunteered for this service. He mentioned that he had most explicitly informed them that they were to have "no grog" during the three years that they might expect to be absent, and that there was no objection, nor even murmuring, among them. Mr. Donnet expressed himself as so completely satisfied of the superior efficacy of the abstinence system as regards the endurance of cold, that he fully purposed adopting it himself, and endeavouring to persuade his brother-officers to do the same.

Dr. PATRICK MARTYN, formerly surgeon, R.N., 23 years in the service, examined—Q. 2354. "After the men have endured great fatigue, and perhaps have got very wet, which do you think it would be preferable to give them as a refreshment and comfort, a glass of spirits, or a good dish of hot tea or cocoa?"—A. "I think, looking at it in a medical point of view, that the hot tea would be better for them." Q. 2355. "Do you mean tea particularly, or tea or coffee?"—A. "Tea or coffee. Coffee, perhaps, would be better, being slightly stimulating, and it is not followed by the after-consequence of spirits. There is a depression and collapse in every instance after spirituous stimulants." A. 2372. "I have seen some of the hardest men in the sea-service testotallers, some pilots in North America, for instance." A. 2373. "I recollect in North America having more than once pilots on board in snow-storms, and instances of men on the bowsprit-end for twelve hours at night, and they refused to have grog. I recollect advising some to take it, thinking from their great exposure that they required it, but they took coffee in preference."

Dr. FITZWILLIAM MANSELL, a half-pay surgeon, R.N., 17½ years in the service, examined—This witness states, that in his opinion half the present allowance would suffice for the necessities of any man,—obviously referring to those who have been accustomed all their lives to the stimulus, and thinking that the younger men might go without it with advantage. Q. 2704. "Do you think that in cases of any great exertion, and when they would, according to the practice at present, 'splice the main-brace,' it would be better and more efficacious for the men to have a cup of hot tea or coffee than

a quantity of spirits?"—A. "I think a cup of hot tea or coffee would be more efficacious, and it would enable a man to do more work than the stimulus arising from spirits, which only exists for a certain time. I dare say that a man would be able to go through more physical exertion with tea or coffee,—that is, continuous exertion."

It appears, then, that a very wide experience has now pronounced decidedly in favour of abstinence from spirits when severe and prolonged cold has to be endured; no theoretical considerations having had the least bearing upon the conclusion, which is founded upon the results of the severest trials that could be well devised, carried on through a long period of time. The testimony of Sir John Richardson and Dr. Richard King, readily given to myself, would be confirmed, I am assured, by that of other Arctic voyagers and travellers; and nothing short of the most positive conviction would have induced the Hudson's Bay Company to prohibit the introduction of spirits into their stations, or would have caused the Admiralty to send out the recent Arctic expeditions upon the "temperance" principle.

It must be remembered, that upon this question the *feelings* of the parties concerned are entirely on the side of the indulgence; and it is not likely that, unless strongly urged by their reason, they would ever voluntarily relinquish it. Notwithstanding this, we find the whaling sailors of our own country and of the United States putting their trust, not in grog, but in tea, coffee, and cocoa; and although it may be said that they are forced to do this by the regulations under which they enter, yet it is perfectly clear that such regulations could not have been introduced had they not been acquiesced in by the men. A large number of the best hands now prefer the employ in which "no grog is allowed," and return to it for successive voyages; notwithstanding that the small crews of merchant vessels are compelled to much more severe exertion, and are subjected to much more exposure, than are the far more numerous crews of a ship of war. And even with regard to a temporary exposure to cold and wet, with severe labour,—circumstances under which a stimulus will be best borne, if beneficial at all,—it will have been seen that there is scarcely one of the witnesses, who does not allow that hot tea, coffee, or cocoa, if these beverages can be procured when they are wanted, would be really more serviceable than grog, although the natural liking of the seaman may lead him to prefer the latter. The four medical witnesses all concur in this view.

It is upon such grounds as these, then, and not upon any theoretical assumptions, that I venture to claim for my statement of the incapability of alcoholic liquors to afford the power of continued and severe cold, or any special protection against the effects of cold, wet, and fatigue combined, the value of an established fact. Those who affirm the contrary are bound to disprove the assertions, not only of occasional voyagers and travellers in Arctic regions, but of those whose regular vocation keeps them during a large part of their lives in a temperature far below zero. And, in order to effect this, they must not satisfy themselves with the mere assertions of those who find a glass of grog very comforting to them and feel loath to give it up; but must procure, if possible, the impartial testimony of those who have tried both plans, and who have not any preference in favour of either, save as far as it may serve its purpose best. When they have got a body of evidence adequate to rebut that upon which the Hudson's Bay Company, the Admiralty, 19-20ths of the 700 whale ships of the United States, and a large part of our own mercantile marine, are at present proceeding, it will be time to take up the question afresh. Until then, "universal experience" may be safely affirmed to be on the side of the abstainer.

It may be objected that I have only alluded in this discussion to distilled spirits, and that *they* may be admitted to be useless, or even injurious, without any argument being thence afforded against the use of the less potent fermented drinks, such as wine or beer. To this I have to reply: *first*, that all the data obtainable as to the results of experience, apply simply to the question between spirits on the one side, and tea, coffee, and cocoa on the other. I am not aware that any attempt has been made to test the virtues of wine or beer by comparative experiment. And *secondly*, if alcohol be proved to exert no such power, even in the form in which it is commonly considered the most potent, it seems fair to conclude (at least until the contrary shall have been proved), that the similar virtue ascribed to other alcoholic liquors is equally fallacious, or that, if these liquors really possess it, they owe it to their other ingredients.

It is worthy of remark, that the witnesses of all grades, examined by the Admiralty Committee, agreed in attributing to the present system of spirit-allowances from *three-fourths to nine-tenths* of the offences for which punishments are required on board ship; a large proportion of these offences being committed in a state of absolute drunkenness, and others in a state of irritability which the regular allowance ap-

pears to produce in some men, who never become intoxicated. Surely common sense teaches us that, if a crew cannot be induced to practise moderation in the use of an indulgence, which cannot be shown to have any beneficial effect on him whatever, and is continually disturbing the peace and harmony of the community, such indulgence should be withheld, except upon special occasions. This measure it is obvious that the Admiralty Committee were well inclined to adopt, several distinguished officers having urged it upon them. But it was of course essential for them to consider the practicability of such a change; and it was determined to begin by reducing the present allowance of spirits to one half, leaving it to time to show whether, in the course of a few years longer, the spiritration may not be entirely given up, without that hazard of rendering the good navy-sailor indisposed to serve in his country's ships, which such a step would at present most probably produce.

. Although the evidence adduced by Dr. Carpenter in favour of abstinence from "spirits" when severe and long-continued cold has to be endured is very strong, yet those who, like our reviewer, adopt the views of Liebig on this subject, may not consider it by any means conclusive. By "spirits" we understand gin, rum, brandy, or other liquid containing about fifty per cent of alcohol, to which alone the term "alcoholic liquors" can be with any propriety assigned. The evidence of several of our eminent Arctic travellers, whose opinions are deserving of the highest respect, would, we believe, if submitted to cross-examination, lead to the following conclusions:—1. That injury was done, not by the moderate, but by the immoderate use of strong spirituous liquors; 2. That a moderate use of spirits diluted with water would not be attended with injurious consequences; 3. That coffee, tea, and cocoa, have not yet had a sufficient trial to allow them to be employed universally as substitutes for spirituous liquors; 4. That the recommendation of entire abstinence from spirituous liquors is rather based on the difficulty of restricting their use within moderate bounds, than on any proved injury to the system by their occasional use in a diluted state, and in moderate quantity. The Admiralty evidence, it must be remembered, represents merely the opinions of a few Arctic voyagers: it is the result of an examination in chief; and, had the non-entire-abstinent party been fully represented by learned counsel, it is probable that the cross-examination of the witnesses would have put a very different interpretation, on the whole of the facts.

Before accepting this evidence, therefore, as a complete refutation of Liebig's views, we should be glad to have some facts connected with the *moderate* (not immoderate) use of spirituous liquors in northern regions. Do the Norwegians, or the inhabitants of the northern parts of the Russian empire, find that during the cold season they are as well sustained, in the long run, by the use of coffee, tea, or, in the absence of these luxuries, by *water*, as by the occasional use of spirits, so taken as not to produce intoxication? It would be safer, for the sake of truth, to settle this question by reference to the habits of persons who are born and actually reside in these climates, than by the observations of others born in different countries, and who only occasionally and for comparatively short periods visit the Arctic Regions. The Hudson's Bay Company, the Admiralty, and the Commercial Whaling Companies, are now trying experiments which in some eight or ten years they may have to lay aside. We cannot, therefore, go the length involved in Dr. Carpenter's assertion, that "universal experience is on the side of the *abstainer*." On the contrary, we believe that with properly sifted evidence, with long experience, and by the aid of observations made on individuals constantly inhabiting cold climates, and who know nothing of scientific theories concerning oleaginous and alcoholic fuels in respiration, it will be found that *excessive use* only is prejudicial, and that a *moderate use*, if not an absolute necessity, is certainly not injurious to health. The Hudson's Bay Company have, we believe, an excellent sanitary motive in prohibiting the introduction of spirits into their stations: it has nothing to do with ultra-abstinence principles; but it is intended for the prevention of the mischief arising from excessive indulgence among the inhabitants of their factories, and the Indian tribes around them. If they could so regulate matters that these excesses would not take place, it is our belief that they would be without a shadow of reason for preventing the introduction of spirits, and that the prohibition would cease. The evidence, therefore, in favour of the *total abstainer*, is, in our judgment, neither universal nor wide.

So far as wine or beer is concerned, Dr. Carpenter virtually abandons the question. There is no proof that in *moderation* these liquids are injurious: there is an abundance of proof that under these circumstances they are highly beneficial to health, although like butter, cheese, and some other articles of food, they may not be absolute necessities to the generality of mankind. The varieties of wholesome *beer* contain only from one to three or four per cent of alcohol: they contain grape-sugar, vegetable

matter abounding in nitrogen, with bitter and other principles capable of affording nutriment. To assert that beer is an "alcoholic liquor" because it contains so small a percentage of alcohol, is, it appears to us, a complete misuse of language. We might as reasonably describe the blood as an oily or fatty liquid. It is very well known that the same amount of alcohol taken in water would not act like beer; and there is no proof that the small quantity of alcohol or of its elements contained in beer, acts in any other way than by preserving the liquid from decomposition. If beer be taken in excess, it produces alcoholic poisoning or intoxication: but what article of food is there, either solid or liquid, which can be taken in excess without producing injury to the system? Tea or coffee would be no proper substitute for beer, any more than dripping or train oil would be a proper substitute for butter. Those who could be induced to make the change would probably find that beer and butter were not actual necessities any more than tea, coffee, or train oil. Under these circumstances, we do not perceive why, so long as moderation is observed, the consumer of tea or coffee should not allow the consumer of beer or train oil to follow undisturbed his own taste. We think evidence might be easily adduced to show that the immoderate use of strong tea is decidedly injurious to health, and that tea-totalism is as great a delusion as vegetarianism. But this would have no more influence in inducing us to recommend the prohibition of the use of tea, than the outcry against beer as an "alcoholic liquor" would induce us to prohibit this national beverage which was used in England for at least seven hundred years before tea or coffee was known.

PARACENTESIS IN ACUTE PLEURISY.

DR. HOMANS reported the case which he had recently seen in consultation with Dr. Morrill Wyman, of Cambridge. The patient was a healthy woman, and the operation was done on the twelfth day of the disease. There was great pain, and such urgent dyspnoea that she had been unable for some time to lie down in bed; with enlargement of the side and other physical signs of effusion. An exploring needle having been passed in by Dr. W., about an inch below the left scapula, eighteen ounces of serum were drawn off, but no pus; and with such relief that the patient was able to sleep comfortably that night, in the horizontal position. Two or three days afterwards about half as much more was drawn off, and recovery soon followed. *American Journal of Medical Sciences.*

Medical Intelligence.

SLOW POISONING BY LEAD.

THE subjoined letter, recently addressed by Mr. Herapath, of Bristol, to the Editor of the *Times*, is well calculated to show the subtle nature of lead-poison even when it exists in the very insoluble form of carbonate of lead.

"Sir,—Your number of yesterday, in some remarks upon Dr. Scoffern's patent for purifying sugar by sulphurous acid, left the public in doubt as to what quantity of lead might be taken by human beings without injurious effects. Some time since, in the West of England, a river, the water of which had been used from time immemorial by the inhabitants of a village on its banks without injury, was found to affect their health; symptoms of indigestion abounded, with loss of flesh and appetite; and there were some few cases of colic: they believed that it arose from the use of the river water, as those who used water drawn from a spring at some distance were not so affected. I was requested to analyse the river water, and found in it 1-500,000th part of carbonate of lead, which arose from a mine worked at the distance of three or four miles from the village, on the other side of a range of limestone hills.

"Your paragraph leaves it doubtful whether $1\frac{1}{4}$ grain of lead taken in a week would be injurious. In the case I relate there would be only one grain of lead in nine gallons of water; and yet the health of the neighbourhood was seriously affected.

"I am, Sir, yours respectfully,

"WILLIAM HERAPATH.

"Old Park, Bristol, Sept. 12."

. Allowing that the individuals poisoned by this water consumed as much as a gallon per day, or swallowed in some form or other the lead contained in that quantity of water, it follows that they would have taken no more than about *three-quarters of a grain* of lead per week! This infinitesimal quantity, according to Mr. Herapath, was quite sufficient to endanger health. An observation of this kind clearly shows that we are not able to set a limit to the proportion of lead in an article of food consumed daily, which will be inert or free from consequences injurious to health. If one *half-millionth part* of insoluble carbonate of lead may thus affect health, it is quite within the range of probability that this or a larger proportion of sulphite, or any other insoluble salt of lead, taken daily in sugar, treacle, or any article of food, may prove equally injurious. In

reference to the sugar-refining question, Dr. Gregory asserts that the highly *insoluble* carbonate of lead is the *truly poisonous* lead-compound; while he says it might be predicted of the sulphite of lead that it is as harmless as chalk, *because of its excessive insolubility*. This is an ingenious way of dealing with chemical doctrines. Either insolubility (in water) has nothing to do with the question of poisoning, or the carbonate of lead should be just as harmless as chalk,—an assumption which is contradicted by daily experience. It may be said that 500,000 parts of water will dissolve one part of carbonate of lead: this is highly probable; and, as insolubility is only a relative term, it is equally probable that this, or even a much smaller quantity of water, would dissolve one part of the sulphite of lead. The "excessively insoluble" sulphate of barytes is dissolved by 40,000 parts of water. It must be remembered, however, that the secretions of the stomach differ from water. They contain organic matters, acids, and salts, which act upon mineral substances in a way which chemical experiments on the solvent powers of water would fail to explain. Mr. Herapath has done good service by showing how minute a quantity of lead will affect human health; since facts of this kind will prevent a general trial of lead-refined sugar with the serious risk of endangering the health of the population.

MEDICAL SUPERNUMERARIES AT FORT PITT.

A CORRESPONDENT of the daily journals gives the following unpromising account of the occupations and prospects of young military surgeons stationed at Fort Pitt, Chatham:—"The supernumeraries," he says, "consist of a fluctuating number of young gentlemen fully educated as medical men, many of them possessing University degrees, and all having passed, in addition to the ordinary professional ordeals, the Army Medical Board examination. These, having through the influence of friends, or as a reward of merit from certain colleges and universities, received Sir J. M'Gregor's recommendation for a medical commission in the army, are ordered to Chatham to do duty at Fort Pitt for two or three months until a vacancy occurs, and are informed that meanwhile they must lodge and maintain themselves. They report themselves to the principal medical officer, take lodgings near the Fort, are subjected to military discipline, and confined within the walls of Fort Pitt (often with severe hospital duty) daily from 9 A.M. until 1 P.M., and occasionally until 5 P.M. They are compelled to join an expensive mess, and to subscribe to the medical library and to a fund for officers' widows. In return the four seniors (called

acting-assistants) receive 5s. *per diem*, and the rest, to the number of ten or twelve, nothing. Their sojourn at Fort Pitt is frequently eight, ten months, or even longer, instead of two or three; and accordingly it sometimes happens that one of these unfortunates, having with difficulty secured what he was led to consider a sufficient fund to enable him to take his commission, is compelled to expend so much while waiting for it that he is obliged to throw up his claim, and to retire with the comfortable reflection that he has given one of the best years of his life, and wasted enough money to have started him in a private practice, not to speak of the actual hand and head work which has thus been exacted without remuneration, and which is very often exceedingly severe, and to a fully qualified medical man somewhat degrading also."

THE CHOLERA IN ALGIERS.

ACCOUNTS from Algeria state that the cholera had ceased at Bona, and broken out at Biscara, where it attacked dogs, fowls, and other animals, with the same violence as human beings. Several officers of distinction, amongst whom is the commandant of Biscara, have fallen victims to the malady. It was, however, believed that the period of its disappearance was near at hand.

MORTALITY OF NEW YORK IN 1849.

THE visitation of epidemic cholera, during the last year, has given an appalling increase of the deaths occurring in 1849, the aggregate being 23,773, which is more than 5 per cent of the population, estimating the latter at 450,000, which is not far from the truth. The average mortality of the city for the last ten years, prior to 1849, was not more than $\frac{1}{4}$ per cent.; so that it has more than doubled during the past year. That the increase is mainly to be ascribed to the direct and indirect effects of the epidemic is scarcely susceptible of doubt. For, although only 5,071 of the deaths are set down as cholera cases, yet several thousand more were the undoubted victims of the epidemic atmosphere equally with these, although reported among the interments as diarrhoea, dysentery, cholera infantum, and cholera morbus! by those who falsely claimed to cure the cholera, and thus concealed the cases they lost, under other names.

FEE'S PAID TO THE MEDICAL ATTENDANTS OF THE LATE SIR ROBERT PEEL.

AN evening paper states that the executors of the late Sir R. Peel have, within the last few days, remitted fees to the various medical gentlemen who attended him, and endeavoured to alleviate the effects of the fearful accident which was the means of

terminating his life:—To Sir Benjamin Brodie, Bart., 250 guineas; to Alexander Shaw, Esq., 100 guineas; and to the other gentlemen proportionate sums.

EXPENSES OF PUBLIC BOARDS.

A PARLIAMENTARY paper has just been printed, showing in detail the expenses of the General Board of Health, the Commissioners in Lunacy, and the Poor Law Board. The expenses of the General Board of Health to the 31st of March last were £3,084. 6s. 2½d. The travelling and personal expenses of the Commissioners in Lunacy, in the year ending the 31st of July, were £2,355. 11s. 11d.; and of the Poor Law Board, for the year ending the 31st of March last, £2,878. 0s. 2d.: leaving a surplus of £1,261. 19s. 10d. For the General Board of Health £8,000 were voted; for the Commissioners in Lunacy the exact sum; and for the Poor Law Board £10,000.

APOTHECARIES' HALL.

NAMES of gentlemen who passed their examination in the science and practice of medicine, and received certificates to practise, on Thursday, 12th September, 1850:—William Smith Scholefield, Lartingham—Benjamin Mallan—Thomas Wheeler, Ulverstone—William Browne—Thomas Johnson, Weymouth.

OBITUARY.

PROFESSOR MARCHAND.

WE have to announce the death of Professor Marchand, which lately took place at Berlin from an attack of cholera. The deceased was the discoverer of some useful tests in pharmaceutical chemistry and toxicology.

MR. THOMAS DAVIS, ESQ.

On the 25th ult., at Hampstead, Middlesex, aged 73, Mr. Thomas Davis, surgeon.

ALEXANDER TAYLER, ESQ.

On the 7th inst., at Queenstown, Ireland, Alexander Tayler, Esq., for sixty-six years surgeon in the Royal Navy.

JOSEPH NEVILLE, ESQ.

On Thursday, the 12th inst., at Croydon, after a protracted illness, Joseph Neville, Esq., surgeon.

CASE OF RECOVERY FROM THE PASSAGE OF AN IRON BAR THROUGH THE HEAD. BY HENRY J. BIGELOW, M.D., PROFESSOR OF SURGERY IN HARVARD UNIVERSITY.

THE accident occurred upon the line of the Rutland and Burlington Railroad, on the

18th of September, 1848. The subject of it, Phineas P. Gage, is of middle stature, twenty-years of age, shrewd and intelligent. According to his own statement, he was charging with powder a hole drilled in a rock, for the purpose of blasting. It appears that it is customary in filling the hole to cover the powder with sand. In this case, the charge having been adjusted, Mr. Gage directed his assistant to pour in the sand; and at the intervals of a few seconds, his head being averted, and supposing the sand to have been properly placed, he dropped the head of the iron as usual upon the charge, to consolidate or "tamp it in." The assistant had failed to obey the order, and the iron striking fire upon the rock, the uncovered powder was ignited and the explosion took place. Mr. Gage was at this time standing above the hole, leaning forward, with his face slightly averted; and the bar of iron was projected directly upwards in a line of its axis, passing completely through his head high into the air. The wound thus received was oblique, traversing the cranium in a straight line from the angle of the lower jaw on one side to the centre of the frontal bone above, near the sagittal suture, where the missile emerged; and the iron thus forcibly thrown into the air was picked up at a distance of some rods from the patient, smeared with brains and blood.

From this extraordinary lesion, the patient has quite recovered in his faculties of body and mind, with the loss only of the sight of the injured eye.

The iron which thus traversed the skull weighs thirteen and a quarter pounds. It is three feet seven inches in length, and one and a quarter inches in diameter. The end which entered first is pointed; the taper being seven inches long, and the diameter of the point one quarter of an inch; circumstances to which the patient perhaps owes his life. The iron is unlike any other, and was made by a neighbouring blacksmith to please the fancy of the owner.

Dr. Harlow, in the graphic accounts above alluded to, states, "immediately after the explosion the patient was thrown upon his back, and gave a few convulsive motions of the extremities, but spoke in a few minutes. His men (with whom he was a great favourite) took him in their arms and carried him to the road, only a few rods distant, and sat him into an ox cart, in which he rode, sitting erect, full three quarters of a mile, to the hotel of Mr. Joseph Adams, in this village. He got out of the cart himself, and with a little assistance walked up along flight of stairs into the hall, where he was dressed.—*American Journal of the Medical Sciences*, July 1850.

BOOKS & PERIODICALS RECEIVED FOR REVIEW.

(The List will be given in our next No.)

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer 30.17
 " " " Thermometer 55.3
 Self-registering do. Max. 0.0 Min. 30.
 * From 12 observations daily. * Sun.

RAIN, in inches, 0.0.—Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was about 2.5 below the mean of the month.

BIRTHS & DEATHS IN THE METROPOLIS During the Week ending Saturday, Sept. 14.

BIRTHS.	DEATHS.
Males.... 725	Males.... 463
Females.. 684	Females.. 466
1409	929

CAUSES OF DEATH.

ALL CAUSES	929
SPECIFIED CAUSES	926
1. Zymotic (or Epidemic, Endemic, Contagious) Diseases....	238
<i>Sporadic Diseases, viz.—</i>	
1. Dropsy, Cancer, &c.	47
2. Brain, Spinal Marrow, Nerves, and Senses	95
4. Heart and Bloodvessels.....	33
5. Lungs and organs of Respiration	91
6. Stomach, Liver, &c.	60
7. Diseases of the Kidneys, &c.	18
8. Childbirth, Diseases of Uterus, &c.	9
9. Rheumatism, Diseases of Bones, Joints, &c.	9
10. Skin.....	0
11. Premature Birth	20
12. Old Age	24
13. Sudden Deaths.....	20
14. Violence, Privation, Cold, &c....	51

The following is a selection of the numbers of Deaths from the most important special causes:

Small-pox.....	7	Convulsions.....	33
Measles.....	15	Bronchitis.....	38
Scarlatina.....	38	Pneumonia.....	40
Hoping-cough.....	20	Phthisis.....	117
Diarrhoea.....	78	Lungs.....	5
Cholera.....	4	Teething.....	10
Typhus.....	47	Stomach.....	1
Dropsy.....	17	Liver.....	4
Hydrocephalus.....	26	Childbirth.....	4
Apoplexy.....	33	Uterus.....	4
Paralysis.....	17		

REMARKS.—The total number of deaths was 64 below the average mortality of the 37th week of ten previous years.

NOTICES TO CORRESPONDENTS.

The letter on the Testimonial to Dr. Conolly shall appear in the following number.

We regret that our space is so occupied that we cannot this week find room for the report of the Committee of the National Institute, or for various papers which are in type.

Dr. Blacke's work on Asiatic Cholera has reached us. It shall be noticed in an early number.

The following papers have been received, and will have early insertion:—Mr. Noble on the Contagion of Cholera; Mr. Kingdon's case of Sloughing of Malignant Tumor; and the King's College Hospital Report.

Lectures.

LECTURES

ON THE

MEDICAL JURISPRUDENCE OF
INSANITY.

*Delivered in the Medical School of King's
College, Aberdeen.*

BY ROBERT JAMIESON, M.D.

Lecturer on Medical Jurisprudence in the
University.

LECTURE V.

*General description of the insane state
(continued)—(c) Insane impulses—(1)
The suicidal impulse—Suicide amongst
the insane—Its Lunatic characteristics—
Medico-legal consideration of suicide—
Felo-de-se—legal consequences—Self-
murder, insane suicide, and suicidal
moral insanity—Circumstances of these
contrasted—Suicide from suggestive op-
portunity, fascination, imitation, &c.—
When is suicide to be deemed an act of
insanity?—Legislation on the subject of
suicide—(2) The homicidal impulse—
Medico-legal consideration of homicide—
Insane homicide and homicidal moral in-
sanity—Reil's description of a paroxysm
of mania without delirium—Delirium
in agendo—Criminal homicide and in-
sane impulse contrasted—When is homi-
cide to be deemed an act of insanity?—
Connected delusion—Contemporaneous
delusion—Consecutive delusion—Moral
insanity.*

THE extravagant conduct evidentiary of intellectual disorder which we have just been considering does not unfortunately comprehend all the diseased actions that lunatics commit. We have yet to speak of those which do not arise out of delusion solely, but are rather the exponents of the diseased will than of deranged understanding. Insanity is not in every instance want of wisdom merely, but often want of virtue also; it may have the aspect of crime as well as that of folly. When the former predominates, the medico-legal difficulties are great. Lunatics are subject to a variety of *diseased impulses*. This is not merely a medical assumption, but a fact proved by the nature of the actions to which they lead, and the testimony of those who have experienced them. There are no psychological nor physiological grounds for denying that these morbid propensities may be the sole indication of mental disease, and the doctrine is upheld by the testimony of many who have felt its truth, and is ad-

mitted by all who have had opportunities of studying the pathology of the mind. When such diseased impulses appear to be uncombined with delirium, they form varieties of what is termed "moral insanity"—that species of insanity (called partial) in which the outward exhibition of the disease is limited to the emotional faculties and the power of self-control.

The suicidal impulse.—The lunatic propensity to self-destruction is the one which we shall first discuss: it is the most common, and certainly not the least curious. The homicidal impulse is only a diseased manifestation of the destructive power, and of the natural inclination to revenge—of tendencies counteracted by moral considerations only; so with kleptomania—so with dipsomania: but self-destruction is a defiance of the strongest instinct of our nature, and, if sanely executed, the extremest assertion of personal freedom.

Amongst the insane, suicide is most frequent in those labouring under monomania with depression, but it may occur as a consequence of any of the forms of insanity. I have known it to be committed by one labouring under extreme and long-standing dementia, in whom there had never been the slightest grounds for suspecting such a desire. In a thousand lunatics the suicidal inclination was clearly developed in a hundred; and of these, five succeeded in effecting their object. It may be the result of delirium, or false perception; of delusion, of sudden impulse, of insane reasoning, of suggestive opportunity, and of example. It is chiefly to be dreaded in those lunatics who are fond of solitude, and who are the subjects of melancholy, insane terrors, and religious delusions.

The suicidal propensity of the insane has morbid features in addition to the disease manifested in the impulse. There are inconsistencies in the desire, and peculiarities in the modes of its accomplishment. The hypochondriac believes that he is dying of an incurable malady, and is miserable because he is dying; yet he seeks, nevertheless, to terminate his own existence: a monomaniac, fearful of everlasting torments, eagerly seeks to anticipate them with violent hands: another, fearful of the gallows, bends every thought to compass a noose for himself in some unseen corner, apart from those that watch him: and a lunatic who believes that he is already dead, and in the other world, is not by any means to be entrusted with a knife. The delusion of an insane person impels him towards suicide, but often it is suicide in a particular way. He is fascinated, say, by the notion of hanging himself, and requires much care to prevent his design; but, if he is successfully thwarted in this, he may

often be trusted with other means of injuring himself. The person who is on such an account compelled to sleep in a bare apartment, whose walls do not exhibit the temptation of a single peg or projection, upon a low bed without posts or curtains, and whose framework is tightly screwed to the floor, may be perhaps allowed with safety to eat his meals with knife and fork, and to enjoy other liberties and comforts which must be denied to others whose suicidal taste is of a different description. In the same way, a lunatic may go on contriving how he shall find an opportunity to cut his throat, to the neglect of all means that may present themselves for hanging himself by the neck. Modes of death are chosen of a kind unknown unless in the history of insane suicide: for example, suicide by starvation is not unfrequent among the insane, but scarcely ever attempted, or if attempted, never persevered in from preference by any person of sound mind. I should perhaps except the case of Pomponius Atticus, the friend of Cicero; but it is not unlikely that his painful disorder had affected his mind: at all events, he was in such a condition as to die on the fifth day, and therefore his perseverance was not brought to an extreme trial. Even amongst prisoners condemned to death the attempt is uncommon, and success in it almost unknown. Amongst the insane it is otherwise. I have myself known of more than one instance. Sometimes, in obedience to a delusion, it is had recourse to from choice, but more frequently it is the last resource of a lunatic thwarted in other modes of self-destruction. The attempt may be combated in many ways, and is usually overcome; but, if the insane resolution fail not—and the insane will is sometimes unconquerable—a certain and a fearful tragedy has begun. The propensity is treated as a disease, but the subject of it resists physic. Leeches are applied to the head, a blister to the neck—something is done to the epigastrium: he is left alone with food—he is tempted with delicacies—he is encouraged to drink—he is promised all things: but he holds out, and emaciates hourly. Food is introduced into the stomach by a tube passed into the œsophagus by the mouth or nostrils: he resists with all his might; and, if unsuccessful, attempts to excite himself to vomiting. He is placed under the influence of chloroform, perhaps, but becomes only insensible, not compliant: he is violently fed daily, but is evidently gaining the victory. The struggle becomes dangerous by the exhaustion itself induces. Nourishing enemata are resisted and expelled; and, some day, when raised up, he suddenly falls back and dies of syncope. In a well-conducted asylum, starva-

tion is almost the only possible mode of suicide. In those sound of mind it is nearly an impossible kind of self-murder.

Medico-legal consideration of suicide.

Suicide is a crime in law—a fact not of a sort to be of necessity presumed. It is, it seems, the crime of *felo-de-se*—a phrase of dubious latinity, understood to indicate self-murderer. Of the moral guilt there can be no doubt; but there are many unchristian offences of which legislation makes no account, which it would be more rational to recognise, both for the weight of example, and on the principle of punishing: for those who are bent on self-destruction on any other grounds than a hysterical impulse of imitation, are incapacitated to profit by application of the example, and remove themselves from the influence of human inflictions; the worst is already looked upon and encountered; nothing can harm them more. The goods of a *felo-de-se*—that is, his own personal and moveable property—are forfeited to the Crown, and his will in relation to them is invalid. Formerly, the body having been further mangled by being transfixed with a stake, was buried dishonourably under the highway. Having disgraced his body and robbed his children, the law entitled the wife to her dower, and permitted the real estate to descend to his heirs, or fall to any to whom it was bequeathed. Suicide is not evidence of insanity. The mere fact of a testator making away with his life, although immediately after the execution of his will, is not deemed sufficient ground for setting aside the testament, in so far as the crime of his death permits it to be valid. If an individual commits murder, and thereupon attempts self-destruction, this will in no way affect his legal responsibility. Still further, if an individual in attempting suicide injured another, the mere fact of suicidal intention would afford no plea against trial for homicide. Thus, if a person bent on self-destruction turned a loaded pistol to his head, but from nervous agitation, or some other cause, missed his mark, and shot some one behind him, the suicidal aim would not free him from the charge of blood-guiltiness. One attempting death by drowning has been found liable for the accidental death of an individual who jumped into the water to save him.

We shall distinguish three kinds of suicide—viz., self-murder, insane suicide, and suicidal moral insanity.

Self-murder is that voluntary rational suicide which is implied in the technical phraseology of *felo de se*. Suicide is not always insanity; it occurs under circumstances which do not indicate incapacitated

judgment or diseased will. It may be the sinful result, in an unchristian mind, of a vigorous comparison betwixt long-continued mental distress and transient physical pain, betwixt one mode of violent death and another, between present actual wretchedness and a doubtful condition of future existence. It may be evil done that good may come: thus Leonidas volunteered the required loss of Heracleidan blood at Thermopylæ; Marcus Curtius leaped into the chasm; the citizens of Calais offered themselves for the gallows: and the virgin may prefer suicide to certain dishonour. In some countries suicide has been in certain circumstances a religious duty or a customary etiquette. The Japanese dignitary rips up his abdomen when disgraced from office; the concubines of the Peruvian Inca sacrificed themselves on his tomb; and the Hindoo widow immolates herself on the funeral fire. Self-murder is, however, I believe, the rarest form of suicide. It is to be presumed to be a sane act, in the absence of contrary evidence, when such motives as the above are clearly traceable; but even then, when it is the deed of remorse or despair, it is the result of a condition as uncontrollable by the judgment as a diseased impulse. Suicide, preceded by revengeful murder, following intelligible motives, implies *felo de se*. The less painful and the more gradual the mode of death selected, the greater would be the presumption of sanity. Death by poison, charcoal vapour, or chloroform, would less indicate a diseased impulse than would pistolling, cutting the throat, or precipitation. The presumption of insanity would also be weakened by the fact of self-destruction being the result of a community of resolution; as when the member of a suicide club, if such a society exist, is no more to be seen, or the crew of a boarded ship blow themselves and the enemy into the air, or the daughters of a Lord Archibald plunge together into the lake. The conjunct suicide of two individuals only, especially of the opposite sex, may, however, be a conjunction of insanity with a diseased intensity of sympathetic emotion.

Insane suicide is the phenomenon of self-destruction occurring amongst those labouring under any of the forms of insanity, as already spoken of, and on which it is unnecessary to dilate. The deed is to be held as resulting from the mental disease in all cases in which it is committed by a lunatic, whether it can be seen to be attributable to the influence of disordered judgment or not; for we can neither trace all the diseased motives which govern the conduct of the insane, nor, as we have already seen, reconcile their actions with the objects which

impelled them. There is no rationale in the doings of lunacy.

Suicidal moral insanity.—The cases of self-destruction which we are to consider as belonging to this class differ from what I have termed self-murder in being associated with symptoms of loss of control, and from insane suicide in being uncombined with manifest disease of the understanding. The suicidal propensity is, in some instances, a form of insanity per se, a variety of moral insanity, and unaccompanied by incapacity of judgment; in other cases it only appears to be so, the intellectual disorder being either undisplayed or overlooked. The simplest example of pure suicidal impulse, uncomplicated with disordered judgment,—suicide from loss of control,—is the self-destruction which may be the result of *fascination*,—the feeling whose approach some of you possibly may have experienced in your own person in looking over a precipitous height. The natural,—that is to say, the healthy, impulse in such a position would be an excitement of the instinct of self-preservation; the opposed feeling, though perhaps common, is truly abnormal, and has, it is probable, a pathological source, and may be allied to that feeling of vertigo which is still more common in such a situation. This last is said to be a sensation developed only in the special circumstance of an immediate tangible connection with the point towards which the eye is directed: it is felt in looking from a precipice, but not from the car of a balloon: and a similar sensation attends looking up the face of a perpendicular cliff, or to the high roof in the interior of a cathedral, but does not interfere with one gazing on the sky. It would be interesting could it be known whether the suicidal impulse is ever developed by looking downward from a balloon. It may be said generally of suicide from suggestive opportunity, that it is a morbid phenomenon. It is the most producible of all the insane impulses. Were a convenient noose to be hung up in the sleeping apartments of a lunatic asylum, doubtless more than those who went to bed suicidally inclined would be found suspended in the morning. It is not so discreditably to the country that the gallery of the Monument was found necessary to be encaged, as that it was not so protected before that necessity was proved! Suicide from *imitation* is another kind of self-destruction consequent on diseased impulses without deranged intellect, although it has been proved capable of being prevented by disgracing the dead body. The Roman senate finding it necessary on one occasion to attempt the check of a suicidal epidemic amongst the fe-

males, succeeded by decreeing the exposure of the suicide's body naked in the public highway; and we are told by Dr. Burrows that the practice of suicide at one time, in a British regiment in Malta, was put a stop to by dragging the naked corpse on a hurdle ignominiously before the whole band, and throwing it into the fosse: but these efforts are no proof that the impulse was not a diseased one; true, self-murder could not be hindered by such means. "The morbid impulse is fettered not by the unaided free will of the individual, but by a strong mental impression tending to arouse the will to free action. Insanity, it has been said, may be prevented or controlled by voluntary exertion; but it will neither be the one nor the other without the agency of external stimulation; unless in the earliest steps of its progress, or the stages of its retrogression." The suicidal inclination is frequently a persistent suggestion rather than a sudden impulse; but is in such cases also a pathological symptom. Our natural instincts, even the strongest, may, in obedience to the higher motives that influence the mind, such as a sense of duty and religious obligation, be successfully resisted; but the annihilation of these instincts, and impulses *contra naturam*, are in every instance to be looked upon as morbid. The desire to terminate life is a psychical reflex of physical disease. "It may be laid down as an indisputable axiom," says one who has thought, written, and seen much of this state (Dr. Winslow), "that in every case of this kind bodily disease may on a careful examination be detected. We never yet saw a case where a desire to commit suicide was present in which there was not corporeal indisposition." In all the foregoing examples of what we have termed suicidal moral insanity the intellect is understood to be sound; but there are others in which this is at least problematical. In many instances, doubtless, the intellectual complication is concealed or overlooked; in others the deed has been the first and last act of the disordered judgment. The attempt at self-destruction is frequently the antecedent of insanity,—the main indication of that premonitory moral disorder to which I have alluded.

When is suicide to be considered as the act of insanity?—In answering this question you are to keep in view that in the word insanity are comprehended the five varieties of the disorder formerly discussed, one of which, moral insanity, is not yet recognised in law. You are to take into account the history of the individual, the mode in which the action was committed, and the condition of the body. Self-destruction ought to be held as insanity when it is the

deed of one hereditarily predisposed to mental disease, or to death by suicide, particularly if the predisposition is inherited from the parent of the same sex, as this gives increased force to congenital tendencies. Dr. Gall, Dr. Burrows, and other writers, have given striking examples of suicide occurring in three or four consecutive generations of a family, the result undoubtedly of inherited qualities, and not to be accounted for on the mere ground of suggestive example and vicious imitation. Self-destruction is to be deemed insanity also in all those cases in which the circumstances of the individual were not such as to develop any of those motives to which allusion was made in speaking of self-murder or *felo de se*; so likewise in every instance in which it is to be ascribed to mere imitation, or to the fascination of suggestive opportunity. The mode of committing the action may itself be indicative of disease. If little could be inferred from the single fact of a person hanging, drowning, poisoning, or pistolling himself, there could certainly be no doubt of the state of mind of one who chose rather to throw himself into a furnace, to starve himself to death, to crucify himself, or to kill himself by cutting off his genitals. I would also hold it as sufficient ground for a similar conclusion where it appeared, as formerly described, that the individual had not so much the desire of self-destruction as that of dying in a particular way; where, for example, he did not seem to wish to kill himself so much as to hang himself. Suicide amounts to proof of insanity when committed by people labouring under certain diseases: as pellagra,—an endemic skin disease in Lombardy, which, according to Professor Tomassini, and other Italian physicians, occasions often an irresistible impulse to self-annihilation,—hypochondriasis, paralysis, hysteria, epilepsy, extreme bodily pain, uterine derangement, spermatorrhoea, nostalgia, intoxication, &c., all of which are efficient causes of mental diseases, and many of them states which are either transitional to, or premonitory of, insanity. Any of those pathological appearances which are more peculiarly connected with mental alienation if found in the body of a suicide, ought to lead us to the inference that the individual laboured under mental disease; and indeed, in the present state of our knowledge, if we can distinctly make out that he suffered from bad health, the violent death should be ascribed to sympathetic cerebral disturbance. In short, suicide is presumptive of insanity, and ought to be held probative in every case in which the recognised causes of *self-murder* were not clearly present.

Self-murder is the rarest form of suicide; and doubtless many cases of supposed felonies are examples of obedience to some morbid impulse; all other suicides are probative of unsoundness of mind. Juries have been often twitted with the commonness of the verdict of "temporary insanity" in cases of self-destruction; but they are in the main right in their conduct and dealing with the matter in the spirit that is most philosophical. The practice of the law is, on the other hand, chargeable with being harsh and unenlightened. The attempt "to put down suicide," unless in the way of preventing its causes, is absurd in the extreme, when the deed is not the consequence of epidemic imitation; for the act is an unnatural one beyond the domain of legislation. The man bent upon death, whom social affections, conservative instinct, and religious principle do not sway, is beyond the influence of example and authority. He has reached an acme of misery not to be rendered more miserable by the prospect of post-obit vengeance, nor to be deterred from its morbid will by legal consequences often but ill understood, and in a multitude of cases impotent and inconsequential; and he appeals his case to a tribunal higher than all the united estates of humanity. Legislation should go no further than this: for the protection of heirs (not their robbery), to vitiate the will of a suicide when it is contrary to the legal order of succession; and for the protection of the public, to hold the intention of suicide when accidentally productive of death to others to be punishable as homicide, unless where insanity can be clearly established, in which case the individual should be confined for life.

The homicidal impulse.—The manifestation of this propensity is much more rare among the insane than the suicidal impulse is. The desire to kill was a feature more or less abiding in about 1 per cent. of the cases treated in the Aberdeen Lunatic Asylum. The insane may commit homicide under the influence of delusion, delirium, imbecility, where there is imperfect appreciation of consequences, or simply as a result of the moral perversion which accompanies the disease. The propensity is rather more common in the insanity of females than of males; it is most usually combined with epileptic mania.

Homicidal moral insanity may be employed as the term to designate those cases in which a morbid impulse to kill is the sole indication of diseased mind. Though much less common, this is a form of mental disorder as veritable as the suicidal moral insanity of which we have spoken; it is admitted by all recent writers, and has been

recognised in criminal courts. The propensity has been found combined with the conditions of menstruation, pregnancy, parturition, uterine derangement, gastric disorder, cerebral congestion, epilepsy, and hypertrophy of the heart, and may be persistent, intermittent, or occasional. These conditions are to be looked upon as predisposing physical causes; there may be also predisposing moral causes at work,—as, for example, grief and despondency; but the deed flows immediately from some mental impression by a morbid action so powerfully exciting the homicidal idea, as to give it an instinctive force, leading to an action which is irresistible,—independent of the will. The deed may be the result of imitation, the idea induced by murderous narratives, or suggested by the sight of lethal weapons, or by the exposed and defenceless condition of the victim. When the psycho-physical relation is once disturbed, whether by the influence of corporeal or of mental agencies depressing the vitality, and the predisposition to diseased manifestations thus induced, ordinary influences become exciting causes of extraordinary conduct. I have already said that mental impressions, often of a common kind, are the originators of delusion (incapacitated judgment); so are they also of irresistible impulses (incapacitated will). In the former case extravagant conduct results mediately from the delusion excited; in the latter it results immediately from the mental impression, which, being an emotional one, manifests itself in action without any intervention of intellectual disorder. The homicidal impulse, and the insane impulses in general, constitute, therefore, the simplest and most completely objective form of madness. Reil adopts the same view. He considers them to be forms of mania, and to be mania without any extraneous addition. He appears, in the following description of the paroxysm, to assimilate its phenomena to those of epilepsy. "The paroxysm," says he (I read from Feuchtersleben's *Psychology*), "generally begins with all kinds of corporeal phenomena: a pressure exists at the pit of the stomach; shuddering through the whole body (often commencing at the spine); the tongue is loaded; there is a sensation of burning heat in the bowels; great thirst and constipation: the heat rises upwards to the chest, neck, and head; this last aches; there is a singing and buzzing in the ears; the look becomes anxious: the patient says that the heat rises before his eyes; he often warns those about him, and entreats to be prevented from doing mischief; the countenance becomes flushed; the arteries of the neck and temples throb

violently: at length the excitement extends to the brain; and at this moment arises the blind irresistible impulse to murder, commit suicide, theft, arson, or some other outrageous act; just as epilepsy ensues when its preliminary *aura* has reached the brain. These somatic (corporeal) symptoms are, however, often wanting. The disease is permanent, though mostly periodical.*

In homicidal moral insanity there is no discoverable intellectual disorder antecedent or subsequent to the act. Several writers maintain, however, that there is disordered perception or delirium during the time that the deed is done. In such a paroxysm as that vividly described by Reil one would infer delirium in the climax. Without insisting on this feature in every case, I believe that very frequently the patient may not have a competent knowledge of circumstances at the time.

I shall not take up your time quoting examples of this variety of insanity, leaving it to yourselves to study the many cases which you will find recorded in medico-legal works. I have often seen the homicidal desire as a feature of derangement, but have met with only one or two instances; and these not of a striking kind, in which the impulse itself formed the sole mental disease. In one of these cases the subject was a young woman, of a good and cheerful disposition naturally, who acted as a servant to an invalid lady. Her health got disordered from the nature of the duties which she had to discharge. She lived in the most solitary way, was very rarely indeed out of doors, and had to submit to perpetual night watching, and the constant peevishness and selfish exactions of an invalid. Such were the predisposing causes of the malady. The propensity was engendered by reading to her mistress, night after night, from newspapers, details of those monstrosities of crime which are so usually to be found in their columns. She was at length seized with a morbid desire to strangle her mistress as she lay asleep. The impulse grew stronger and stronger, and feeling that it would become irresistible, she determined to save herself by flight. She left her situation, and, at her earnest solicitation, to be protected

from the consequences of her actions, was lodged in an asylum. This patient had not the melancholic temperament which is common in such cases, had no hysterical tendencies, but was robust, of a sanguine habit, cheerful, and well inclined. There was no disorder in the uterine functions; the only distinct symptoms of disease were gastric, accompanied with headache and restlessness at night. When removed to the hospital there was no manifestation of mental affection, save depression of spirits; and when her disordered health was rectified she was discharged, and has ever since kept well.

Homicide from insane impulse differs from murder, in being a diseased act, suddenly committed in blind obedience to an instinctive impulse, or to a desire which has gradually become irresistible. None of the ordinary motives of crime are discoverable: there was no antecedent enmity or quarrel, and there is no resulting advantage. The objects sacrificed are most likely either previously unknown to the patient and accidentally encountered, or they are those, as his wife and children, whom he has always regarded with the deepest affection. The act is one indicative of much mental and physical excitement; it is not slow-poisoning secretly carried out, but a bloody deed savagely done; there is probably not one wound, but many; not one ghastly corpse, but several. When the slaughter has satiated the desire, there is either terror and flight, grief and confession, or remorse and suicide; but as frequently no relief seems to follow the indulged impulse; in which case there is neither attempted concealment, surrender to justice, denial, nor remorse. There will often be evident physical disease, with indications of moral insanity, or perhaps even of approaching or overlooked intellectual disorder. None of the circumstances of the action necessarily indicate insanity; the most characteristic of them are of occasional occurrence in criminal homicide. The absence of all motive is presumptive, but is with difficulty distinguished from concealment of motive; and the dependence of the impulse on disease, with its independence of the will, may be indicated in the history of the homicide and his crime, but cannot be indubitably proved.

When ought homicide to be considered as the act of insanity?—In speaking of self-destruction, I stated that the act by itself is to be held as presumptive of insanity; but in regard to homicide the presumption is much more on the side of crime than of disease; the former runs contrary to natural instincts, whereas the latter results from normal impulses which the divine commandment indicates have to be re-

* In a case of insane impulse (suicidal and homicidal) which I lately saw with Dr. Christie, of Aberdeen, the existence of our *aura* as described by Reil was well marked. The strange feeling, described by the patient as a "heat," began in the lower extremities, progressing gradually upwards to the head. The impulse was not manifested merely when the paroxysm reached its height, but commenced at the same time with the *aura*, which produced an intense feeling of restlessness and wretchedness when at its climax.

strained. Every one has the capacity to kill in self-defence; but there are many whom the instinct of self-preservation would, in all circumstances, keep from self-destruction. Homicide is not to be held as a criminal action when it is committed by one labouring under any form of delirium, the consequence of disease, or naturally of such an imbecile condition as to have an imperfect knowledge of right and wrong, and an imperfect appreciation of consequences. It ought to be considered as an act of insanity when it flows directly from a delusion which overrides the judgment and directs the conduct; indeed, in every case in which insane delusions exist, the presumption is strong that the deed is an act of insanity, whether it can be directly traced to the impulse of delusion or not; for, as already insisted upon, the consequences of a delusion are not to be foreseen. When outrageous or extravagant conduct co-exists with delusion, they are probably connected with each other, although the relation may not be apparent. Homicide without any of the usual motives of murder ought to be esteemed an act of insanity when it is followed by evident derangement of mind; for the deed has, in such a case, it is to be presumed, been the consequence of the moral perversion, which you are aware is so often the initial symptom of lunacy. In cases in which no intellectual derangement is obvious, the action is still to be attributed to insanity when it appears to have been motiveless, the manifestation of a blind irresistible impulse, inconsistent with the natural tendencies of the individual, and indicating in its circumstances the temporary existence of delirious loss of control; when it is also the act of an individual labouring under some of those diseased states which are frequently associated with insanity, as cause, complication, or consequence,—who has a hereditary predisposition to disease of the mind, or who has at some former period actually been afflicted with the malady.

ON PARALYSIS OF THE SIXTH PAIR OF NERVES. BY M. E. L. EMILE BALIN D'HURTEBISE.

THE tuber annulare is the ordinary seat of lesion in paralysis of the sixth pair of nerves: in one case it may extend to the pyramidal bodies, in another it may reach the nerve itself.

The small extent of the origin, and consequently of the relations of the roots of the sixth pair, accounts for the comparative rarity of their affection.

The nerve may become the seat of disease in its course, but it is more frequently

affected by morbid conditions of parts with which it becomes related in its course—*e. g.* varicose ophthalmic veins, or aneurism of the artery, or disease of the bone.

Paralysis of the sixth pair is seldom the result of cold, or of lead-poisoning, and is in any case rarely met with except as complicated with paralysis of the third pair. It occurs more frequently on the left than on the right side, and in men more than in women. It is also common at the adult age.

The symptoms will vary according as the paralysis is simple, or complicated with paralysis of other nerves.

In simple cases two orders of phenomena are observed—the inward deviation of the eye, and impairment of vision. The inward deviation is constant, but may be more or less distinct; sometimes the whole of the cornea, sometimes only a portion thereof, disappearing at the inner angle of the orbit. The globe may be moved upwards or downwards, but not outwards. This condition remains the same if the eye be forcibly closed. The impairment of sight, with double vision, is also constant, and among the earliest symptoms. Objects are seen confusedly as well as double. If the sound eye be closed, one object will be perceived, but only indistinctly. When seen double, if the objects be superposed in vision, the lower is the image of the real object. When the double images are side by side, as when placed vertically, the image to the right side is the image of the real object when the left eye is affected, and *vice versa*. The impairment of vision is in all cases proportionate to the strabismus.

Complicated cases.—The accompanying paralysis of the third pair is seen in the falling of the upper lid, the dilatation and immobility of the pupil, the immobility and a certain amount of protrusion of the globe, with confused vision of a single image. The same phenomena, with superadded anaesthesia of the face, indicates the implication of the fifth pair. Accompanying paralysis of the seventh is also shown by its own symptoms. In all cases, simple or compound, headache is frequently present.

This form of disease may be distinguished from strabismus by the permanency of the deviation of the globe, while in the latter it is possible occasionally for a short time to evert the eye. The impairment of vision differs also in the two cases: in strabismus the object is seen singly, from the habitual employment of one eye; in the other case vision is always confused.

The treatment must in every case be governed by the etiology.—*L'Union Médicale.*

Original Communications.

REMARKS ON THE

VITAL STATISTICS OF THE
BOROUGH OF PLYMOUTH.

FOR THE YEAR ENDED ON THE 30TH OF
JUNE, 1850.

By Dr. W. HAMILTON.

THE close of the eventful year which has just passed away, and the appearance of the valuable tables of the vital statistics of the borough which, with

their accustomed zeal and ability, the registrars have, according to their annual custom, placed upon record for the information of posterity, call for more than a passing notice, and suggest much for grave and instructive reflection.

The first fact of importance presented to the eye on the most cursory inspection of these tables is the enormous excess of mortality they exhibit above that of any of the ten preceding years; amounting, in fact, to one-fifth of the aggregate amount. The following has been the annual mortality of the borough from the year 1839 to the present time:—

1840	1841	1842	1843	1844	1845	1846	1847	1848	1849	Sum.	Mean.	1850.	1850. Above the decennial mean.
618	843	829	1107	887	1022	749	1030	1148	1268	9,501	950.1	1,876	926.99 55 p. ct.

Of this decennial mortality, 2,201 deaths, or above 23 per cent. of the aggregate from all causes, resulted from epidemic or zymotic complaints. An examination of the tables for the past year shows, however, that, in the

brief space of twelve months only, this mortality amounted to 1,019, or nearly 46 per cent. of the aggregate decennial zymotic mortality, as will be seen by the following table of the zymotic mortality in each of the following years:—

1840	1841	1842	1843	1844	1845	1846	1847	1848	1849	Sum.	Mean.	1850.	1850. Above decennial mean.
59	130	185	400	190	219	98	157	305	458	2,201	220.1	1,019	799.363 p. cent.

What were the principal causes of this epidemic mortality will be seen in the next table;—

	18	41	1842	1843	1844	1845	1846	1847	1848	1849	Decennial.		1850.	1850 compared with the decennial mean.
											Sum.	Mean.		
Variola	1	1	46	47	63	25	9	—	18	177	387	38.7	2	36.7
Rubeola	1	43	1	17	18	41	15	46	36	19	238	23.8	20	3.8
Pertussis	8	14	72	29	26	24	17	2	65	37	294	29.4	15	14.4
Scarlatina	4	1	10	238	6	10	2	1	—	18	290	29	25	4
Cyn. Trachealis	4	7	5	2	4	12	5	5	1	12	57	5.7	17	11.3
Aphthae	1	—	2	1	1	1	1	3	2	1	13	1.3	3	1.7
Diarr. and Dysen.	7	28	11	27	27	25	25	56	52	46	304	30.4	55	24.6
CHOLERA	2	6	2	6	2	5	—	—	2	22	47	4.7	819	814.3
Influenza	3	1	1	—	1	2	1	—	40	—	49	4.9	3	1.9
Febris	24	22	28	27	30	52	16	30	79	111	419	41.9	49	7.1
Erysipelas	2	3	4	5	7	13	3	8	6	11	62	6.2	2	4.2
Zymotic	57	126	182	399	185	210	91	151	301	451	2160	216	1010	794
All causes	618	843	829	1107	887	749	1030	1148	1268	9501	950.1	1876	926.99	

We have here a frightful excess of mortality, in the year which has just closed its fatal career, above the mean of the ten preceding years; amounting, from all causes, to 326, or 97 per cent. above the decennial mean, and, from the eleven causes specified in the table, to 794, or nearly 368 per cent. above the decennial mean of the mortality from those eleven causes. The greatest mortality from any single cause was in 1843, when scarlatina swept off 236, or upwards of 11 per cent. of the aggregate decennial mortality from the eleven epidemics included in the table. The next greatest was in 1849, when variola carried off 177, or above 8 per cent. of the aggregate epidemic mortality. Diarrhoea and dysentery proved fatal in the whole period to but 304, or about 14 per cent; while CHOLERA swept away, in the comparatively insignificant space of 127 days, about 1-28th part of the ten previous years, 819, or above 85 per cent. of the aggregate mortality from all causes, and nearly 38 per cent. of the aggregate decennial mortality of the causes comprised in the table. Enormous, however, as we find the preponderance on the side of this single disease, in so brief a space of time, it appears to have had little or no influence upon the mortality arising from the more usual causes.

Dr. Soltan, of this town, has, in his instructive paper on Cholera, in the MEDICAL GAZETTE for last March, fallen into the popular error that it is "an axiom connected with the history of epidemics, that, whenever they prevail, they have a tendency to stamp every other disease with their own peculiar type, especially such as are from their nature allied to them;" and he cites scarlatina and sore-throat, influenza and bronchitis, as illustrations of the opinion. But upon consulting the table last given, it will be seen that diarrhoea and dysentery—disorders which betray a manifest connection with cholera—were not sensibly affected by the presence of their Leviathan relative, but actually exceeded the mean of the ten preceding years by 246, or above 8 per cent. And so far was cholera from absorbing into itself, as it were, all the ordinary causes of mortality, that we find, on clearing the aggregate mortality of the year from that resulting from this single cause, the balance which remains exceeds the average decennial mortality by no less than 107, or up-

wards of 10 per cent.; thus corroborating, in a most unanswerable manner, the observation made by Dr. Shapter, at the 27th page of his valuable "History of Cholera in Exeter in 1832," where he says,

"It has been a not unfrequent statement, and I believe it is an opinion commonly entertained—that, during the progress of plague, all other diseases subside. The above table of deaths that occurred in Exeter shows this not to have been the case during the prevalence of the cholera. The deaths from other causes were by no means few nor far between, and were rather a large proportion to those from cholera itself. If the few deaths that occurred after the 19th of September be excluded there were from cholera 336, and from other causes 90. This latter number was scarcely less than the average mortality, which was about 50 deaths per month. So that it would appear the deaths from cholera were entirely superadded to what may perhaps be termed an otherwise not fatal season. Recent careful investigation has shown this to be the case elsewhere; and there is now reason for believing that the above opinion is founded on popular error—an error probably due to the all-absorbing topic, during the prevalence of a plague, of the plague itself, and to the habit of the public mind to assume, without any very intimate inquiry, that all the deaths which take place are from the prevailing cause—in fact, in common parlance, to die at the season of the plague is to die of the plague." This opinion is still farther confirmed by a comparison between the mortality from zymotic and that from all causes during each of the eleven years which have just expired, as in the following table, from which it may be seen that, while the deaths from epidemics in 1840 amounted only to 95 per cent. of the aggregate mortality of the year, they reached in 1850 nearly to 54 per cent. (see next page.)

We learn from this table that, while the centesimal proportion of the aggregate zymotic mortality of the entire period amounts to but 28 per cent. of the aggregate mortality from all causes, this proportion is more than doubled when the zymotic mortality of 1850 is compared with the aggregate mortality of that year from all causes. And even rejecting the surplus mortality of 819 resulting from cholera alone, a balance

Mortality from Epidemics centesimally compared with that from all Causes during the eleven last years.

	1840.	1841.	1842.	1843.	1844.	1845.	1846.	1847.	1848.	1849.	1850.	Mortality in eleven years.	
												Sum.	Mean.
All causes	618	846	829	1107	887	1022	749	1030	1148	1268	1876	11,377	1,034.2
Zymotic .	59	130	185	400	190	219	98	157	303	458	1019	3,220	292.7
Difference	559	713	644	707	697	803	651	873	843	810	857	8,157	741.6
Cent. prop. of zymotic	9.73	15.42	22.42	36.1	21.42	21.43	13.0	15.24	26.56	36.12	54.3	28.3	2.83

of 200 deaths from other epidemics, or upwards of 10.75 per cent. of the aggregate mortality of the year, still remains. Thus clearly demonstrating the truth of Dr. Shapter's observation, that cholera was, to employ a commercial expression, a bonus thrown into the general mortality of the year.

In examining into the influence of age, Dr. Shapter appears to be somewhat in error when he says:—"Another and very general error exists, that cholera chiefly attacks the young and those of the middle ages of life, leaving exempt from its influence infancy and old age. This has arisen from an imperfect manner of viewing the subject. When the deaths resulting from cholera at each period of life are compared with the numbers living at the same period (and which is the only true mode of arriving at a sound deduction upon this point), it will be found that, during infancy, its ratio is rather above that which takes place between the ages of ten and thirty-five, when the deaths are by no means numerous; and that, after the age of thirty-five, there is a considerable increase in the relative amount of mortality; but that its greatest proportionate amount takes place about sixty-five years of age." And in support of this opinion, he gives a table of the "Deaths from Cholera in Exeter in 1832," distinguishing the ages and the duration of the attack.

But if the object of the inquiry be, as I have proposed, not simply into the fatality of cholera at different ages compared with itself, but into its fatality at those ages as measured by that of all other sources of death, Dr. Shapter's table furnishes no adequate data for instituting the comparison, and cannot enter into competition for a moment with the elaborate table given by Mr. Wyatt in the statistics before me,

deduced from official materials which Dr. Shapter unfortunately could not command.

That the mortality in the first ten years of life is, under all circumstances, greater than that of mature age, is a fact known to all who have made the statistics of life their study; and, consequently, that cholera should, in this respect, conform to the general rule, is only what might have been anticipated; and it was not my object to demonstrate by fresh evidence a statistical axiom.

In the supplementary statistics inserted in the *MEDICAL GAZETTE* of the 18th of January last I have said, at p. 105:—

"That, while the centesimal proportion of deaths from cholera falls short of the mortality from all causes by nearly 17½ per cent. under 10 years, and by about 11.17 per cent. between 60 and 100,—from 10 to 20, when youth is advancing to manhood, and beginning to be a profitable member of society, the mortality from cholera exceeds that from all causes during the eight preceding years by 3.95 per cent.; and during the meridian of life, from 20 to 60, when the individual, attaining the full maturity of his faculties, mental and corporeal, has become an important constituent of society—as the head, perhaps, of a numerous family dependent on his labours for their daily subsistence—the excess of mortality from cholera amounted to no less than 20.35 per cent. This is an important feature in the character of cholera, in Plymouth at least, which, if confirmed by experience elsewhere, will go far to correct the general belief of infancy and age furnishing the larger proportion of victims—a belief shared, as I am informed, by Dr. Shapter, who states it (unless I am misinformed) as a fact, in his history of the epidemic at Exeter.

That the popular opinion is incorrect, as far as Plymouth is concerned, the tables just given abundantly prove, furnishing matter for grave and melancholy reflection to the political economist no less than the philanthropist. Eight hundred and nineteen of those classes that earn their daily bread by the sweat of their brow and the toil of their hands have been swept from the face of the earth, of which number more than half were in the enjoyment of life, full of health, strength, and vigour, able to contribute their quota to the welfare of society; while of those who were dependent upon them for support, by far the smaller proportion was taken.

From the following re-arrangement of the table given by Dr. Shapter at page

1st period—under 10 yrs.				2d period—10 to 25 years.				3d period—25 to 64 years.				4th period—65 to 100 years.			
Deaths under	M.	Fem.	Total	Deaths between	Males.	Fem.	Total	Deaths between	M.	Fem.	Total	Deaths between	M.	Fem.	Total
1 year	7	5	12	10 & 15	11	9	20	35 and 45	30	32	62	65 and 75	16	22	38
1 to 5	24	19	43	15 & 25	14	20	34	45 and 55	23	22	45	Above 75	6	15	21
5 to 10	22	15	37	25 & 35	19	30	49	55 and 65	18	22	40	Not specified	2	1	3
Sum.	53	39	92	Sum.	44	59	103	Sum.	71	76	147	Sum.	24	38	62
Cent. prop.	57.6	42.4	100	Cent. prop.	42.7	57.3	100	Cent. prop.	48.3	51.7	100	Cent. prop.	38.7	61.3	100

219 of his work, so as to compare the deaths of each sex with the aggregate number of both at each of the four periods of life indicated in the passage I have extracted, it will be found that the greatest centesimal amount of mortality is, as might have been expected, on the side of the males in the first, and on that of the females in all the rest.

In a letter of the 25th of January last, Dr. Shapter asks—

“Have you not erred in stating the relative amount of deaths of the two sexes, and at the different ages, by a mere comparison with each other, instead of comparing them with the population of each sex, and the numbers living at each age of life; without taking these elements into consideration the deductions cannot be accurate: your deaths are—males, 394; females, 425. It may be that the proportion of female deaths is really less than that of males. In order to arrive at the true proposition you must compare each with the proportion of each sex comprising the population of 39,571. Your paper does not give this. Again, there is the same necessity for comparing the deaths at each age with the living at each age.”

The tables given in my last paper having been merely transcripts of those given by the registrars in their most important sheet of the statistics of cholera, I might fairly, perhaps, disclaim all responsibility for the facts they contain and the conclusions they suggest; more especially as I was not in possession of the official materials from which they were constructed. Having incorporated them, however, with my paper, and made them the basis of the opinions I advanced, I assumed to myself the responsibility of their correctness, and identified myself with those from whom they originated; I was therefore called upon to prove their correctness or admit their inaccuracy.

To do the former in the manner required by Dr. Shapter, upon the eve of a fresh enumeration of the population, and when an interval of eight years had nearly obliterated from the chart of time all the landmarks fixed in 1841, presented a barrier of difficulties I knew not how to surmount; while my conviction of the truth of my deductions forbade the adoption of the latter alternative. In this fix, as our transatlantic brethren would not unwarily term it, the genius and perseverance of Mr. Wyatt

happily came to my relief, and extricated me from my dilemma; convincing me that the arguments based upon his figures were not erroneous, although the inquiry had been conducted in a manner differing from that proposed by Dr. Shapter. This difference gave a certain degree of incompleteness to my conclusions without affecting their general truth.

"It may," as he justly observed, "be very desirable to compare the number of each age and sex that fell victims to cholera centesimally with the numbers actually living at its commencement: but such an examination, unless made conjointly with a similar inquiry into the mortality from all causes, would be equally incomplete; for it would be clearly as inconclusive and unsatisfactory to learn that cholera had swept from the stage of life any centesimal proportion of this or that sex or age living at the time, as to learn that such a proportion had been subtracted from the entire population of the town, irrespective of the results of a similar reduction from causes in permanent operation." This double inquiry has been prosecuted in the fifth of the tables

published by the registrars in their admirable synopsis of last year.

The object of that table was to exhibit in a more striking point of view the peculiarities of cholera in its effect upon the four principal stages of life, on a centesimal comparison with those of the ordinary causes of mortality, for the purpose of illustrating the curious fact that—while other epidemics, as variola, rubeola, pertussis, scarlatina, &c. limit their ravages chiefly to the young—cholera was most fatal in the two intermediate periods of existence, aiming its destructive shafts in a more especial manner at the young and vigorous, overthrowing, as was observed in my last paper, "the pillars of the social fabric," which it will take another generation to replace, and leaving comparatively intact the helplessness of infancy and the decrepitude of age.

To meet the views of Dr. Shapter, and to establish this peculiarity of cholera on an unquestionable foundation, Mr. Wyatt, at my request, compiled the following table, which he added to the annual sheet of statistics published at the close of last year:—

Mortality from Cholera during the summer of 1849, at different ages, with the centesimal proportion to the estimated numbers living, compared with the mortality from all causes during the eight preceding years.

Under the population age of	Estimated at each on the 1st of July.	MORTALITY FROM				
		All causes in 8 years.			Cholera in 127 days.	
		Total deaths.	Relative number.	Centesimal proportion.	Total deaths.	Centesimal proportion.
10 years	9,112	3,698	377	4.10	269	2.959
20	7,758	330	33	.43	66	.86
30	6,510	525	54	.82	91	1.40
40	5,966	515	52	.87	113	1.80
50	4,219	468	48	1.10	95	2.20
60	2,875	548	55	1.90	77	2.60
70	1,849	709	73	3.90	65	3.50
80	841	768	78	9.20	34	4.
90	325	418	48	12.90	6	1.80
100	116	63	6	5.20	3	2.60
Sum .	39,571	8,040	819		819	
Males .	17,516	3,965	404	2.30	394	2.24
Females	22,055	4,075	415	1.88	425	1.92

In this table the mortality from cholera at the different decennial periods of life is compared, not with the number

actually living at each period, but with a number of deaths at that age from all causes, proportionate to the total mor-

tality from cholera. This proportionate, or, as Mr. Wyatt terms it, relative number, is obtained by taking the proportion between the total number of deaths from all causes, the number at that particular age, and a number equivalent to the total mortality from cholera. Thus the total mortality from all causes was 8,040, and the total under the age of 10, 3,968. Then, as 8,040 : 3,968 :: 819 : 377, and a minute fraction which is rejected as unimportant. This number being thus obtained, the centesimal proportion to the number living is easily found as follows:—As 9,112 : 877 :: 100 : 4·10. And the centesimal mortality from cholera is similarly found—As 9,112 : 269 :: 100 : 2·959, or nearly three per cent.

From the table thus constructed we learn that, while the mortality from all causes under ten years exceeded four per cent., that from cholera under those years fell short of three per cent. But passing this mark in the calendar of

time, we find the balance progressively inclining to the opposite direction, until, at the age of 60, the mortality from cholera actually doubles that from all other causes during a period of nearly twenty-four times its actual duration, and that this preponderance of mortality from cholera continues up to the age of 80, when that from all causes resumes its normal proportion.

To place all this, however, in a still more prominent point of view, I shall here insert a table arranged upon the principle of that formerly given at page 105 of the MEDICAL GAZETTE for last March, in juxtaposition with that table, which I gladly reproduce in order to correct an error which crept in in the former, where the centesimal proportion of the mortality from cholera (13·18) is represented in the column of centesimal decrease for 45·87, the true amount, through some unaccountable inadvertence.

Age.	Centesimal proportion of deaths from						Estimated population at each period of life.	Centesimal proportion of deaths from						
	All causes in the last 8 yrs.		Cholera in 1849.		Centesimal amount of			All causes during the last 8 years.		Cholera in 1849, in the space of 127 days.		Centesimal prop.		
	No.	Centes. proport.	No.	Centes. prop.	In-crease.	De-crease.		No.	Relative prop.	Centesimal prop.	No.		Incr.	Decr.
Under 10	3698	45·995	269	32·84		28·59	9,112	9698	377	4·10	269	2·9		27·6
10 to 20	330	4·10	66	8·05	96·34		7,758	330	33	4·8	66	84	100	
20 to 60	2054	25·547	376	45·90	79·71		19,570	2054	209	1·	376	1·9	79	
60 to 100	1958	24·35	108	13·18		45·87	3,121	1958	200	6·3	108	3·4		45·7
Sum	8040		819				39,571	8400	819		819			

The first of these is the same (corrected) which has already appeared, and the second is taken from that just given, for the purpose of showing the close correspondence of the results, by whatever method they have been obtained; the comparison in both being between cholera and all other causes of mortality.

With respect to the influence of sex, Dr. Shapter expresses an opinion that "it does not appear to be very considerable." He gives a table, however, which exhibits a small centesimal excess of mortality on the part of the males, upon which he remarks that, "From the following table, in which the relative intensity and proportion of

the duration of the disease, and of death proceeding from it, are contrasted, it would appear that males are rather more obnoxious to its influence than females, a rather larger proportion of these having died (1·54 per cent. males, and 1·33 per cent. females), and at an earlier period of the attack."

At the time I transmitted my last paper to the MEDICAL GAZETTE, I was led to imagine the reverse to have been the case; but it will be seen by the last table but one that this was erroneous, the male exceeding the female mortality by 0·32 per cent, or 0·11 per cent. beyond Dr. Shapter's experience—a sufficiently close correspondence for results obtained at such a distance of time and

place. Since then I have been favoured by Mr. Wyatt with a table showing, as nearly as possible, the influence of cholera on each sex, in each of the four stages of existence which have been already employed for the purpose of comparison.

Age.	Sexes.	Estimated amount of population.	Amount of mortality resulting from cholera.	
			No.	Centesimal proportion.
Under 10 years.	Males	4,038	187	3.3
	Fem.	5,074	132	2.5
	Total	9,112	269	2.9
10 to 20	Males	3,432	22	.65
	Fem.	4,326	44	1.02
	Total	7,758	46	.86
20 to 60	Males	8,660	186	2.1
	Fem.	10,910	190	1.7
	Total	19,570	376	1.9
60 to 100	Males	1,886	49	3.5
	Fem.	1,745	59	3.3
	Total	3,181	108	3.4

We learn from this table that, while females have been the greatest sufferers, to an extent of above a quarter per cent., from cholera, between the ages of 10 and 20, at all other periods of life the preponderance was against the males, except in the more advanced periods between 60 and 100, when the fatality to both sexes approaches to within 0.2 per cent. of a perfect equality.

A similar fact has been observed in the ravages of cholera in India, whence it appears to have extended to Europe. In the narrative of a residence of four and twenty years in that country, which has proceeded within the last few months from the pen of a lady of the most masculine mind, highly cultivated understanding, and critically refined taste, published by Pelham Richardson, of Cornhill, I find it stated, at p. 283 of the first volume, that—

“Out of sixty deaths”—from cholera—“there will be forty Hindoos to twenty Mussulmans: more men are carried off than women—eight men to two women. The Mussulmans eat more nourishing food than the Hindoos, and the women are less exposed to the sun than the men.”

Thus we see that, while with us the amount of advantage enjoyed by the female portion of the community fell short of 1 per cent., it reached to 400 per cent. in India,—a difference perfectly intelligible to those who know anything of the difference of habits between the inhabitants of warm and cold climates. And this, too, will serve to account for the greater mortality in the middle periods of life, when the active employments of men expose them in a far greater degree than those at a more or less advanced period, or those of the less robust sex, to the proximate causes of the complaint. And hence it is that cholera inflicted a deeper and more permanent injury upon society than any of the more common and intelligible forms of disease, sweeping away with remorseless fury those who were its most useful members, who were in the fullest possession of their mental and physical faculties, and leaving comparatively unharmed those in whom those faculties were but in the infancy of development, or the impotence of decay.

In another respect cholera presents features which strikingly distinguish it from the general sources of mortality, and point to another form of injury inflicted by it upon society—namely, its almost total disregard of the state of gestation in females, which in almost every other disease arrests the progress of morbid action until the maternal functions have been fully completed, and the primary object of our nature accomplished. In Exeter, indeed, it appears to have made less difference than in Plymouth, and to have spared indiscriminately as many as it destroyed. On this point Dr. Shapter observes, at p. 221, that—

“With regard to the influence of the peculiarities of the female condition it may be mentioned that several cases occurred during the advanced stages of pregnancy, but that this, notwithstanding its severer symptoms were present, did not in any way affect the progress of the disease. Of two cases, between the eighth and ninth months, in both of which there were rigors, cramps, serous discharges, &c., one died, and one recovered.

“From the above it may, therefore, be generally concluded that neither age nor sex appear to exert any very great influence on the duration or virulence of the disease; irrespective of these its

course is rapid, and almost ephemeral. Unlike most epidemics, which single out for their victims persons at particular periods of life, it is, in fact, a general disease, and obeys, in the proportional amount of its mortality, the laws which govern the mortality of mankind: but then this mortality is crowded into a narrow and conspicuous place."

Conceiving that an examination of the annual and quarterly returns of births during the last eleven years would be the most certain method of obtaining light upon this important branch of the subject, I have collected these returns from the annual statistics of the registrars into the following table:—

Quarterly Births of both Sexes during the eleven years ended on the 30th of June, 1850.

Years.	Summer quarter ended 30th Sept.			Autumn quarter ended 31st Dec.			Winter quarter ended 31st March.			Spring quarter ended 30th June.			YEAR.			Years.
	M.	F.	Total	M.	F.	Total	M.	F.	Total	M.	F.	Total	M.	F.	Total	
1840	136	129	265	134	114	248	141	153	294	121	121	242	532	517	1049	1840
1841	133	118	251	128	181	269	144	143	287	135	140	275	540	532	1072	1841
1842	132	136	268	160	184	294	135	128	263	163	157	320	590	555	1145	1842
1843	132	158	290	172	154	326	163	161	324	152	135	287	619	608	1227	1843
1844	145	157	302	178	185	363	169	169	338	168	172	340	660	683	1343	1844
1845	161	176	337	201	177	378	185	155	340	177	168	340	724	671	1395	1845
1846	189	156	315	176	164	340	201	194	395	196	176	372	732	690	1422	1846
1847	174	169	343	187	170	357	182	178	360	184	177	361	727	694	1421	1847
1848	155	149	304	182	178	360	228	200	428	197	198	395	762	725	1487	1848
1849	166	183	349	207	184	391	245	204	449	237	218	455	855	789	1644	1849
1850	174	180	354	215	196	411	206	203	409	187	210	397	782	789	1571	1850
Sum.	1667	1711	3378	1940	1787	3727	1999	1888	3887	1917	1867	3784	7523	7253	14776	
Mn.	151	156	307	177	162	339	182	171	353	174	170	344	684	659	1343	

Hence we see that the number of births during the last eleven years was 14,776, giving an annual mean of 1,343, or an excess of 294 above the number born in 1840. The mean annual increase of births during the whole period may be taken at 47·5; but for the first ten years of the series it was 59·5. Had it not been for the reduction of the population in 1849-50, this rate of increase would, in all human probability, not only have been maintained, but increased. Assuming, however, only the average rate for the preceding ten years—namely, 59·5, or, in round numbers, 60, and adding this to the 1,644 born in 1848-9, we shall have 1,704 for the number of births that might have been reasonably counted upon for 1849-50; in place of which the number actually registered was only 1571, or 133 less than what it should have been, had the previous rate of increase only been maintained.

Taking the births for the year at 1704, the proportionate number for three quarters, or nine months, the ordinary term of gestation, should have been

1278; and rejecting the surplus 8 for twins, we shall have 1270 for the number of women whose gestation was more or less advanced when cholera commenced its slaughter on the 4th of July. But, as between the 1st of July, 1849, and the 31st of March, 1850, only 1,174 births were registered, the deficiency of 96 must be accounted for on the presumption that an equal number of those who should have been the mothers of those deficient children fell victims during the epidemic,—a number of deaths during pregnancy which is certainly remarkable when compared with the results of other epidemics, whose progress is generally stayed till after delivery. The age in which child-bearing most frequently occurs is from 18 to 50; and the number of females estimated to have been living between these limits at the commencement of cholera was 10,160, of whom 167 died, or 1·64 per cent.

That this reasoning, though based in a considerable degree upon hypothesis, is not in truth very wide of the mark, will be apparent from an examination

of the question in another point of view, which will present us with nearly an identical result.

On examining the table more minutely, we observe that of the 14,776 births which took place during the eleven years, 7,105 belong to the summer and autumnal quarters, and 7,671 to the remaining two; giving a mean of 646 for the two former, and of 697 for the latter; and for each quarter of the two first 318, and for each of the two last of 344. During the first of these two quarters in 1849-50, it will be remembered that cholera was raging in all its most destructive intensity; yet we find that in the first of these, comprising July, August, and September, which yielded 764, or above 60·28 per cent. of the 819 forming the aggregate harvest of the cholera mortality, the births, 354 in number, instead of being diminished by this fearful amount of mortality, and falling, as might have been expected, below the mean, actually exceeded it by 36, or upwards of 11 per cent. And in the autumn quarter, in which but 55 deaths from cholera occurred, this excess above the mean, 344, is nearly doubled, amounting to 67, or nearly 19·5 per cent. Hence it is evident that cholera had been unable, during either the meridian or the evening of its power, to make any sensible impression, or, indeed, check in the very slightest degree the onward march of population, even although, as we have just seen, nearly 2 per cent. of the female portion of the community, between the ages of 18 and 50, were its victims.

No sooner, however, had the labours of the destroyer ceased,—no sooner had society begun to recover from its state of alarm and prostration,—and no sooner had hope begun to whisper that the void created by cholera would be speedily filled up by the constantly increasing addition of births,—than we find an unexpected check given to this obliterating movement, and that, although the enemy had retired from the field, the effects of his past presence continued to be felt. Although, on comparison with the averages, an increase continues to be still perceptible in these quarters; yet, on comparison with the corresponding quarters of the preceding year, we cannot but be struck with the progressive falling off.

[To be continued.]

OF IRREGULAR CONTRACTION OF THE UTERUS.

By JAMES KIRK, M.C. GLASGOW.

If, after tying the cord and separating the child from its mother, we lay the hand over the abdomen of the latter, we will generally find the uterus lying between the umbilicus and the pubis, in the form of a solid globular body about the size of the foetal head; indicating that the uterus has contracted regularly, and that the placenta may be expected to come speedily away.

But it sometimes happens that in place of this state of matters we find one very different, namely, that the uterus instead of a globular has assumed a distinctly oblong or other abnormal shape: when this is the case, we may be certain that the uterus has contracted irregularly, and that the placenta will be retained as a consequence of this irregular contraction.

We find in actual practice that the placenta may be retained by various irregular contractions of the uterus; the following being those most commonly met with. 1st. That in which the uterus contracts firmly around the placenta, inclosing it wholly within its cavity. 2d. That in which there is a small hour-glass contraction high up. 3d. That in which there is true hour-glass action of the middle circular, or transverse fibres of the uterus, while those of the neck and fundus are uncontracted. In the first of these states, the uterus feels hard and much larger than natural when the hand is laid over the abdomen; in the second and third it has a distinctly oblong feel, sometimes presenting a ridge rising up in the middle like a sow's back. We do not recollect to have met with flooding either in the first or second variety, but always in the third.

Causes.—The causes which we have observed to predispose to irregular contraction are—1st. a very rapid delivery. 2d. severe and protracted labour; thus we see it oftenest in a first case. 3d. mental anxiety; we find it often in women whose children have been born in the absence of the medical attendant, and in young unmarried women. 4th.

we have known it to be hereditary. Dragging at the cord has also been said to give rise to it, but we have never known this to be a cause of irregular contraction.

Symptoms.—The symptoms are the oblong shape of the uterus already mentioned, and retention of the placenta with or without hemorrhage; the insertion of the cord is not felt on examination per vaginam.

Treatment.—If there is no hæmorrhage there can be no harm in waiting an hour, and making gentle friction over the uterus, particularly as patients always complain of pain on the introduction of the hand into the vagina; but if there is flooding we must immediately give 60 drops of laudanum in water, and introduce the right hand into the vagina, using the cord as guide to the placenta; then lay the left hand over the abdomen to steady the uterus; next carry the right hand up to the stricture, which is to be overcome by a gentle boring motion; then separate the adhering part of the placenta carefully with the fingers: the uterus begins to contract regularly, the placenta is removed with the hand, a bandage is applied, and the danger is over.

We are aware that it has been recommended by Gooch to keep the hand in the cavity of the uterus until it contract and expel the placenta and hand together; but we have never kept the hand longer in the uterus than till we felt it beginning to contract; and have never seen cause to alter the practice.

Perhaps the following outlines of cases which have occurred to me during the last three years may serve to set this matter in a clearer light than anything further we can say on the subject.

CASE I. Aug. 29, 1848.—Past eight in the evening, was called to attend a young unmarried woman, who had given birth to a child (her first) in the absence of all assistance at 6 P.M.: on arriving found her eyes glazed, the feather bed wet through, and cord cut close to the external parts. I immediately introduced the hand into the vagina, carried it into the uterus, and found a stricture nearly dilated, allowing it to pass easily, and the placenta adherent beyond it. Detached the placenta and brought it away. The uterus contracted, and there was no more hæmorrhage; then gave plenty of spirit, both undiluted and in toddy: she

rallied a little immediately after the placenta was removed, spoke, said she had no pain, but felt weak; swallowed freely, but presently vomited and began to sink. I immediately removed the bandage from the abdomen to examine the state of the uterus, but it had slipped down into the cavity of the pelvis: uncovered the external parts, but there was no hæmorrhage: she continued to sink, and died in about 15 minutes after my arrival. Very unfortunately I had no laudanum with me, and as the messenger had a little distance to go, she was expiring before it could be procured. This woman's mother had been, and her sisters are, when in labour, subject to hour-glass contraction.

This case may be considered as a fair example of the way in which hour-glass contraction is likely to terminate when left to itself; or when assistance is called in too late.

CASE II. July 8, 1849.—Ten in the morning, was called to attend a young unmarried woman in her first confinement; there was nothing at all unusual in the case except that the edge of the perineum gave way under my fingers as the child's head was passing over it; the placenta followed speedily, and I thought the case was over; but there was some hæmorrhage afterwards, which I at first supposed to come from the small rent in the perineum, and accordingly bathed it with spirit of turpentine (recommended as a styptic long ago by Yonge); but the hæmorrhage still continuing, and the uterus having the oblong shape when felt through the wall of the abdomen, I gave 60 drops of laudanum, and introduced the hand into the vagina, thence into the uterus, and found an hour-glass contraction about half way up: on getting through this I found a large clot of blood in the upper chamber, and brought it away with the hand. The uterus contracted regularly: a bandage was applied, and the patient made a good recovery.

This case shews that irregular contraction of the uterus may take place after the expulsion of the placenta as well as before it.

CASE III. Sept. 24, 1849.—At seven in the morning was called to attend a married woman living at a considerable distance, in her fourth confinement

Attended immediately, and waited for some time; but as her previous labour had been long and tedious, and there was no prospect of a speedy termination to the present; I left, giving directions to send for me when she became worse. After coming home I was unavoidably detained longer than I intended: no message was sent to hasten me, and before getting back the child had been born for some time. The placenta was retained, but there was neither hour-glass contraction nor hæmorrhage. The uterus felt large and globular: learnt on inquiry that the pains had suddenly become so strong and powerful that three or four had sufficed to expel the child; the uterus had then contracted around the placenta, in which state I found it on my return. After employing friction for some time with very little effect, removed the placenta with the hand, and the patient had a good recovery.

In this case there was no laudanum given, as the patient did not seem to require it. I had attended this woman in her former confinement, when she had neither irregular contraction of the uterus nor retention of the placenta.

CASE IV. March 9, 1850. — Was called to attend a married woman in labour; but as I had a considerable distance to go, and the patient had been late in sending, the child was born before my arrival; although I had gone on the first summons. The placenta had not come away. There was no hæmorrhage, but the uterus had not the proper globular shape: waited about an hour, and employed friction, but in vain. I then gave 60 drops of laudanum, introduced the hand, and removed the placenta. There was a small hour-glass contraction high up: the case ended favourably. I had attended this woman in her previous confinement, but not having so far to go was present at the birth of the child, and she had no hour-glass contraction.

CASE V. July 17, 1850. — Was called to attend a young married woman in her first confinement: she had been ill a considerable time before I saw her: labour lingering and tedious: lips of os uteri thin and sharp like the edge of a knife: for this she had an enema of starch and laudanum, as recommended by Dr. Lever, with the best effect; the character of the pains became immediately altered for the better, and the

child was born not long after; but the placenta was retained, and the uterus had not the proper globular shape; there was no hæmorrhage. After waiting nearly an hour and using friction I introduced the hand, and found a small hour-glass contraction high up, with the placenta adhering by the membranes only; removed it, and gave 60 drops of laudanum; the patient made a good recovery.

In conclusion we may state, that though we have seen many cases of irregular contraction with retained placenta, we never knew of any that did not terminate favourably except the one mentioned in this paper; and that we are inclined to think that such cases owe their immunity from after bad consequences to the large dose of laudanum given at the time of the operation; which seems to act by preventing the nerves of the uterus from resenting the injury offered to it: or, in the words of Dr. Billing, by allaying the morbid sensibility, which occurs after parturition.

Glasgow, August 28, 1850.

MORBID APPEARANCES MET WITH IN FATAL CASES OF UTERINA AT PHILADELPHIA.

The following report has been drawn up by a Committee of the College of Physicians appointed for this purpose:—

"1. In the recent subject, the peritoneal coat, like all the serous membranes, was in all remarkably dry. The lubricating serosity was deficient in the serous membranes. 2. The epithelial layer of the intestinal mucous membrane was in all the specimens either entirely removed; or was detached, adhering loosely as a pulpy layer mixed with mucus or an albuminous substance. 3. Peyer's glands were developed to a greater or less extent in all the cases examined. 4. The solitary glands were also developed, and contained in the recent subject a minute quantity of white substance. 5. The villi were denuded of their covering, but unchanged in other respects. 6. The capillaries were entire, and manifested no departure from their normal state."

In addition to the above, according to Dr. Neill, who conducted the investigations, the proper tissue of the liver was exsanguineous, but the large blood-vessels were gorged with blood. This was also the condition of all the large blood-vessels of the abdominal viscera. — *American Journal of Medical Sciences for July, 1850.*

A CASE OF
VENTRAL EXTRA-UTERINE PREG-
NANCY.

By H. M. HUGHES, M.D.
Assistant Physician to Guy's Hospital.

(From Notes by Mr. Kingsford).

WITH A DETAILED ACCOUNT OF THE POST-
MORTEM INSPECTION.

By JOHN BIRKETT,
Assistant Surgeon to Guy's Hospital.

C— F—, aged 26, was admitted into Mary Ward, Guy's Hospital, March 14th, 1849. She stated that she had been married* five years, but had never been pregnant; that the catamenia appeared at the age of 14; that they had been constantly regular at the monthly periods up to five months ago; had usually been rather scanty, and lasted about three days; and that she had generally been more or less troubled with leucorrhœa.

She was a well-formed woman, of middle stature, and of light complexion; had been always actively employed, and generally enjoyed good health, till two and a half years ago, when she was under the care of Dr. Bird for simple continued fever; since her recovery from which she had remained well up to the commencement of her present ailments.

Five months since the catamenia ceased, and about two months afterwards she observed a hard swelling in the right inguinal region. This swelling increased in size, and gradually extended over the whole hypogastrium. The mammae at the same time began to enlarge, and the areolæ to become darker than natural in colour; but she had not then, nor had she at the time of her admission, the most remote notion that she was pregnant. She continued pretty well in health, the tumor continuing constantly to increase in size, till six weeks ago, when she suddenly fainted, and upon her recovery was affected with severe pains in the lower part of the abdomen. The pains varied in intensity, but never entirely

ceased. From that time a watery discharge had constantly issued from the vagina in greater or less quantity. She also complained of bearing down, and irritability of the bladder and rectum, experiencing a frequent desire to pass her urine, and a cutting pain in the act of micturition, together with a costive state of the bowels, nausea, and loss of appetite, since the attack of syncope. She had, in consequence of her pains, which produced sleepless nights, consulted three medical practitioners; but as they all differed in opinion as to the cause of her complaint,—one stating that she was pregnant; another that she had a fever; and the third that she was suffering from inflammation of the womb,—she determined to obtain admission into a hospital.

Upon her being placed in bed, the countenance was found pallid and distressed; the tongue clean, pale, moist, and indented by the teeth; the skin natural; and the pulse small, frequent, and rather feeble. An irregularly nodulated hard tumor existed in the hypogastric region. At some parts this tumor was tender upon pressure: it extended transversely across the lower part of the abdomen, but was rather more prominent upon the right side than upon the left side. Upon examination *per vaginam*, the uterus could be felt distinctly separate from the tumor, quite moveable, somewhat enlarged, and rather flabby, and unnaturally turned aside from its normal position. A tumor about the size of a chestnut was observed in the left inguinal region: it appeared soft, moveable, and altered by pressure, and was probably situated in the integuments.

The mammae were enlarged and firm, with a dark brown areolæ, about the size of a half-crown piece, and with enlarged glandules. They presented, as was observed by one of the physician-accoucheurs, the appearance represented by the words of an old author, "*quasi inflata*."

March 15th.—After further examination, it was considered probable that, although the fetal heart could not be discovered, nor any "uterine souffle" (so called) distinctly heard, the patient was pregnant, and that the fœtus was extra uterine. The bladder was considerably distended; the bowels were confined; and the vaginal discharge was considerable. It was ordered that

* It afterwards appeared doubtful if she had been married at all; it was stated that she had been a prostitute, but had lived with one man for the five years above mentioned.

the catheter should be introduced; that a fetid enema should be administered; and that she should take three times a day a draught containing rhubarb, cinchona, and oilbanum.

21st.—She complained of general abdominal pain, and some local tenderness; and the physician-accoucheur was requested to examine her. In the interim she was ordered to have half a grain of opium, to be taken when the pain was severe, eight leeches applied to the tender part of the right inguinal region, and afterwards to have a poultice to the abdomen. The obstetric physicians, while they acknowledged the probability, from the general and local symptoms, of the existence of extra-uterine pregnancy, supposed that, from the absence of any indication of foetal pulsation, from the presence of milk in the breasts (for it could now be pressed from both nipples), and from the consideration that if the foetus were dead the milk would have ceased to have been secreted, the symptoms, both local and general, might possibly arise from disease of the ovary, independently of pregnancy.

22nd.—The leeches afforded her much relief: she had passed a good night, and was much refreshed, and altogether much more comfortable in the morning.

26th.—The pains were much more severe, and entirely deprived her of rest: the bowels had not acted for two days. Ordered Enema Terebinthinæ stat. Pergat.

27th.—The countenance was flushed and rather anxious, and she complained greatly of pain at the hypogastrium: the tongue, however, was moist and clean; the skin natural; and the pulse not excited. The catheter was again introduced, in consequence of her having passed only a little urine with difficulty; but only half a pint of clear straw-coloured fluid was removed. The vaginal discharge was profuse: and she complained of vomiting, and loss of appetite. Ordered Soda Water; Cataplasma Lini.; hypogastrio applicand.; Pulv. Rhei. gr. x.; Pulv. Potass. Sulphat. 3ss., statim sumend; Opii gr. ss. 4ta quaque hora.

29th.—She was altogether better yesterday; but had passed a sleepless night: the pains were still very acute in the hypogastrium; the countenance was anxious, and the face alternately flushed and pale; the bowels were moderately

acted on by the powder, and continued very slightly relaxed. Ordered to continue the Opium during the day, and to take half a grain of Hydrochlorate of Morphia at bed-time.

30th.—She had a good night, but complained much of pain, and particularly of a dragging at the waist. She expressed much annoyance from a constant thirst; and the Opium was consequently omitted, but the Morphia continued.

31st.—She complained more of pain; and the local tenderness being considerable, five leeches were ordered to be applied; as on a former occasion they had afforded her so much relief. At ten p.m. she said she felt easier since the application of the leeches, and appeared inclined to sleep.

April 1st.—It was stated by the patient in the adjoining bed that our patient awoke about 4 A.M., and complained of great pain, and expressed a wish for the morning, that she might see the sister of the ward and the doctors. She continued very restless for the space of two hours, and then suddenly called for something to drink; after taking some water she composed herself apparently to sleep; but at the expiration of half an hour the nurse again visited her, and found her dead.

Necropsy, 33 hours after death.

The surface of the body was remarkably exsanguine. The joints, although rigid, were with facility flexed. At the junction of the mesogastric region with the hypogastric, and of the left iliac fossa with the pubic region, a firm and rather hard tumor elevated the abdominal parietes. The mammae were well developed, and a plentiful secretion of thick, creamy, opaque white, lacteal fluid could be expressed from the nipples. The body appeared to have been well nourished.

The abdominal cavity contained a large quantity of bloody serum and large masses of recently coagulated lymph. The great omentum extended downwards into the pelvis, and was adherent to the peritoneal covering of the hypogastric abdominal walls. The stomach and transverse colon were pulled down by this adhesion. The alimentary canal was normal, except that a few adhesions extended between the cæcum and sigmoid flexure of the

colon and abdominal tumor. The appendix cæci was firmly adherent to the posterior wall of the tumor.

Turning downwards the hypogastric abdominal walls and omentum, a large mass was seen filling up the pelvis. Its longest diameter was placed transversely over the brim of this cavity. The general outline of this tumor was triangular, the apex descending into the pelvic region. The base of this triangular mass was smooth, and seemed to be covered by the peritoneum. The largest part, however, appeared to consist of coagulated blood entangled in abnormal adhesions. Evidences of old peritoneal inflammation existed in different parts of the peritoneal membrane.

The urinary organs were normal. The direction of the axis of the uterus was towards the left side. It was of a larger size than is usually seen in the unimpregnated condition. It was remarkably exsanguine. Projecting from the os was a very firm plastic plug, of a soft solid consistence, something between mucus and gelatine, of a pale yellow tint, and very cohesive. The cavity of the uterus was lined with a soft flocculent tissue, of a pale yellow tint and speckled appearance. The right uterine appendages were bound together by slight adhesions. The right ovary was corrugated, and contained small Graafian vesicles. The right fallopian tube had a small cyst developed in it at about $1\frac{1}{2}$ inches from its uterine aperture. From the uterus to the site of this cyst it was pervious and normal; but between the cyst and the fimbriated extremity the tube was dilated and filled with mucous flocculent matter. Where the tube and cyst were in contact the former appeared to be obstructed. The left uterine appendages formed integral portions of the tumor. The left ovary was corrugated, and it contained a small cyst, which had the appearances of a recently formed corpus luteum. The left fallopian tube was pervious and normal along the whole canal; but its fimbriated extremity was adherent to the tumor.

To the left uterine appendages a cyst was attached, which appeared to be composed of an expansion of the broad uterine ligaments, a new membrane, and a portion of the great omentum. The anterior surface of the cyst was formed by the omentum: a corrugated and ex-

panded portion. The posterior surface was formed by the left broad ligament. Between this cyst and the uterus were masses of coagula, apparently the result of old and recent effusions of blood. The cyst contained a male fœtus of about the fifth month of development, and perfectly formed, with the exception of two small excrecences on the head. The umbilical cord and placenta were perfect. The amnion was well formed; but there was no appreciable quantity of liquor amnii. The back of the fœtus was directed towards the mother's umbilicus; its head to her left ileum.

When that portion of omentum composing the cyst was examined, some enormously distended veins were found attached to a red mass, which proved to be the placenta. In fact, this, which corresponded with the uterine surface of the placenta, was attached to a part of the omentum—the analogue of the internal surface of the uterus. In one point a separation had taken place; and, from the appearance of the coagula in its vicinity, it was clear that here the fatal hæmorrhage had occurred. The vessels which carried blood to the placenta were from the uterine arteries. The numerous enlarged veins in the great omentum returned the blood into the mother's circulation. The ovum, escaping from the left ovary must have fallen upon the superior surface of the left broad ligaments, and there taken root.

It is highly probable that, as the anterior surface of the omentum was adherent to the peritoneal aspect of the anterior abdominal walls, and by its posterior aspect to the placenta, some action of the abdominal muscles, during her fits of restlessness, may have given rise to the separation of the omentum from the placenta, and hence the fatal hæmorrhage.

I cannot state that there were evidences of old adhesions existing about the uterine appendages before the abnormal conception. All I saw appeared to be produced by the tumor itself.

THE PUBLIC HEALTH ACT IN WORCESTER.
THE citizens of Worcester have declined, on the plea of expense, to appoint a medical officer under the Public Health Act.

MEDICAL GAZETTE.

FRIDAY, SEPTEMBER 27, 1850.

BEFORE another number of this journal issues from the press a new medical session will have commenced. During the recess but few changes appear to have taken place in the Metropolitan and Provincial Schools. We recognise in the advertised lists the same well-known names as teachers. The only important change in the metropolis is, that the Professorship of Surgery at University College, vacated by Mr. Arnott, is now filled by Mr. Erichsen, a gentleman who has already earned a good reputation as a practical surgeon. The order of the courses to be pursued by the student remains unchanged; for we cannot conceal the fact that we are still jogging on in an unreformed state. Again, the amount of the fees for Hall and College is calculated with very great precision, so that a student may know to a fraction what will be the cost of his medical education; and he will also find, in the advertisements of different schools, every variety of accommodation as to the time and mode of payment.

It is not our custom to recommend to the student the selection of one medical school in preference to another. There are, however, some points on which it may be proper to make a few observations. We will suppose that the student is desirous of obtaining a sound knowledge of his *profession*, and not merely of acquiring such a superficial acquaintance with the various branches of medical science as will enable him (by the aid of a grinder) to procure the *diplomas* of the College and Hall. Under these circumstances he should select a school to which there is

attached a hospital or infirmary, in the wards of which clinical instruction is regularly imparted by the attendant physicians and surgeons. A *medical* school unconnected with an hospital is a misnomer; and it is our belief that whatever temptation is held out to the student in the shape of "ruinously low" fees, or an abundance of prizes, &c., his time will be lost by associating himself with such an establishment. This is so well known, that the suggestion is scarcely needed, except for the guidance of those who have no professional relation or friend to advise them on the selection of a place of study. Of the Hospital Schools of London, and the reputation of the medical officers associated with them, we need say nothing. These institutions are now under such control and management, that there is not one in which, with ordinary care and industry, a student may not acquire an excellent knowledge of his profession. It is, however, a fact calling for notice, that there is a sort of advertising competition among these establishments, against which we must put neivices on their guard. Among the Metropolitan schools which have acquired good repute as seats of study, the difference in the total amount of fees required for the College and Hall qualifications is so small, as to be scarcely a matter for consideration. Competition has here taken another form. Scholarships and prizes on a profuse scale are held out as tempting baits to allure the wavering student. The whole affair appears like a medical lottery, in which every student who purchases a ticket is almost sure of obtaining a prize of some description. Such schemes may be expedient and even necessary in some cases to attract young gentlemen fresh from the country; but we doubt the propriety of allowing the character of a school, as a place of medical *education*, to depend upon such a system. The

only valuable prizes to a medical student are the dresserships, clinical clerkships, and other professional offices about a hospital; because in working for them, he cannot fail to improve himself in a practical knowledge of his profession. If tempted by a gold medal, or a handsome prize in money, to work on some special subject, there can be no doubt that his general medical education must be neglected. During his studentship he may make a good medalist, and an honoured prizeman, but in after life he will be found an indifferent practitioner. It is, we believe, within the experience of most students and teachers, that men whose attention is thus directed to one subject are marked throughout the session as neglectful of other branches of their education.

We observe in the advertisement of one Hospital School, that a prize of one hundred pounds, to be awarded in 1851, is offered for the best essay "*On the Chemical and Physiological Action of Mercurial Preparations.*" We are inclined to ask seriously what can be the motive for making such an offer as this? If such a prize had been thrown open to the profession generally we could understand the object of it; but when we find that it is presented as an October attraction to gentlemen who have yet to learn chemistry and physiology, and who can have had no practical experience, without which such an essay would be worthless, the offer is simply ridiculous. It may, however, have a mischievous effect: it may induce some ten or twenty students to devote their time to the preparation of essays on a subject on which a professional standing of ten years, with good opportunities of acquiring experience, and a constant attention to the rapid progress of chemical and physiological sciences, would alone qualify a man to write. Under these circumstances the competing students must be withdrawn

from their proper duties; and we doubt not, that all but the successful candidate will regret the waste of valuable time. We have seldom met with a more absurd illustration of the injudicious extent to which the students' prize-essay system can be carried than this.

The lists of advertisements further show us that the commercial value of the lectures and practice required in the metropolis for the College and Hall qualifications, varies from *fifty-seven to one hundred guineas*—a fact which shows either that the professors at some schools are content to impart their experience at a very small profit, or that those belonging to other schools are greatly overpaid for their services. The quality of the instruction, and the relative means of teaching, are, we fear, not sufficiently investigated by the licensing boards. The student must therefore look to these points himself, or he will find when too late, that it will be necessary to withdraw from one school and enter to another. As a summary of our advice, we shall remark:—Select a school to which an hospital is attached; and look with distrust upon those establishments whose principal recommendations are very low fees, or an abundance of medals and prizes.

We have now before us the last Annual Report of the Sydenham Society, and we are glad to perceive, from the contents of this document, that the members have already derived many advantages from its establishment. Our readers are doubtless aware that this Society has been founded for the publication, in a cheap form, of—I. Reprints of standard English works: and in this department it has issued, during the last seven years, the entire works of Sydenham, Harvey, and Hewson. II. Miscellaneous selections from the ancient and from the earlier modern

authors. Three works which fall under this division have been published. III. Digests of the works of old and voluminous authors, British and foreign. For satisfactory reasons assigned in the Report, this part of the original plan has not yet been carried out. IV. Translations of the Greek and Latin medical authors, and of works in the Arabic, and other eastern tongues. Under this department several works have been issued, including the seven books of Paulus Ægineta, the genuine works of Hippocrates, and the Treatise of Rhazes on the Small-pox. V. Translations of foreign works, of which several valuable volumes have appeared. VI. Original works of merit. The Council admit that no work referable to this division has yet been published. The subject has been well considered, but some serious difficulties have presented themselves in carrying out the scheme. It is stated that a medical bibliography of all countries is now in preparation. This cannot fail to prove a most valuable aid to the student of medical literature.

"In addition to the Bibliography, the following works are also in preparation, some of which are ready for the press:

"I. The remaining portions of Rokitsansky's Pathological Anatomy, now the only one of the Society's publications which is not completed.

"II. A volume on Epidemic Catarrh.

"III. The writings of Unzer and Prochaska on the Nervous System.

"IV. A further Selection from the Works of Dupuytren.

"V. A volume to comprise the most important Writings of the earlier British Physiologists; and,

"Lastly, during the past year, the Council have made arrangements for an edition of Hunter's Plates of the Gravid Uterus."

That the Society has not been idle during the seven years of its existence is apparent from the following extract from the Report:—

"Those who have been members of

the Society from the beginning have now received, for their seven annual subscriptions of £1. 1s., twenty-three 8vo volumes, which have been got up in excellent style; the paper, printing, and binding, being the best that could be obtained without entailing any extravagant expenditure. Each volume has on an average 475 pages; the whole twenty-three volumes comprising 11,000 very full pages of letter-press, and twenty-six illustrative plates. This, it will be found, is as much, if not more matter than other similar societies, with a far greater number of members, have been able to issue. The ordinary publishing price of the Sydenham Society's works would probably be about £16 or £17. It is, therefore, scarcely necessary to adduce further proof of the mercantile success of the Society. It may, however, be mentioned, that the Sydenham Society's books are eagerly sought after in the second-hand book market, where a few copies have found their way, chiefly through the death of members, and have always fetched good prices.*"

We find from the Report that the publications of the Society have been already distributed among 2543 members of the profession—a fact which shows the great extent to which medical literature has been already diffused by this Society. From its *quasi*-commercial character it is clear that the greater the number of subscribers the larger will be its sphere of usefulness, and the greater the amount of benefit conferred upon each subscriber. The Society is deserving of the patronage of all those who take an interest in the diffusion of medical literature.

* We subjoin a list of the works already issued by the Society:—*For the First Year, 1843-4*—Hecker's Epidemics of the Middle Ages, one vol. 8vo. pp. xx. 380; Louis on Phthisis, one vol. 8vo. pp. xxxv. 871; Th. Sydenham, Opera Omnia, one vol. 8vo. pp. xxx. 668. *For the Second Year, 1844-5*—The Seven Books of Paulus Ægineta, vol. i. pp. xxviii. 668; Observations on Aneurism, one vol. 8vo. pp. xii. 524; Simon's Animal Chemistry, vol. i. 8vo. pp. xx. 360, plate. *For the Third Year, 1845-6*—Simon's Animal Chemistry, vol. ii. 8vo. pp. xii. 360, 2 plates; Paulus Ægineta, vol. ii. 8vo. pp. xi. 611; Haase's Pathology, 8vo. pp. xvi. 400. *For the Fourth Year, 1846-7*—The Works of W. Hewson, in one vol. 8vo. pp. lvi. 360, portrait and 8 plates; Dupuytren's Lectures on Diseases and Injuries of Bones, one vol. 8vo. pp. xvi. 459; The Works of W. Har-

Reviews.

Medical Biographies: Oxford Series.

The Lives of Charles Brandon Trye, Surgeon; George Cheyne, M.D.; Thos. Harrison Burder, M.D.; Sir James Stonhouse, Bart., M.D. Oxford: Parker. London: Churchill.

THIS series of works was originally published anonymously, but the fact is now well known that they have been produced under the editorship of the learned Dr. Greenhill, of Oxford, known to the profession as the editor of several valuable works in medical literature, and among these of the Latin edition of Sydenham's works, and of the translation from the Arabic of Rhazes on the Small Pox, both published by the Sydenham Society.

Our object in now bringing these biographies under notice is not so much to laud eminent medical attainments,—for perhaps of the four medical men above referred to, the name of one only is known far beyond his own immediate circle,—but rather to impress upon our readers the great and all-important lesson which it is the desire of the editor to inculcate by their publication—viz. that medical skill and great professional and other acquirements are perfectly compatible with, and will receive lustre from, their union with high spiritual attainments; that the perfect medical character is best seen in the earnest and sincere Christian; and that a due sense of the

value of the soul can alone give a due sense of the physician's responsibility to his patient.

Although our limits will not permit us to borrow many particulars of the subjects of the several memoirs, we must not dismiss them without a brief notice of each.

Mr. Trye was many years an eminent surgical practitioner in Gloucester, but still better known and endeared to his fellow-townsmen by his attention and benevolence to the poor, by his strict integrity and genuine piety founded on a firm and influential belief in the truths of Christianity. Mr. Trye died in 1811.

Dr. Cheyne's name is more widely known in medical literature, which he enriched by many productions between the years 1702 and 1742, in which year he died. His pen did also some service in the cause of religion. "In a sketch of his character that appeared in one of the papers at the time of his death, he is called 'a learned physician, a sound Christian, a deep scholar, and a warm friend;' and it was said 'that those who best knew him, most loved him.'"

"Dr. Cheyne followed the business of his profession with great diligence and attention, dividing his time between London and Bath"—a mode of practice, we may observe, not so likely to prosper in the present day, even under the advantages of railroad travelling.

Dr. Burder's reputation, though partly founded on his professional attainments, is mainly based on his religious character. He practised several years in London; but the feebleness of his health, which forbade his so prosecuting the practice of medicine as to rise to eminence therein, at last compelled him to relinquish its pursuit entirely. Dr. Burder died in 1843. His writings were not numerous, but excellent in their way; he is more especially and usefully known as the author of "Letters from a Senior to a Junior Physician," which have also been republished by Dr. Greenhill.

Of Sir James Stonhouse it is recorded that he was a successful practitioner at Northampton, and the intimate friend of Doddridge and Hervey; and that he relinquished practice and took Holy Orders.

However different the intellectual endowments and professional attainments of these several individuals may have been, they all possessed the

vey, M.D., complete in one vol. 8vo. pp. xcvi. 624. For the 5th year, 1847-8—Paulus Ægineta, vol. iii., pp. viii. 653.; Feuchtersleben's Medical Psychology, pp. xx. 393.; Microscopical Researches of Schwann and Schleiden, pp. xx. 268, 6 plates; Memoirs of the French Academy of Surgery, pp. x. 293. For the sixth year, 1848-9—Rhazes on the Small-pox and Measles, 8vo. pp. viii. 212; The Genuine Works of Hippocrates, translated from the Greek, vol. i., 8vo. pp. x. 466; The Works of Sydenham, translated from the Latin, vol. i., 8vo. pp. cvi. 376; Rokitsansky's Pathological Anatomy, vol. ii. (the first issued), 8vo. pp. 875. For the seventh year, 1849-50—The Genuine Works of Hippocrates, vol. ii., pp. 406; Essays on the Puerperal Fever, and other Diseases peculiar to Women, 8vo. pp. 552; The Works of Sydenham, translated from the Latin, vol. ii., pp. 396.

Notice.—New Members may still obtain the first, fourth, fifth, and sixth year's works; but of the second and third year's books none remain. In lieu of the third volume of Paulus Ægineta, new members will receive a copy of some other work; so that, on commencing with the fourth, or either of the subsequent years, the series of books which they will obtain will be complete in themselves.

"one thing needful." Although their biographies afford us but little of strictly medical interest to transfer to our pages, yet the records of their lives, and of the gradual development of their Christian character, as contained in these volumes, will furnish the best of lessons to those who may peruse them. Their histories are truly "philosophy teaching by example"—and examples indeed, teaching true philosophy.

There is one feature in the biographies which deserves especial notice—namely, the copious annotations by the editor: these supply much valuable information, respecting many persons and events, which could only have been obtained by laborious research and extensive erudition.

We may notice also that no small share of interest which each of the volumes before us possesses, is derived from the correspondence with individuals eminent for religious and medical attainments, which is contained therein.

We might draw many contrasts in favour of the unobtrusive goodness of such men as Trye, Cheyne, Burder, and Stonhouse, when compared with more extended reputations, solely professional;—we might, for example, set against these instances many of greater celebrity,—but we content ourselves with one parallel, and submit to our readers to select between goodness and mere greatness.

Our continental brethren have recently, with their wonted pomp and ceremony, inaugurated a statue to the memory of Larrey, in the Military Hospital of Val-de-Grâce, Paris. To grace the ceremony, a number of cripples who had been operated upon by Larrey on the field of battle were ranged at the base of the statue; with these stood the remains of "*la grande armée*," dressed in their historical costumes; and among them, the nephew of "the Emperor," who, as President of the French Republic, attended in its name to do honour to departed surgical skill. All the learned, civil, and political institutions of Paris sent their representatives, to add dignity to the ceremony. Eloquent laudatory orations were delivered in praise of Larrey and of French surgery. The skill, the courage, and the perseverance, and all the moral qualities of the deceased, were lauded to the skies,—but of the highest of all praise, that he was an eminent

Christian, we are not informed by his eulogists: perhaps this was not regarded as being a qualification necessary for the erection of a statue at Val-de-Grâce.

Some of our brethren on this side the Channel seem disposed to regard such posthumous honours as greatly enviable. If we may judge from the frequency with which we meet with the discussion of the topic in periodicals, the public award of honours to members of the medical profession is one which seems desirable to many, and the attainment of which occupies much of their attention. It should not be forgotten, however, that our mission is to the homes of our neighbours; our instruments are kindness, skill, and attention, and we doubt if these be compatible with the introduction of the elements of ambition as an inducement to the performance of our duties.

We are disposed to regard it as a higher honour to the profession, to be ranked among the "good," than among the merely "great:" we doubt not that the majority of our readers will concur in this opinion.

Some more of these biographies are promised, which we shall gladly welcome. Each sketch "forms part of a series of Christian medical biographies which it is proposed to publish, of those physicians who have been most eminent for their piety, in whatever age and country they may have lived;" and it is to spread the influence of their good example that we would urge upon our readers to possess themselves of these volumes.

Medical Portraits. Drawn by J. H. MACGILL, and printed by HANHART. 1850.

We have now before us, of the series of medical portraits of which we have already been able to speak in high terms of approbation, lithographic likenesses of *Mr. F. C. Skay and Professor Paget*, of St. Bartholomew's Hospital; of *Mr. Hancock* and *Mr. Avery*, surgeons of the Charing Cross Hospital; of *Mr. James Ronald Martin*, Presidency Surgeon, and Surgeon to the Body Guard of the Governor-General of India—author, in conjunction with the late Dr. Johnson, of a Treatise on the Diseases of Tropical Climates; and lastly, of the late *Sir William Ellis*, of

Hanwell. Of these six portraits, five are marked by the hand of a master. We have never seen the lithographic crayon applied to portraiture with such success as by Mr. J. H. Maguire. His drawings are clear, the position varied, and, while the greatest pains have been bestowed on the features, the details of the dress are accurately and carefully given. The portrait of Professor Paget is an admirable likeness, and is a beautiful specimen of lithographic art. The portrait of the late Sir William Ellis is evidently by a different hand, and in a different style. We recommend this serial collection to all who wish to have a real gallery of medical portraits.

Proceedings of Societies.

ACADEMY OF SCIENCES, PARIS.

September 30, 1850.

On the General Diffusion of Iodine—Gôitre—Researches on the Constitution of certain Waters.

M. CHATIN, who has been for some time past engaged with investigations on the existence of iodine in crevasses and other *fresh water* vegetables, has presented another long communication to the Academy, having for its object to show the general diffusion of iodine throughout various departments of nature, and especially in fresh water and the plants which vegetate therein. M. Chatin has ascertained the presence of iodine in aquatic plants, not only of European growth, but in those of Asia, Africa, America, and New Holland, shewing the general diffusion of this body on the surface of the earth. He also finds that the ashes of vegetables which grow out of contact with water do not contain iodine. Hence M. Chatin derives the inference, that the state of the globe at different epoch may be deduced from an analysis of the ashes of the vegetables then produced. Thus, for instance, coal rich in iodine would be the produce of vegetables which flourish whilst the waters covered the surface of the earth: anthracite, containing a less proportion of iodine than coal, would show that its formation was derived from an admixture of terrestrial plants with the great cryptogamous plants of the coal formation, and therefore at a period subsequent to the appearing of the dry land; whilst the lignites, which contain little or no iodine, indicate their formation to have occurred

during the present condition of our globe. Iodine reappears in the ashes of peat, the modern detritus of the stunted vegetation of our marshes and bogs. Graphite, from the large proportion of iodine which it contains, would appear to claim a place amongst the productions of an organic and aqueous origin, representing the vegetation of a period long anterior to the coal formation, and probably the first which appeared on the surface of the earth after it had become cooled.

The animals which live in fresh water were found by M. Chatin to contain iodine, and in larger proportion than the plants grown under the same circumstances. M. Chatin is of opinion that the richness of waters in iodine may be correctly deduced from the amount of iron they contain: so that the waters to which we apply the term ferruginous may also be called ioduretted waters. The waters of volcanic formations appear from M. Chatin's researches to contain more iodine than the sedimentary strata. The waters containing much lime and magnesia salts usually show but slight traces of iodine. The usual form of the appearance of iron in these waters is that of iodide of iron.

Iodine exists also in some terrestrial plants and animals, especially in plants frequently watered. The salts of soda, potash, and magnesia, as ordinarily met with in commerce, almost always contain appreciable quantities of iodine. Fermented liquors are found by M. Chatin to contain iodine, but in less proportion than the mean of fresh waters. Wines also contain iodine, varying in quantity according to the variable nature of the soil producing them. Milk is richer in iodine than wine, and asses' milk contains more than the milk of the cow. The quantity of iodine appears to be in an inverse proportion to the abundance of that secretion. Eggs are extremely rich in iodine. A hen's egg of the weight of 2½ ounces contains more iodine than a quart of milk, or than two quarts of wine or good water.

Iodine exists also, according to M. Chatin, in arable soil, in sulphur, the ores of iron in peroxide of manganese, and in sulphuret of mercury. M. Chatin considers it very probable that the want of iodine, or its existence in too small a proportion in the waters employed in different countries for domestic purposes, is the principal cause of gôitre, and recommends the employment in cases of this affection of such diet as is found to contain a somewhat large proportion of iodine.

As somewhat in connection with the above, we may mention some recent researches of M. Maumené on the water of Rheims. M. Maumené states that he is

unable to find the least trace of magnesia either in the waters or in the soil; and to this want of magnesia in the water he attributes the almost universal prevalence of the goitre in that city, there being but few families in which one or more persons are to be found exempt from this affection.

In his investigations on these waters, M. Maumené notices a fact somewhat opposed to preconceived opinions; it is, that water containing sulphate of lime is not always decomposed by soap. His experiments go to prove that all the salts of lime, without exception, may exist in somewhat considerable proportion in water which does not contain other salts, without producing a deposit on the addition of soap: the maximum limit he finds to be about 10 grains of sulphate of lime, 64 grains of chloride of calcium, and 10 grains of nitrate of lime, each, in one quart of water. Carbonate of lime would at first sight appear to prove an exception; but the addition of soap produces only an opacity up to a certain limit, beyond which a deposit takes place. M. Maumené considers that the insoluble carbonates do not exist in water in the state of bicarbonates, as has generally been supposed. If the carbonic acid influences the solubility of these carbonates, it is not, he considers, by reason of any chemical action, but simply in virtue of a dissolving force. Carbonic acid is not the sole agent in the solution of the carbonates; there are certain salts which have the power of destroying this insolubility. Hence he concludes that in natural waters the carbonates are dissolved partly by the action of carbonic acid, and partly by the action of other saline substances.

POISONING WITH DULCAMARA. BY DR. PLATSCHKE.

A MAN, 40 years of age, who was using decoction of dulcamara-stalks for a cough, took, one forenoon, from three to four quarts prepared from a peck of the stalks. In the evening he was suddenly seized with numbness in his limbs, and pains in the knees and elbows, dryness of the throat, and paralysis of the tongue. These symptoms increased so much in the course of three or four hours, that he could scarcely move either his limbs or tongue. The head remained unaffected, consciousness unimpaired, the pulse quiet, but small and rather hard, breathing regular, the skin cool; there was neither nausea nor vomiting. From the time which had elapsed since taking the decoction, the administration of emetics was contra-indicated; recourse was therefore had to stimulants. Camphor was given freely, and the symptoms gradually disappeared. — *Casper's Wochenachrift*.

Hospital and Infirmary Reports.

ST. BARTHOLOMEW'S HOSPITAL.

Necrosis of a Portion of the Os Calcis—Extraction of the Dead Bone.

JAMES RAGGON, *et. 16*, an apparently healthy lad, fell down stairs a year ago and hurt his left heel. The injury was followed by swelling and severe pain, and subsequently by a discharge of matter, in which a few small pieces of dead bone were discovered.

July 26th, 1850, admitted under Mr. Stanley. The neighbourhood of the heel and malleolus is swelled, and there is very little motion in the ankle-joint. There are four fistulous openings in the skin, on the outer side of the os calcis; and through one of these, behind and below the outer malleolus, dead bone can be plainly felt with the probe, in a situation that may readily be approached. An operation for its removal was therefore proposed, and performed in the following way, on Saturday, Aug. 17th.

When the patient had inhaled sufficient chloroform to be insensible to pain, Mr. Stanley made a semilunar incision along the outer margin of the calcis and insertion of the tendo-achillis, and reflected the integuments, which formed a thick flap, from the outer surface of the calcis, and exposed a loose piece of dead bone, about the size of a nutmeg, occupying a cup-shaped cavity, with a highly vascular lining, in the outer wall of the calcis. The cavity which lodged it was so superficial, that the dead bone was removed without further cutting; and when the integuments were replaced, very little injury appeared to have been inflicted on the neighbouring parts by the operation.

Sept. 5th.—The wound is healed, and all the sinuses also, except one, which is healing. The swelling is diminished, and the foot promises to be nearly as serviceable as the other.

Necrosis and Removal of the first and second Metatarsal Bones.

Sarah Jackson, *et. 36*, a pale and thin woman from the country, in the habit of working hard, states that seven months ago her right foot became painful, swelled on its inner side, pattered, and was lanced by a surgeon. The wound did not heal, and the pain continued; and, to avoid pressure on the part, she contracted the habit of walking on the outer margin of the foot.

July 25th, 1850, admitted under Mr.

Stanley. The sole of the foot could not be placed flat on the ground, the inner side being raised and fixed in the position of an ordinary talipes varus. There was a wound on the inner side of the foot, over the first metatarsal bone, through which dead bone was felt. In the beginning of August Mr. Stanley enlarged the wound, and extracted nearly the half of the first metatarsal bone, which was dead, and detached from the surrounding tissues; and, on introducing his finger, discovered that the remainder of this, as well as the whole of the second metatarsal bone, was dead, and that each bone was broken in two about its centre; and, as is usual in necrosis of the metatarsus, there was no attempt on the part of nature to form new bone. As separation of the dead bone without surgical interference would, in this case, be a very long and tedious process, which the weak state of the patient could ill afford, Mr. Stanley advised its removal, as well as that of the phalanges, which could be of no use if allowed to remain without their metatarsal bones.

Aug. 10th.—The operation was performed while the patient was under the influence of chloroform. The necrosis had not extended beyond the two inner metatarsal bones; these were removed, together with their phalanges, and the integuments were brought together by sutures.

Sept. 6th.—The patient's health has improved since the operation, the wound is nearly healed, and the cloistrix is on the upper part of the foot, where it will be free from pressure. The varus of course remains, and probably will not be detrimental to a foot which possesses its three outer toes only.

Necrosis and Reproduction of the Clavicle.

Caroline Gurney, *et. 16*, states that about three months ago she was much fatigued by carrying a child on the left arm; that she subsequently discovered a swelling, and experienced a great deal of pain, in the neighbourhood of the left clavicle, and was quite unable to move the arm on that side.

Sept. 28th, 1847, admitted under Mr. Lloyd. There is swelling and fluctuation over the centre of the clavicle. On pressing the bone a crepitus may be felt. The arm is powerless. Some leeches and a cold lotion were applied, and the arm was banded to her side.

During her stay in the hospital she was attacked with erysipelas of such a severe nature, that at one time her life was despaired of. The swelling over the clavicle was opened, and pus was discharged from the wound.

Dec. 6th.—The erysipelas has left her in a very weak and emaciated condition, from

which she is slowly recovering. Pus is still discharged from an opening over the clavicle, and the bone is not yet united.

Feb. 6th, 1848.—She is now regaining strength rapidly, and has recovered partial use of the arm. The clavicle is united, but tender on pressure, and surrounded by hardness and thickening of the adjacent parts.—Discharged.

In May, 1850, she came to the hospital to show a piece of bone that she had herself extracted from her shoulder. She stated that the end of it projected through the skin, and, being loose, she took hold of it, and drew out what proved to be the clavicle, nearly entire, wanting only its two articular extremities. It is smooth and white on its surface, and has a large canal through its centre, caused by the absorption of its cancellous structure. A new clavicle had been formed, and appeared to answer every purpose remarkably well; its surface was rather irregular, and adherent to the skin at different places. She had very good use in the arm, and was able to work as a domestic servant.

The spot where the bone originally gave way must have been near one of its extremities, as there was no fracture in the necrosed portion now produced. And at the time she left the hospital the dead clavicle must have been inclosed in a sheath of new bone strong enough to allow partial use of the arm, and to give the clavicle the appearance of having united as well as was expected after such a severe attack of erysipelas.

Excision of an Epithelial Cancer from the lowest Part of the Back.

Jane Collins, *et. 59*, states that a year ago she experienced a painful sensation at the lower part of her back, and discovered a small tumor in this situation. It has increased in size, and has frequently been very painful, especially on slight irritation or pressure from her dress, during the last six months.

Aug. 28th, 1850, admitted under Mr. Paget. A circumscribed subcutaneous tumor, about the size of a walnut, of a dull grey or nearly black appearance, prominent and elastic in the centre, but firm and extended in the base, moving readily in the subjacent fat, but adherent to the skin covering it, which is thin, was situated nearly in the middle line of the back, over the sacrum. As it had somewhat the appearance of melanosis, the patient was asked whether she had previously noticed any mole or spot in the skin—so frequently the seat of these tumors—in this situation. Of this, however, she had no knowledge. Its excision was deemed advisable, and was readily consented to by the patient.

Aug. 31st.—Chloroform was administered, and Mr. Paget removed the tumor, with a portion of the skin and subcutaneous fat in which it was embedded. The edges of the wound were brought together by strips of plaster.

On examining the tumor, Mr. Paget pronounced it to be a soft cancerous growth of an epithelial nature. Its dark colour was confined to its superficial surface.

Sept. 6th.—The wound is granulating, and the patient is going on well.

Correspondence.

NEW APPARATUS FOR FRACTURES OF THE INFERIOR EXTREMITIES.

SIR,—If you consider the undermentioned description of a newly-invented apparatus for the cure of fractures of the inferior extremities to be worthy of a place in your excellent journal, I shall feel obliged by your inserting it in an early number.

I have the honour to be, sir,

Your obedient servant.

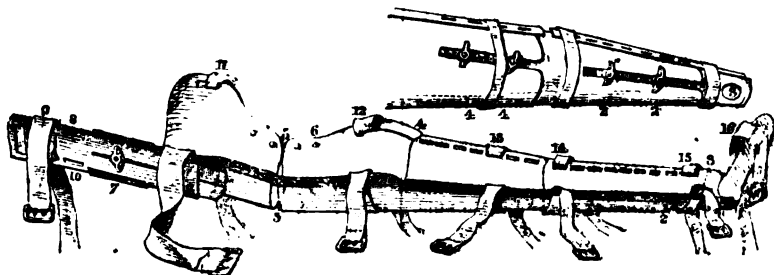
J. WHEAT D. BROWN, M.R.S.C.E.
House Surgeon and Junior Demonstrator
of Anatomy, London Hospital.

There are few things in surgery, according to my experience, more difficult to accomplish than the cure of an oblique fracture of the femur without deformity of

the limb, because the strong muscles of the thigh contract powerfully, and draw the inferior portion of the broken bone upwards, which necessarily causes a shortening of the limb; and, as compound and comminuted fractures of the thigh are also very difficult to manage, a great many ingenious mechanical contrivances have been used with the view of overcoming the power of the muscles, and of keeping the fractured ends of the bone in apposition. Still, as but few cases of fractured thigh-bones, when consolidation has taken place, are free from some kind of deformity, I have, after bestowing considerable attention on the subject, invented an apparatus by means of which I think that surgeons will succeed with more ease to themselves, and with a greater certainty of curing all kinds of fractures of the thigh without deformity, than by any of the other plans of treatment usually had recourse to.

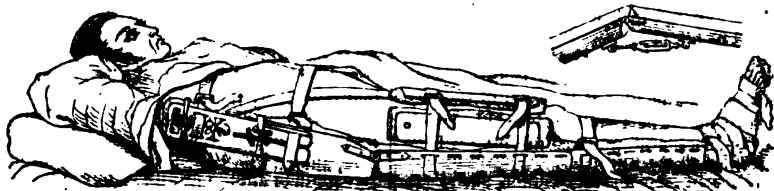
In describing this new apparatus, I shall, as I proceed, refer to the accompanying drawings of it, which will enable me more clearly to explain its principle and mode of action. It is made of thin sheet iron, formed into the shape of the limb, and extends from near the axilla down to the sole of the foot (see drawing 1, fig. 11). The part that embraces the leg consists of two portions which slide upon each other (fig. 2; see back view, 2 2), and are firmly fixed underneath by means of screws. The lower part has a foot-piece attached to it; and at the place where the heel is to rest, a piece is taken out so as to prevent undue pressure on that part (fig. 3). The upper part

DRAWING 1.



DRAWING 2.

DRAWING 3.



of the leg-piece slides upon the lower portion of the thigh-piece, and is fixed underneath by means of screws. The upper part of the thigh-piece is attached to the hip portion by two hinges, one upon the outer side, the other upon the inner (vide fig. 6 & 6). The hip-piece, which is attached to the thigh portion by means of these hinges, extends by a narrow projecting part half way up the side (fig. 7), into which another part that is made to draw up towards the axilla slides; the hip-piece expands very much just above the crest of the ilium, and passes over towards the opposite hip, in order to support the sacrum behind; and from this point down to the joint it gradually slopes in, so as to leave the perineum free (fig. 6).

This apparatus, as has been shown, consists of four distinct portions; two for the leg; one for the thigh, the upper portion of which, or hip-piece, covers and adapts itself to the formation of the hip and hip-joint, so that by means of the slides the apparatus can be lengthened or shortened, according to the length of the patient's limb. The upper and outer side of the hip-piece that extends upwards receives the fourth piece, which is fixed in its slide by means of a screw, so that this part of the apparatus can also be lengthened or shortened according to circumstances. At the upper part of the fourth piece there are two openings through which a piece of webbing is passed for the purpose of fixing this part of the apparatus to the chest (fig. 9); and this strap is kept in its place by two pieces of tape which cross each other, being fastened before and behind. Immediately below the above-mentioned openings there are two others through which pass the ends of the perineal band, for the purpose of making counter extension (fig. 10). At a short distance lower still are two more openings, one opposite the other, through which a band is passed to fix the pelvis to the apparatus (fig. 11). Immediately below the hip-joint are two other openings, one on each side, through which a strap is passed to fix the upper part of the thigh (fig. 12). Above and below the knee-joint are two more openings, opposite each other, through which straps are passed to fix the limb above and below the knee (figs. 13 and 14); and at the ankle-joint and upper part of the foot-piece are other openings of a similar description, for the purpose of fixing the ankle and toes (figs. 15 and 16).

Before applying the apparatus, a soft cushion, well padded with wool, of the size and shape of the apparatus, but rather broader, is placed in it, or an air cushion of similar shape and size. As soon as one or other of the above cushions has been properly adjusted, the limb is to be put

into the apparatus, and a two-headed roller applied round the ankle, which must first be defended by leather strapping and wool: the perineal band is then to be tightened, and securely fixed, and extension of the limb made, by drawing the leg down, and fixing the foot, by means of the roller, to a projecting handle attached to the foot-piece, which firmly fixes the whole limb. After this has been done, should one end of the bone rise above the other, and the broken limb be found to be shorter than the sound one, all that is required to be done is to loosen the joints of the apparatus, by turning the screws that fix them, and gradually lengthen them until the limb has been sufficiently elongated, when they are again to be fixed by the screws. The transverse straps above described are next to be brought round the limb and firmly fixed, which will necessarily keep the broken ends of the bone in apposition;—indeed, I think it must be evident, after this apparatus has been properly applied, that displacement can hardly take place; because the limb will lie upon, and be supported by, a smooth surface throughout its whole length, from the crest of the ilium to the sole of the foot. The action of the strong muscles situated on the back part of the hip-joint, the extensor muscles of the thigh, the flexors of the leg, and the extensors of the foot, will be overcome, and can hardly, I think, displace the ends of the broken bone: and if pressure be found to be required at any particular point on the anterior part of the thigh, there is no necessity for having recourse to any kind of bandage; all that is requisite to be done is to loosen the belt that is placed above the knee, and the one at the upper part of the thigh; then to place over the point where the pressure is required as many well padded short splints (drawing 2) as may be necessary to support the part, and to fix them by again drawing the above-named straps tight; and, as it is not necessary while doing this to move the apparatus, or in any way to alter the position of the patient, the fractured part of the bone can at all times be examined without the slightest inconvenience, which is a great advantage.

Another important point connected with this apparatus is, that the patient can raise himself up a little in his bed; for, as his body is fixed to the portion above the joint of the apparatus, he is enabled to flex it at this joint, and still there will be kept up sufficient extension, counter extension, and also pressure on the muscles of the hip behind, to prevent the broken ends of the bone from being displaced; whereas, where the common splints are used, however skillfully they may be managed, the efforts of

the patient to raise himself up for the purpose of eating, drinking, &c., too frequently cause displacement of both splints and bone. In cases where the fracture has taken place high up, and the upper portion of the bone is displaced by the action of the psoas and iliacus muscles, the lower portion must be brought into a line with the upper, which can easily be effected by raising the under part of the apparatus, which will bend at the hinge opposite the hip-joint, and still continue to keep up, from behind, a firm pressure upon all the muscles of the hip, thigh, and leg; and as success in the treatment of fractures of the femur depends chiefly upon being able to counteract the action of the muscles, and to keep the limb unmoved until after consolidation of the callus has taken place, I doubt not, for the reasons I have just assigned, that the above apparatus will satisfactorily accomplish those important objects, as has been proved at this hospital in one very severe case of compound comminuted fracture of the thigh-bone, in which, after every other plan of treatment had failed, this apparatus was used with much greater success than could possibly have been expected under the peculiarly unfavourable circumstances of the case: and I cannot allow this opportunity to pass without observing that, in cases where the femur is fractured above, or at its middle, that extension from the lower part of the bone itself may, I think, be advantageously made, through means of this apparatus, by passing round the knee a rather broad, firm, well padded knee-band, to which two short straps, with buckles at their ends, are fixed, one on the outside, the other on the inside of the joint; then, by passing round the outside of the foot-piece, below the projecting handle, the middle of a long leather strap, the ends of which, having been brought up close to the leg as far as the knee-joint, are, as soon as the broken bone has been properly placed, to be pushed through the above-mentioned buckles, drawn tightly, and secured, which, I think, will cause sufficient extension above the knee to prevent the inferior portion of the broken bone from being drawn upwards; and as soon as this has been done, the leg and foot should be extended, and firmly fixed in the apparatus, as above directed. I have not yet had an opportunity of trying this method of treatment, and cannot, therefore, at present speak positively as to its effects; but where the thigh-bone is broken as high up as its middle, it certainly, I must say, does appear to me to be plain, from the construction of the apparatus, that it might, in the manner just described, be so used as to render the risk of after displacement of the ends of the broken

bone but very small; because in this way, if properly managed, extension will necessarily be made and kept up from the lower end of the broken bone itself in a line with the upper portion, the leg and foot being at the same time extended and fixed, as above described, by means of the bandage at the ankle.

In cases where the patella is broken transversely, the apparatus now under consideration will be found to be of great value; for, as it bends at the hip, the limb, when placed in it, may be elevated to the proper height, and, at the same time, the body, by means of the hinge at the hip-joint, can be raised as high as may be requisite, which, in my opinion, is the best position in which a patient with a transverse fracture of the patella can be placed. The apparatus, too, may be used in fractures of the leg; and I shall only add, that it can also be made to answer the purpose of a double-inclined plane for the thigh and leg, by placing a hinge below the knee with a screw (drawing 5), through means of which, and the hinge at the hip, the leg and thigh, when placed in the apparatus, can be bent to, and kept at, any angle that may be required.

London Hospital, September 16, 1856.

TESTIMONIAL TO DR. CONOLLY.

SIR,—The following letter will, I hope, be regarded by you as of sufficient public interest to be allowed insertion in the MEDICAL GAZETTE.

I find by the printed circular that it has been determined to present Dr. Conolly with a Public Testimonial in acknowledgment of his successful efforts to ameliorate the condition and improve the treatment of the insane; and I doubt not that subscriptions ample for the purpose will be raised.

The testimonial considered most appropriate by the Committee is a portrait of Dr. Conolly, to be presented to his family; and an engraving of the same, to be presented to the subscribers. It is on this form of testimonial I wish to remark.

The first object of a testimonial is to do honour to merit; and in many instances this object embraces all that is required to be fulfilled. But in many instances, also, there is another object to be held in view, to confer a benefit as well as an honour upon the meritorious individual or his family: and this is to be accomplished by the form or kind of testimonial.

It has appeared to me that, on several occasions, the Committee authorized to decide upon the form or kind of testimonial have erred, and, instead of conferring an honour and benefit upon the individual

or his family, have rather taken advantage of a good name to promote some local improvement, or to serve some body of men.

In the instance of the late Rev. Canon Tate, his friends and pupils were called upon to do honour to his name by subscribing to rebuild the school-house at Richmond; which was indeed to make use of the esteem and repute in which the Rev. Canon was held, to effect a purpose beneficial to Richmond, while his family, to whose benefit the subscription fund should have been appropriated, were disregarded.

In the instance of Sir Benjamin Brodie, the subscriptions of his friends and admirers were diverted to the founding of a medal for the students at St. George's Hospital; the effect being to benefit the medical staff of the hospital rather than to honour and gratify Sir Benjamin, by presenting to him a testimonial to be handed down with his title as an heirloom to his family.

Had the Rev. Canon Tate bequeathed funds for the rebuilding of the school-house, or had Sir Benjamin Brodie given the medal to the students of St. George's Hospital, the form and kind of memorial would be consistent; but the course adopted leaves the impression of advantage having been taken of a fair name to promote a local interest or gain a private end.

These remarks do not apply to a portrait! Yet it is a question what testimonial would be most beneficial and acceptable to Dr. Conolly.

To determine this, it should be remembered how arduous has been the struggle made by Dr. Conolly to effect the good he has accomplished, and to elevate himself to his present high position: it should be remembered, that the efforts of his professional career (in common with the efforts of every physician) have been not to gain honour only, but to acquire fortune for the advancement of his family in life, and the independence of himself in old age. Would not, therefore, the presentation of a purse, say of a thousand guineas, be the more fitting and substantial testimonial? A portrait is of little value; and, where not only the immediate friends but the public are the subscribers, an engraving is not called for, and will not be coveted.

On the occasion of Dr. Conolly leaving Warwickshire, his friends desired to possess some memorial of him; and a portrait and an engraving, as indicative of personal esteem, were quite in character.

Besides, when I recollect the immense wealth and the famed liberality of this country, and the vast numbers who take a lively interest in the sunderation of the condition of the insane, I cannot but think the presentation of a portrait a reward too

insignificant for the public to bestow. There are many, very many, who would not subscribe to a portrait, who would yet give liberally to a more sterling testimony. I cannot but think that if greater publicity by circular and advertisement were given to the subject of a *pecuniary* testimonial to Dr. Conolly, as a public acknowledgment and reward for his long, zealous, disinterested, and most successful labours, the appeal would be responded to by wealthy philanthropists by subscriptions not of single guineas, but of five, tens, twenties, fifties; and that a fund would be raised worthy of the object and of the public.

Your obedient servant,
M.D., A SUBSCRIBER.

Bath, Sept. 14, 1850.

TREATMENT OF ANASARCA BY INCISION— THE CLAIMS OF MEAD AND LOMBARD.

SIR,—It would be a difficult and profitless task to have to determine the title which every modern writer has to originality; but, as a curious instance of the credit which some gain for that which is not their own, I may quote a sentence from Dr. Todd's lecture on Cardiac Dropsy in this week's Gazette:—"There is another method," says he, "of relieving the anasarcaous legs, lately proposed by Lombard of Geneva. Instead of pricking the legs at various points with a needle, he advises to make a single incision above the inner malleolus, or in some other convenient position (not too near a vein or artery), cutting through the skin and subcutaneous tissue down to the fascia. The length of the incision may be half an inch or an inch; and the rapid flow of water that these incisions admit of, is further encouraged if the patient sits on the side of the bed a certain time during the day with his feet placed in a tub." The plan of treating anasarca here attributed to Lombard is minutely described by Mead in his *Precepta Medica*, and illustrated by a case. He says, "*In leucoplegmatia invisio in tibia parte inferiore feri debet duobus super talum digitis ita ut vulnus ad cellulosa membranam nec ultra penetret quo per dies aliquot frequens humor feratur, crus autem interea fovere oportet decocto herbarum emollientium et calidarum adjecto spiritu vinoso cui addita sit camphora. At mirum tamen est quantam aquosi latidis sic effusi jacturam cum levamine ferant interdum hydro-pici. Cuius rei singulare testimonium proferre libet femina generosa, mihi cognata anasarca fere quinquaginta corpore firmo satia et robusto hydropice intercute et ascite laborabat ita in immensum tandem ventre distento ut decumbens omnis tam grave ferro nequirit. Cum de vita fere desperaretur*

unicam spem salutis eamque dubiam in humoris e talo emissionis superesse dixi. Illa autem strenue repugnabat. At precibus denum amicorum victa cessit itaque ex vulniculo in utroque crure eo modo quem exposui facto profluxit decem continuis diebus aqua ad congruam mensuram minimum quotidie." He goes on to say, that by tonics and diuretics the health of his patient became re-established, and that she remained well for five years.

It is not my purpose to discuss the treatment of anasarca, but to transfer from Lombard to our own Mead, all the credit this method may deserve.

I am, sir,

Yours respectfully,

WILLIAM GULL, M.D.

Gay's Hospital, Sept. 21, 1850.

MEASLES OCCURRING SPORADICALLY IN NEW-BORN CHILDREN.

SIR,—In the several discussions that have taken place regarding contagion in cholera, the fact which has always been considered most decisive upon the negative side of the question has been the demonstration of a genuine case where contagion cannot be traced, or scarcely conceived possible. On such occasions the difficulty of showing at all times the source of contagion in other universally recognised contagious diseases is but rarely adverted to. Let any one, however, who is interested in this subject, endeavour to determine the immediate origin of all the exanthematous fevers with which he may meet, and the links that connect case with case will present anything but an unbroken chain. My own experience as regards measles, scarlet fever, and small-pox, exhibits the difficulties of the contagious theory just as remarkable in respect to them as to pestilential cholera. Some of your readers may feel interested in the subjoined particulars, which exemplify in a striking manner the development of measles, complicating our ordinary notions very much on the propagation of this disease, and possessing, I conceive, considerable importance in their bearings upon the general subject of contagion:—

Mrs. E. gave birth to her first child on the 25th of July, 1847, attended by a friend on my account, during my own absence from home. I saw both mother and child upon my return in a few days, and they seemed doing well. Having taken my leave of the patient in about ten days from her labour, I was summoned to see the infant child in three or four days afterwards, and found it covered with measles, attended by fever, and having been preceded by the ordinary premonitory symptoms of coryza and cough. The symptoms declined in

the usual way, and in about a week the child was well. I neither saw, nor could hear of, any other case from which it could have been taken. The mother, intending a probable explanation of the phenomenon, stated that, according to the statements of her mother, she and a twin sister had measles also when a fortnight old.

On the 12th June, 1849, Mrs. E. gave birth to a second child. She did well, and I ceased to visit her at the expiration of about a week.

On being summoned to attend another member of the family some months afterwards, I was informed that the second child had had measles at the same period after birth, and exactly under the same circumstances, as in the former instance. All the inquiries I could make led to a conviction upon my part of the accuracy of the statements that were made. It had not been considered necessary to call in medical aid.

During the present year Mrs. E. again became pregnant, and I was once more engaged to attend her. She was confined on the 31st ult. under circumstances quite favourable. I was naturally curious to know what would happen with the child, when ten or twelve days old, and continued my own personal observation. Measles again became developed, accompanied and preceded by slight fever, with mucous irritation of the air passages. The case this time was but slight, yet it was unequivocal. The infant is now well again. Measles do not prevail in the neighbourhood; nor am I aware of any case from which the contagion could have been received.

I have mentioned the above circumstances to several medical friends, but have heard of no parallel experience; neither have I in books discovered records of anything of the kind.

The cases are curious at least; but in what point of view are they to be regarded *pathologically*? Were they true measles? Or does the debris of some *rubeolous* poison yet lurk in the maternal system, transmissible, like the syphilitic poison, to the fetus in utero? I do not myself offer any speculation.

Such instances, however, should be kept in mind when, from difficulty and impossibility of tracing contagion, we are prompted to reject altogether the idea of certain diseases being communicable.

I remain, sir,

Your obedient servant,

DANIEL NOBLE.

Manchester, Sept. 15, 1850.

General Intelligence.

THE INCORPORATION OF MEDICAL PRACTITIONERS.

Copy of Report of the General Committee of the National Institute.

THE Committee, in accordance with the resolution of the Council agreed to at the last meeting, took immediate steps to place the Bill for the Incorporation of the General Practitioners in the hands of James Wyld, Esq. M.P., in order that it might be presented to Parliament as speedily as possible; and they are enabled to report that the bill was laid on the table of the House of Commons on the 29th of July last, with an understanding between that gentleman and the Government that it should not be proceeded with during the then session of parliament: the bill has since been printed by order of the House.

With the view of securing future co-operation in support of the proposed measure during the ensuing session of parliament, and also of increasing the pecuniary resources of the Institute, the Committee have deemed it expedient to forward the following note to those who, although members of the Institute, are in favour of the incorporation of the general practitioners:—

The National Institute of Medicine, Surgery, and Midwifery.

4, Hanover Square,
August 20th, 1830.

SIR,—I am directed by the Council of the National Institute to inform you that the Bill framed for the Incorporation of the General Practitioners has been presented to the House of Commons by Jas. Wyld, Esq. and Colonel Thompson, and printed by order of the House, and may be obtained at the office for parliamentary documents, Great Turnstile, Holborn. Should you wish for a copy I shall be happy to forward one to you. The Council are most anxious to afford those gentlemen who favour the measure every information on the subject, in the hope that they will use their best endeavours to disabuse the minds of such of their professional friends as may entertain groundless fears and apprehensions with reference to the proposed measure, and with the view, also, to secure their co-operation and support during the ensuing session of parliament.

I am desired further to inform you that the introduction of the bill into parliament, and the means necessary to be adopted by the Council to facilitate its future progress, will be attended with considerable expense;

the Council of the National Institute therefore feel that at the present period they are fully justified in placing most prominently before the profession the subjoined copy of a resolution unanimously agreed to at a general meeting held at the Hanover Square Rooms:—

Copy of Resolution.

"That the interests of the profession at large at the present moment demand that unanimous and cordial support be given to the Council of the National Institute, for the purpose of maintaining, strengthening, and extending the present organization of the general practitioners in medicine, surgery, and midwifery; and that a voluntary subscription-book be forthwith opened for the purpose of collecting funds to enable the Council of the National Institute to carry out in full the measures now adopted by this meeting."

The Council cannot but entertain the greatest confidence that the just appeal contained in this resolution will be cordially responded to by those who advocate the independent incorporation of the general practitioners, especially by those who have evinced in the schedules returned their desire to accomplish this great object.—I have the honour to be, sir,

Your most obedient servant,
HERBERT WILLIAMS,
Secretary pro tem.

Subscriptions of any amount may be forwarded by post-office orders, or otherwise, either to Edward Tegart, Esq. or John Dodd, Esq., Treasurers; or, if by post-office order, made payable at the Post Office, Old Cavendish Street.

Several letters of a favourable character have already been received in reply to this communication: but as its issue is of recent date, the Committee are unable to state how far the application may prove generally successful.

Since the last meeting of the Council the Committee have had occasion to consider the suggestions contained in the letter recently transmitted by the Society of Apothecaries to Sir George Grey for the purpose of effecting an alteration of the Act of 1815; and they have in consequence felt it their duty to forward to the Society a resolution condemnatory of the course therein suggested, it being not only inconsistent with the principles they have hitherto advocated conjointly with the National Institute, but as tending to prejudice permanently the interests of the general practitioner:—

Copy of the Resolution.

"That this Committee having considered the propositions contained in the letter

from the Society of Apothecaries to the Right Honourable Sir George Grey, Bart. for altering the Act of 1815, hereby declare that they cannot support any scheme for regulating the education and conducting the examinations of the general practitioners in medicine, surgery, and midwifery, which is incomplete, or which does not embrace all the branches of education and practice necessary for the efficient performance of the duties of general practice. That in particular the Committee repudiate the remodelling of either of the existing Institutions (as they have hitherto the establishment of any new institution) that is not based upon the union of medicine and surgery, and which, in the education and examination for admission into such institution (which is also intended to confer a title to practise) does not embrace all the branches of both medicine and surgery.

"That the want of proper authority to examine in surgery has at all times since the Act of 1815 operated prejudicially upon the examinations instituted by the Society of Apothecaries; and that this Committee unequivocally condemns any proposition to alter that Act which does not embrace an amendment in this essential point. That the union of medicine and surgery has been the fundamental principle of the National Association and of the National Institute, and has been consistently adhered to throughout; and that, without questioning the right of the existing institutions to retain their special character, the Committee resolves to adhere to this principle, in reference to the education and qualification of the general practitioners, as essential to any measure of medical reform."

An acknowledgment only of the receipt of this resolution has as yet been received from the Society of Apothecaries.

At the annual general meeting of the members of the Institute, which took place on the 14th of August last, the following resolution was unanimously adopted:—

"That this meeting, deeming it inexpedient at the present time to proceed to the election of a new executive, do now adjourn until the second Wednesday in December, and that the present Council be requested to continue to conduct the affairs of the Institute in the interim."

The Committee hope that at the period mentioned in this resolution circumstances may enable the Council to suggest to their constituents and the profession generally some decisive course of action for their adoption, in order to sustain, during the ensuing session of Parliament, those principles for which they have so long contended.

On taking a retrospective view of the

proceedings of the National Institute, and the present state of the medical reform question, the Committee reiterate the sentiments which were specially addressed to the Shropshire Medical Association in their Report of August 9th, 1848. In commenting on the petition forwarded by that body to the House of Commons, the Council of the Institute stated, on the part of the General Practitioners of England, that, if the object of the petitioners could be obtained—viz., the concentration of the great body of the profession in the College of Surgeons, with a ten years' franchise, the right of voting carrying the right of holding offices of honour and distinction in the College, under proper limitations, but with no distinction as to whether the individual practises as a surgeon purely considered, or as a general practitioner—by which modification of the constitution of the College of Surgeons the general practitioners would have an efficient control over their own affairs, and a direct influence over the examinations of their own class—that their demands would be satisfied.

The foregoing principles have invariably been advocated by the Institute upon every occasion, so far as they relate to the College of Surgeons; and the Committee propose on a future occasion to lay before the profession their opinions on the scheme enunciated by the Council of the Royal College of Surgeons in their last communication to the Right Honourable Sir George Grey Bart.

THE IMPORTANCE OF LIFE INSURANCE TO MEDICAL PRACTITIONERS.

THE destitution in which so many of our brethren die and leave their families, affords scope for the utmost commiseration and regret. Some, it is true, suffer from the untoward urgency of circumstances, but others go on from day to day, without employing the means which lie at their disposal, to avert the else certain impoverishment that awaits wife and child, and perchance other dear dependents. The man must be wanting in natural affection or common sense, who neglects to provide for those whose welfare God has lodged in his hands. Who has a right to contract marriage, without discharging, so far as in him lies, the sacred obligations which it entails? There have been those who lived in unscrupulous profusion, whose families, nevertheless, it has devolved on the public to support. It is a common feeling with medical men, that they must "keep up an appearance;" and to this they too often sacrifice, or hopelessly procrastinate, the yet more insuperable obligation of making a provision for their families. How much better than this unwarrantable ostentation would

be the division of the daily receipts, half for immediate sustenance, and half for prospective requirements. If a man cannot or will not save, he should either abandon his profession, or at least not implicate others in his improvidence. Considering the exigencies and inevitable uncertainty of human life—medical life in particular—no one can deny the necessity, no one contest the efficacy, of saving. That admirable invention of modern times, life assurance, enables every man to compass a provision in reversion, almost out of hand. For the yearly outlay of twenty pounds, a practitioner turned twenty-six can secure a thousand pounds, with additions, payable were his decease there and then to ensue the very instant after his policy was signed. For a reasonable increase, the policy, for that or any other given sum, may be paid up in ten, fifteen, or twenty years; securing, as it were, in addition, a reversionary annuity for his declining years, or those other purposes which family requirements do not fail to suggest. I invite every member of the profession, if his life be not already assured, or his independence otherwise secured, in place of an annual guinea to a medical fund, to hand over his guinea, if no more, to an insurance company. For this slender sum a man at the age above noted will certainly secure to his family some fifty pounds at the period of his decease. The Irish Benevolent Fund, by its last audit, had but two hundred and fifty pounds at its disposal—a sum manifestly inadequate to meet the numerous applications (those of sixty-three heads of families in all) which were made to it. What was this among so many? There can be no possible reason why all medical men should not thus avail themselves of the benefits of assurance; and little doubt, I think, can remain upon the mind as to which procedure is the more eligible—which more conformable to prudence and real charity to survivors. These remarks apply, *mutatis mutandis*, as much to Britain as to Ireland. I am anxious to impress it on the members of our noble profession, not to rely on the slender contingency of a benevolent fund, but upon their own right arms. I would urge all to appreciate the inestimable blessings of life assurance, to practise it themselves, and impress it on those within the sphere of their influence; and I would respectfully solicit the press to yield these my well-meant, and I trust useful, exhortations, a wide-spread diffusion.

HENRY M'CORMAC, M.D.

THE CHOLERA AT VIENNA.

The cholera is very prevalent among the workmen on the railway of Mount Soem-

mering. In Vienna itself it appears to be stationary. Since its reappearance there have been 1019 cases, of which 422 proved fatal, 432 recovered, and 165 were still under treatment. At Pesth, in Hungary, the cholera is still on the increase; but it is particularly in a small town of Moravia, Grosshöflein, that the disease is producing the greatest ravages. In two days it carried off 68 persons—i. e., about 5 per cent. of the population, which does not exceed 1300 persons.

THE CHOLERA AT MALTA AND IN THE IONIAN ISLANDS.

THE intelligence from Malta, coming down to as late a date as the 15th instant, continues very unsatisfactory. The cholera, which in the early part of September was materially on the decline in the island, has subsequently been very fluctuating, as will be seen in the official reports below, which embrace only the civil population; and the disease is no longer, as at first, confined to the lower and indigent classes. According to these returns, there were, on the 1st of September, 10 attacks and 14 deaths; 2d, 13 attacks and 13 deaths; 3d, 7 attacks and 8 deaths; 4th, 19 attacks and 12 deaths; 5th, 16 attacks and 8 deaths; 6th, 16 attacks and 9 deaths; 7th, 36 attacks and 18 deaths; 8th, 13 attacks and 14 deaths; 9th, 49 attacks and 21 deaths; 10th, 20 attacks and 21 deaths; 11th, 32 attacks and 16 deaths; 12th, 20 attacks and 17 deaths; 13th, 17 attacks and 8 deaths; 14th, 10 attacks and 10 deaths; which, added to former reports since the 13th of June, bring up the total attacks to 2,813, and the deaths to 1,502. Among the military there are only occasional cases, confined entirely to the ranks; but the Bellerophon ship of the line, which came in from the squadron then in the offing, on the 9th of September, to fill up water and provisions before leaving for England to be paid off, had four seamen attacked on the 12th, and 16 more on the 13th, several of whom died within a few hours after being attacked, whereupon the Rear-Admiral-Superintendent of Malta-yard, as senior officer, ordered the ship immediately to sea. Vice-Admiral Sir William Parker, accompanied by his family, left Malta in the Queen, with the Terrible in company, on the 8th. The Board of Health at Malta has reduced the quarantine on arrivals from Marseilles to nine days for steam packets and twelve for sailing ships. We learn, *via* Malta, that the cholera had manifested itself at Cephalonia on the 21st of August, and up to the 4th of September there had been 70 cases, of which 34 had been fatal. A report had reached Malta that a suspicious case had

occurred at Zante, Corfu, and the other islands of the Ionian states which were free, had placed Cephalonia under a quarantine of twelve days, which Malta had also adopted. Recent letters from Tripoli and Tunia received at Malta report the former place to be still seriously infected with cholera, but at the latter it had nearly subsided, and those who fled, panic-stricken, had to Malta, Leghorn, and other parts, were returning to their homes.

SICKNESS AMONG THE ENGLISH TROOPS AT HONG KONG, CHINA.

INTELLIGENCE from Hong Kong, dated July 24, states that sickness among the troops has been on the increase: as many as 160 men have been in the hospital at one time, and about 35 have died of fever. This happened when there was no unusual sickness among either the community, or the Government police force. In 1848 the 95th Regiment, from illness and the number in hospital, was declared unfit for duty, and about 100 were carried off in a short space of time. Some attribute the cause of this to the unhealthy position of the barracks, occasioned chiefly by the nature of the ground in the rear. It is more likely, however, to proceed from the crowded state of the rooms occupied by the men, and at night in particular, their beds being only six inches apart—as close, we understand, as they are in an European and cold climate.

Good ventilation and as much air as possible is considered a *desideratum*, and necessary by all in an eastern climate; but the soldiers have been deprived of this necessary of life; air is excluded, and in consequence an obnoxious and a poisonous atmosphere is generated. In the hospital the same evils exist; and we have heard those qualified to judge express an opinion that healthy persons exposed for a short time to the atmosphere that exists in these places, would soon be affected and carried off, as the unfortunate troops are.

OBSTACLES TO THE DIFFUSION OF VACCINATION.

ALTHOUGH the poor are now vaccinated gratuitously, and ample means are provided by the Poor-law Board for carrying out the intentions of the legislature, an unaccountable prejudice still exists against vaccination. Some of the objections to it are excuses for negligence; others are based on a sort of fatalism; but others, again, amount to almost criminal neglect. The Registrar for Nottingham states that a woman in his district who had lost a child, by disease, assured him that she would rather lose half a dozen children by it, than *fly in the face of Providence in having one vaccinated!*

LIST OF ASSURANCE OFFICES WHICH RECOGNISE THE PRINCIPLE OF REMUNERATION TO ALL MEDICAL REFEREES.

[We copy the following list from a contemporary:—]

Architects, 69, Lombard Street, London.
 Britannia, 1, Princes Street, Bank, London.
 British Mutual, 17, New Bridge Street, Blackfriars.
 Church of England, Lothbury.
 Commercial, 112, Cheapside, London.
 East of Scotland, 1, Bank Street, Dundee.
 Engineers, Masonic, and Universal, 345, Strand.
 English and Scottish Law, 12, Waterloo Place, London.
 English Widows' Fund, 67, Fleet Street.
 General and Mining, 4, Bridge Street, Blackfriars.
 General Benefit, 4, Farringdon Street.
 Great Britain, Waterloo Place, and King-William Street.
 Indian and London, King-William Street, and 14, Waterloo Place.
 Industrial and General, 2, Waterloo Place, Pall Mall.
 Kent Mutual, High Street, Rochester.
 Kent Mutual Life Assurance Society, 6, Old Jewry, London.
 Leeds and Yorkshire, Commercial Buildings, Leeds.
 Legal and Commercial, 73, Cheapside.
 London Indisputable, 81, Lombard Street.
 London Mutual Life, 63, Moorgate Street, City.
 London and Provincial, 39, Nicholas Lane.
 Medical, Legal, and General, 126, Strand.
 Medical, Invalid, and General, 25, Pall Mall.
 Metropolitan and General, 27, Regent Street, Waterloo Place.
 Mitre, 23, Pall Mall.
 National Loan Fund, Cornhill.
 National Mercantile, Poultry, Mansion House.
 New Equitable Assurance Company, 450, West Strand.
 North of England, 11, Cheapside, London, and Old Haymarket, Sheffield.
 Professional, 76, Cheapside.
 Prudential, 14, Chatham Place.
 Royal, Royal Insurance Buildings, Liverpool.
 Royal Exchange, Royal Exchange.
 Royal Farmers and General, 346, Strand.
 Scottish Equitable, 26, St. Andrew's Sq., Edinburgh.
 Sovereign, 5, St. James's Street.
 Solicitors and General, 57, Chancery Lane.
 Star, 44, Moorgate Street.
 Westminster and General, 27, King Street, Covent Garden.
 Yorkshire, York..

MEDICAL SOCIETY OF LONDON.

At a meeting of the Council of this Society, held on the 19th inst., the following gentlemen were elected members of the Sectional Committees on Medicine, Surgery, and Midwifery:—

Medicine.—Dr. Webster; Dr. Sibson; Dr. J. R. Bennett; Dr. Copland; Dr. Babington; Dr. Garrod; Dr. Lankester.

Surgery.—Mr. Hancock; Mr. Dendy; Mr. Pilcher; Mr. Erichsen; Mr. Hird; Mr. Hilton; Mr. Toyne.

Midwifery.—Dr. Murphy; Dr. Chowne; Mr. Greenhalgh; Dr. Merriman; Dr. Smiles; Dr. Waller; Dr. Lever.

MEDICAL EDUCATION IN SPAIN.

DURING the year just elapsed there were 19681 students in the Colleges and Universities of Spain. These were distributed in the following order:—

1.	Class of Philosophy	13453
2.	Law	3750
3.	Medicine	1646
4.	Theology	1157
5.	Pharmacy	539
6.	Surgery	144

OBITUARY.

DR. JOHN MARSHALL.

At Falmouth, on the 30th ult., aged 67 years, John Marshall, M.D., for many years a surgeon in the Hon. E. India Company's Service, and late member of the Medical Board, Calcutta, and Physician-General of the Bengal Presidency.

DR. W. BAKER.

On the 30th ult., at Derby, William Baker, M.D., aged 60.

DR. FOGO.

On the 18th inst., at Tiverton, Devon, Thomas Macmillan Fogo, Esq., M.D., late of the Royal Artillery.

M. PREVOST.

It is with regret we announce the death of this eminent chemical philosopher, which took place recently at Geneva. M. Prevost had acquired a high reputation by the researches which he made conjointly with M. Dumas. He was in his 60th year.

DR. WILSON.

At Lahuan, in the Eastern Archipelago, from the effects of a sun-stroke, Dr. Wilson.

Selections from Journals.

A SURGICAL CURIOSITY.—A GOOSE'S LARYNX REMOVED BY TRACHEOTOMY FROM THE LARYNX OF A CHILD. BY PROFESSOR BUROW, KÖNIGSBERG.

DR. BUROW states that a favourite plaything with children in his country is the larynx of a freshly killed goose. It is removed with ten or twelve rings of the trachea, and forms a sort of musical (?) toy, when blown, giving out the cry of the goose nearly the same as during life. A boy, twelve years of age, was amusing himself in this manner, when he suddenly coughed, and the goose's larynx was immediately and violently drawn into his own larynx. He was seized with great difficulty of breathing, which, however, soon in some degree abated. When seen by Dr. Burow, eighteen hours after the accident, he was still suffering distress in his breathing; his countenance heavy; his face congested, and covered with perspiration. Each inspiration was effected by a spasmodic action of the muscles of the neck, and was accompanied by a clear shrill cry like that of a goose. However improbable it seemed, from the relative size of the parts, it was not possible to come to any other diagnosis than that the goose's larynx was impacted in the glottis of the child. The danger with which life was threatened determined Dr. Burow on having immediate recourse to tracheotomy.

When the trachea was opened, it was with the greatest difficulty that the object could be reached by instruments. The patient's trachea was completely filled with mucus, besides which the irritation of the instrument induced a violent fit of coughing, which elevated the larynx out of reach. At last, three rings of the trachea were brought away by the forceps, thus confirming the diagnosis. After several fruitless efforts, the whole of the goose's larynx was removed through the wound in the child's trachea. The patient recovered rapidly from the effects of the operation.

This case was so remarkable in itself, and seemed so improbable, that Dr. Burow, fearing his statement of the case would scarcely be credited, took the precaution of having twelve of his pupils present to witness the operation. — *Casper's Wochenschrift*.

X

MICROSCOPIC EXAMINATION OF THE DISCHARGES FROM THE BOWELS IN CHOLERA.

DR. R. S. HOLMES, of St. Louis, says:—I have examined (microscopically) the discharges from the bowels in six cases

of cholera, and have found the cells of cryptogami in a greater or less degree in four of these cases, and *vibriones* very abundantly in one. The theory I think amounts to nothing. I have found in flour *every one* of the forms of cryptogami that I have been able to discover in cholera cells; one has a peculiar shape, which I have not seen described. I have had a bottle of flour and water on my table for some months, and I am confident I could show in the course of a few days every one of the forms of vegetable growth in it that are seen in cholera discharges, by a Ross one-eighth lens; I say in a few days, for these cells vary in the flour, and are sometimes not to be seen: the cell of the mould of flour precisely resembles that of the smallest of the cholera cells, which is not more than the ¹³⁰⁰⁰th of an inch in diameter, although the peculiar *cholera cell*, so called, seems to have been limited by the English investigators to a much larger cell, with buds upon it.

I may mention that I discovered distinct crystals, having the exact forms of those of lithic acid, in one case where there was suppression of urine.—*American Journal of the Medical Sciences*, July 1850.

BOOKS & PERIODICALS RECEIVED FOR REVIEW

DURING THE LAST TWO WEEKS.

Die Asiatische Cholera und die Gesundheitspflege. Von Dr. C. F. Riecke.

Der Kriegs- und Friedens-Typhus inden Arméen, von Dr. C. F. Riecke.

Results of the Treatment of Fever in the Glasgow Hospitals and Out-door Practice contrasted. By Jas. Adams, M.D.

On a New and Successful Treatment for Febrile and other Diseases. By William Taylor, M.R.C.S. &c.

The Pharmacopœia of the King and Queen's College of Physicians in Ireland. 1850.

Deafness practically illustrated, &c. By James Yearsley, M.R.C.S.E.

Statistics of the Medical Society of London. Report of the General Board of Health on the Epidemic Cholera of 1848-9.

With

Appendix A, by Dr. Sutherland; and Appendix B, by Mr. Grainger.

Boston Medical and Surgical Journal. September 1850

The Monthly Journal of Homœopathy. September 1850.

The Teeth and their Preservation. By Charles Vasey, Dentist.

BIRTHS & DEATHS IN THE METROPOLIS

During the Week ending Saturday, Sept. 21.

BIRTHS.		DEATHS.	
Males....	702	Males....	460
Females..	672	Females..	398
1374		858	

CAUSES OF DEATH.

ALL CAUSES	858
SPECIFIED CAUSES	858
1. Zymotic (or Epidemic, Endemic, Contagious) Diseases....	196
<i>Sporadic Diseases, viz.—</i>	
1. Dropsy, Cancer, &c.	40
2. Brain, Spinal Marrow, Nerves, and Senses	118
4. Heart and Bloodvessels	26
5. Lungs and organs of Respiration	88
6. Stomach, Liver, &c.	44
7. Diseases of the Kidneys, &c.	11
8. Childbirth, Diseases of Uterus, &c.	8
9. Rheumatism, Diseases of Bones, Joints, &c.	7
10. Skin	3
11. Premature Birth	30
12. Old Age	43
13. Sudden Deaths	11
14. Violence, Privation, Cold, &c.	36

The following is a selection of the numbers of Deaths from the most important special causes:

Small-pox	7	Convulsions	21
Measles	10	Bronchitis	20
Scarlatina	30	Pneumonia	17
Whooping-cough	29	Phthisis	11
Diarrhoea	56	Lungs	1
Cholera	5	Teething	4
Typhus	38	Stomach	5
Dropsy	18	Liver	7
Hydrocephalus	30	Childbirth	8
Apoplexy	28	Uterus	3
Paralysis	15		

REMARKS.—The total number of deaths was 154 below the average mortality of the same week of ten previous years.

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer	29.35
" " Thermometer	58.4
Self-registering do.	Max. 60 Min. 31
• From 12 observations daily. • Sun.	

RAIN, in inches, .35.—Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was 0.6 below the mean of the month.

NOTICES TO CORRESPONDENTS.

Troviamo tanta difficoltà nel fare cambio dei giornali, coi nostri contemporanei italiani, che siamo costretti a ricorrere la proposizione contenuta nella lettera del Dottore Nannini, di Venezia.

The papers on the Walker Testimonial fund shall receive our attention.

Mr. C. R. Walsh's communication shall be noticed next week.

The contributions of Mr. Lonsdale and Mr. Kingston will be published in the following number.

CORRECTION.—In Mr. Gallwey's paper, last No., page 494, col. 1, line 11, for "Clinical," read "Chemical."

Lectures.

COURSE OF LECTURES
ON
DISEASES OF THE HEART.

*Delivered at St. Vincent's Hospital during the
Session 1849-50.*

By O'BRYEN BELLINGHAM, M.D.

Fellow of, and Member of the Court of Examiners
of, the Royal College of Surgeons in Ireland, and
one of the Medical Officers of the Hospital.

LECTURE XII.

EXAMINATION OF THE HEART IN DISEASE,
(continued).

General signs of cardiac disease—Countenance in heart disease—Position of the patient—Palpitation—Palpitation depending upon organic disease—Palpitation independent of organic disease of the heart—Characters by which the palpitation depending upon organic disease of the heart is distinguished from that which occurs independent of organic disease—Immediate cause of palpitation—Influence of the coronary circulation.

General signs of heart disease.

THE general signs of the advanced stage of cardiac disease have considerable similarity, and the symptoms most generally present are common to several diseased conditions of the heart. In the majority of cases they have their origin in some impediment to the free passage of the blood through the orifices of the left side of the heart, by which the circulation through the heart, or through the lungs, or through both, is impeded.

Disease of the heart may lead to disease of the lungs, and *vice versa*. In a healthy condition of the parts there is a perfect equilibrium between the development and the capacity of the two organs: the exact amount of blood is transmitted by the right ventricle to the lungs, which, in the latter organs, can be converted into arterial blood; and the exact amount of blood is returned by the pulmonary veins, which can pass freely through the left chambers of the heart. But, if this equilibrium is disturbed; if the pulmonary circulation is obstructed, owing to disease in the lungs, bronchial tubes, or pleura, the right auricle and ventricle become distended, and eventually, if the cause continues in action, dilated or hypertrophied. If, again, there is any impediment to the free passage of the blood through the left side of the heart,

congestion of the lungs, as a natural consequence, ensues; œdema follows, then hæmoptysis or pulmonary apoplexy. The effect of the obstruction of the pulmonary circulation extends to the right side of the heart; its cavities become distended, then dilated, or their walls may become hypertrophied. The great venous trunks not being able to empty themselves, become distended, and the whole venous system congested: hence the lividity of the face and extremities, the engorgement of the lungs, liver, kidneys, and other abdominal viscera, which is partly relieved by the escape of the serum of the blood into the cellular tissue of the extremities, giving rise to anasarca; or into that of the lungs, causing œdema pulmonum; partly by its effusion into the abdomen or thorax, constituting ascites or hydrothorax; and sometimes by the escape of the blood itself upon the congested mucous surfaces, giving rise to epistaxis, hæmoptysis, &c.; or into the pulmonary tissue itself, constituting the state with which we are familiar as pulmonary apoplexy.

The general symptoms of the advanced stage of cardiac disease are pretty constant; while those of its early stage vary as the disease is acute or chronic: they vary likewise according to its nature, and to the parts of the heart engaged. Thus, while in the early stage of the *chronic* forms of disease there is scarcely anything characteristic in the symptoms, those which accompany the early stage of *acute* diseases of the organ are usually very well marked.

Among the general symptoms of heart disease, palpitation is usually one of the first which attracts attention. More or less dyspnoea is generally present at the same time, which may or may not be accompanied by cough, or by pain in the region of the heart: the pulse, likewise, sometimes exhibits diagnostic characters. When the disease has lasted for a longer time, or when obstruction to the pulmonary or the general circulation has set in, other symptoms are usually superadded; signs of congestion of the lungs, of the liver, kidneys, or gastro-intestinal mucous membrane, make their appearance. Orthopnoea supervenes upon the dyspnoea, and the over-loaded or over-distended vessels are relieved in part by the escape of the serum of the blood into the general cellular tissue, or into the serous cavities; or sometimes by the escape of the blood itself upon some of the mucous surfaces, or into the pulmonary tissue.

Before entering upon the consideration of those symptoms it may be well to delay for a moment, to consider whether there is anything in the expression or character of the countenance of a patient labouring

under heart disease, or in the position which he assumes, from which information as to its nature may be gathered. In the acute inflammatory affections of the investing or lining membrane of the heart, information may not unfrequently be obtained by attention to these particulars. In the chronic forms of disease nothing is to be learned from them as long as the disease is recent; but when the disease is advanced they are sometimes sufficiently characteristic. Before the discovery of auscultation and percussion, and before diseases of the heart could consequently be diagnosed with certainty, these points received a much larger share of the attention of practitioners than at the present day: they are, however, of sufficient importance to deserve a short notice.

Countenance in Heart Disease.

In the *acute* inflammatory affections of the lining or investing membrane of the heart the countenance early acquires an anxious and depressed character: indeed, it is often so marked as immediately to attract attention; and from it alone the practitioner has been induced to institute a minute examination into the condition of the central organ of the circulation, although the patient made no complaint of any symptom referable to it. This applies particularly to cases of acute rheumatism, in which the articular inflammation comes to be complicated with pericarditis, or endocarditis, or both. The characteristic expression of the countenance in this and similar affections is explained by the sympathy which subsists between the respiratory nerves of the face and the nerves of the heart.

In the *chronic* forms of cardiac disease little is to be learned at first, either from the expression of the countenance, or from the character of the individual features: in the advanced stage, however, the countenance becomes sometimes almost pathognomonic. Thus, in cases where the pulmonary circulation has been long impeded, the venous system throughout the body becomes congested, the capillaries injected, the extremities swollen and cold, and the nails assume a violet hue. The face becomes puffed, tumid, or bloated, the eyes suffused, the lips and cheeks purple, and the respiration hurried and laborious. On the other hand, it not unfrequently happens that, in an equally advanced stage of cardiac disease, there is neither œdema or swelling of the extremities; the face, instead of being bloated, livid, and purple, is extremely pale, haggard, contracted, and expressive of intense distress. The dyspnoea occurs in fits, during which the respiration is gasping; there is a painful sense

of impending dissolution, or the patient utters exclamations of pain, which is referred to the precordial region, or which shoots from the region of the heart to the spine, or scapula, and extends down the arm.

Corvisart* first made the remark, that in persons of the lymphatic temperament the face, instead of assuming a purple tint, becomes more pale than natural in the advanced stage of cardiac disease. Dr. Hope repeats the same remark: "In persons of the sanguine temperament (he observes) who are naturally florid, the lips and cheeks often become injected, and the countenance bloated; while in individuals of the lymphatic temperament, who are naturally pale, it usually becomes still more so." It appears to me, however, that the original complexion of the sufferer is not necessarily the cause of this difference; but that it depends rather upon the nature of the organic disease of the heart under which the patient labours, and whether this has, or has not occasioned congestion of the lungs. Thus, when it consists in a very contracted state of the mitral orifice, with hypertrophy and dilatation of the right ventricle, in which congestion of the lungs is a common result, the face will be congested and bloated, the lips and cheeks purple, and the lower extremities livid and anasarcaous. On the other hand, in cases of disease of the aortic orifice, or its valves, permitting regurgitation into the left ventricle, in which congestion of the lungs does not necessarily occur, the face is not bloated, the lips and cheeks are not purple, and the extremities are not livid or anasarcaous; but the face becomes very pale, and the countenance anxious, and expressive of severe suffering.

When congestion of the lungs is combined with congestion of the *liver*, the complexion, as Dr. Budd† remarks, "becomes purplish, and at the same time *sallow*." "As the blood, when its passage through the lungs is impeded, is imperfectly *decarbonized*, and gives a purplish colour to the face; so, when its course through the liver is impeded, the blood is not completely freed from the principles of bile, and the countenance acquires a slightly jaundiced or *sallow tint*." This had not escaped the observation of Dr. Bright, who remarks: "When obstruction takes place to the circulation through the chest, but more particularly when the heart becomes over-distended with blood, we observe the countenance gradually assume a dingy aspect, in which the purple suffusion of carbonized blood is mingled with the

* Essai sur le Maladies du Cœur.

† On Diseases of the Liver.

yellow tint of slight jaundice; the conjunctiva is more decidedly tinged, and, if the disease continues long, sometimes completely prevails over the purple tint."

A prominence of the eye-balls, giving the eyes a peculiar staring appearance, as if they were larger than natural, or as if they were protruded forward, has been noticed in a few instances in connection with cardiac disease, or with symptoms simulating it, of which the most prominent was palpitation. It coincided in the majority of cases with an enlargement of the thyroid gland. The subjects were females, often of an hysterical habit; and the attacks of palpitation in some instances were prolonged and severe. Dr. Graves* first called attention to it, and subsequently Dr. Macdonnell published some cases. It appears to have a doubtful connection with cardiac disease. In one case, where the subject of it was an extern patient of the hospital, she had continued for several years much in the same state. In another, who presented the same peculiar appearance of the eyes, but without any enlargement of the thyroid body, and who died in the hospital, a small tumor, growing from the dura mater, was found on a post-mortem examination.

Posture of the Patient.

In the early stage of the chronic forms of cardiac disease nothing is to be learned either from the position of the patient, or from the posture which he assumes. He is usually able to rest in the horizontal posture, and seldom finds greater difficulty in lying upon one side than the other; sometimes he prefers the supine posture: frequently it is with great reluctance that he remains in bed.

In acute affections, more particularly in that form of pericarditis where a large amount of fluid is rapidly effused into the pericardial sac, the patient is quite unable to lie down, or to obtain rest in the horizontal posture. Sometimes the restlessness is so extreme that he cannot remain for more a few minutes in any one position, and he is continually changing it: at others he obtains partial relief only in one particular posture, any deviation from which is sure to add considerably to the dyspnoea and distress.

In the advanced stages of valvular disease, when the circulation through the lungs becomes greatly impeded, or when fluid is effused into one or both pleural cavities, the patient cannot lie down; he must have the head and chest elevated, or he is unable to remain in bed, and is forced to preserve the sitting posture. In extreme

cases he is often only able to snatch momentary rest by leaning forward, and resting his head upon his knees, or upon a table. Sometimes the dyspnoea is so extreme that he obtains ease only by lying on his face: this, however, is more frequently observed in aneurism of the aorta pressing upon the trachea than in organic disease of the heart.

When, in addition, effusion to a considerable amount exists in the peritoneal cavity, or when the lower extremities have become much swollen, the position of the patient will be influenced in some measure by these complications. I have seen the dyspnoea aggravated in a remarkable degree by merely bringing the anasarcoous lower extremities to the same level with the body, and the patient could not rest unless they hung down by the side of the bed. In many such cases, indeed, the patient refuses to remain in bed, and sits constantly in a chair.

In other instances, where there is neither anasarca of the lower extremities nor ascites, but where pain and dyspnoea are the most prominent symptoms, and where the dyspnoea occurs in paroxysms of extreme severity, the position which the patient assumes in the paroxysm is peculiar: he sometimes seizes upon some fixed object against which he strongly presses the sternum; or he fixes the arms firmly, so as to call every muscle of inspiration into action; or he writhes about, and almost screams with agony. A patient was in hospital some time since who, during the paroxysms, was unable to obtain ease in any position but sitting upon the ground with the back firmly pressed against the wall. Dr. Hope has drawn a vivid picture of an aggravated case of this kind:—"Incapable of lying down, the patient is seen for weeks, and even for months together, either reclining in the semi-erect position, supported by pillows, or sitting with the trunk bent forwards, and the elbows or forearms resting on the drawn-up knees."

"With eyes widely expanded and starting, eyebrows raised, nostrils dilated, a ghastly and haggard countenance, and the head thrown back at every inspiration, he casts round a hurried, distracted look of horror, of anguish, and of supplication; now imploring in plaintive moans, or quick, broken accents, and half-stifled voice, the assistance already often lavished in vain; and now, in an agony of despair, drooping his head on his chest, and muttering a fervent invocation for death to put a period to his sufferings."

Palpitation.

Among the general signs of cardiac disease some may be said to be direct, others

indirect: the former have obviously more value than the latter. Among the direct symptoms palpitation is one of the most common; the character of the pulse being also occasionally diagnostic may be included under this head: under it, also, turgescence of the jugular veins, pulsation in those veins, and *fremissement cataire*, may be included: the latter have, however, already been considered. The indirect—or, as they are more correctly termed, the secondary symptoms—are common to diseases of the lungs as well as of the heart. Under this head will be included congestion of the lungs, cough, dyspnoea, orthopnoea, hæmoptysis, pulmonary apoplexy, congestion of the liver, spleen, kidneys, and brain, anasarca, and effusion into the cavities of the pleura and peritoneum.

Among the direct signs of cardiac disease palpitation is perhaps the most constant and most frequent: it is often the first symptom which attracts the patient's attention, and it frequently distresses and harasses him throughout every stage of the disease, or until its final close. On the other hand, palpitation is frequently a prominent and troublesome symptom when the heart is in a perfectly healthy state.

It has already been said that, in the healthy subject, where the chest is well formed, and the heart and lungs have their normal proportions, the impulse of the apex of the heart is so slight that the individual is not sensible of it himself: the motion, in fact, is performed without his consciousness, and is only perceived in a very limited space when the hand is laid upon the parietes of the chest. Whenever, consequently, a person becomes sensible of the beating of his own heart, it may be said to constitute palpitation. Palpitation may therefore be defined:—An unusual action of the heart, perceptible to the individual, and inconvenient to him (though the latter is not essential), the impulse being usually stronger than natural, and the action of the organ often more rapid than in health, and occasionally intermittent or irregular.

Palpitation presents numerous varieties: sometimes it is so slight that the patient is unconscious of it, or makes no complaint unless questioned; at other times it is so violent as to elevate the clothes, and shake the bed upon which the patient lies. Sometimes the impulse communicated is a slight, rapid shock; sometimes it is strong, prolonged, and heaving, elevating the head of the observer; at other times the impulse is not altered, or is even weaker than natural. Sometimes the sounds of the heart are increased in intensity; and, in rare cases, so much, that the patient can hear them himself when he lies upon the left side; at others, the sounds are not at all increased

in intensity. In some cases palpitation sets in suddenly; in others, it makes its approaches very gradually. Sometimes the fits are short, quickly subside, and the patient is perfectly free from it in the intervals; at others, the fits are longer and more severe, or more or less palpitation is constantly present. Sometimes the palpitation is accompanied by uneasiness, by a sense of constriction, or of weight, or pain in the region of the heart, or by a sense of sinking or of fluttering in the epigastrium; sometimes by dyspnoea or orthopnoea; sometimes by headache, vertigo, or noises in the head: at others, nothing of the kind is observed, and palpitation is the only symptom we are called upon to treat.

Palpitation, although one of the commonest symptoms of morbid states of the investing or lining membrane of the heart, of its valves, orifices, or muscular tissue, frequently occurs independent of any organic change in this organ, and is not unfrequently a greater source of anxiety or alarm to the patient under such circumstances than in cases of organic disease. Thus it is a symptom in plethora, and in the very opposite condition—*anæmia*: it is a common attendant upon dyspepsia and derangement of the digestive organs, and it arises in debility from any cause: it is a frequent symptom in hysteria and hypochondriasis: it is not unusual in the early stages of pregnancy, and it accompanies several diseased states of the pulmonary organs.

The value of palpitation as a symptom mainly depends upon whether it is *symptomatic* of diseased states of the heart, or whether it is *sympathetic* of some other affection: hence it is of considerable importance that its cause should be determined. We shall therefore consider it here under two heads—viz., as it has its cause in organic disease of the central organ of the circulation, or as it occurs *independent* of cardiac disease.

Palpitation depending upon organic disease of the heart.—Palpitation depending upon organic disease of the heart usually, unless in cases of acute inflammation of the investing or lining membrane of the organ, comes on slowly and insidiously, and increases gradually: it is constant, although more distressing at one period than another: it is very generally combined with derangement of the general circulation, or with disorder of the respiration; and it is very generally also accompanied by well-marked physical signs, or by other general symptoms of cardiac disease. Palpitation will therefore be a symptom in pericarditis and endocarditis, in hypertrophy, and in dilatation of the ventricles, or when these two states are combined; in

diseased conditions of the valves or orifices of the left side of the heart, causing obstruction, or permitting regurgitation; in adhesion of the pericardium, the result of pericarditis; and in softening of the heart.

Thus, in acute pericarditis, or endocarditis, palpitation is often one of the first symptoms which attracts the attention of the patient or practitioner; the heart's action is more rapid than natural, and the impulse is increased. Cases, however, occasionally occur where the palpitation is so slight as scarcely to attract attention. In adhesion of the pericardium the result of pericarditis, more or less palpitation is generally likewise constantly present.

Again, palpitation is one of the commonest symptoms of hypertrophy of the ventricles: here the impulse is increased, often considerably, but the action of the heart is not more rapid than in health. In dilatation of the ventricles the palpitation is accompanied by increased loudness of the heart's sounds, but the impulse has not much real strength. In the combination of these two states, or hypertrophy with dilatation of the ventricles, particularly when the disease is advanced, the palpitation is often very distressing, and the paroxysms are frequently prolonged and severe.

In organic disease of the valves or orifices of the left side of the heart palpitation is very generally and constantly present; but, as they are usually accompanied by hypertrophy, with or without dilatation of the ventricles, and as they always tend to produce these morbid states, the palpitation has its cause partly in this complication. Finally, in softening of the muscular tissue of the heart, palpitation is frequent: here, however, it is of the passive kind; the impulse is feeble, and the heart's action usually intermittent and irregular.

Palpitation independent of organic disease of the heart.—Palpitation independent of organic disease of the heart is more common in the female than the male: it usually sets in suddenly, and is almost always intermittent. The impulse is often sharp and knocking, but never heaving or prolonged, and the action of the heart is generally rapid. The palpitation comes on in paroxysms, often when the individual is in a state of repose: it is readily excited by mental emotion; and the physical and general signs of valvular or other disease of the heart are absent.

The palpitation which occurs under these circumstances presents several varieties, and may be traced to several causes, some of which are intrinsic to the heart, others are extrinsic to it. Thus palpitation will ensue when the blood is returned to

the heart more rapidly than in ordinary, or when the motions of the heart are impeded, whether the exciting cause be seated in the lungs, in the parietes of the chest, or in the abdomen. Palpitation occurs in states of debility, whether this depends upon excessive evacuations, or arises in convalescence from acute disease; as well as in certain states of the nervous system, whether this be constitutional or acquired. Finally, it may ensue when two or more of the foregoing conditions are combined.

Thus, 1st. In a healthy subject, violent exercise on the one hand, or the indulgence in intoxicating liquors on the other, will hurry the circulation, and give rise to palpitation. But, in such cases, as soon as the exciting cause is removed, the effect ceases. In states of plethora, likewise, where blood is rapidly formed, and the vascular system becomes overloaded or oppressed, more or less palpitation is generally present.

2ndly. In cases where the bony parietes of the thorax are contracted and deformed; or in adults whose chests are narrow and extremities long; or in young persons about puberty, whose growth has been rapid, palpitation is often complained of. Here its cause appears to lie in the narrowness of the chest, which interferes with the free play of the heart, or with the free expansion of the lungs. Palpitation is also occasionally complained of in the advanced stage of pregnancy, in cases of large ovarian or other abdominal tumors, or in ascites, in which the descent of the diaphragm is interfered with, or pressure is exercised upon the large vessels.

3rdly. Palpitation is a very frequent symptom in states of debility, as in anæmia, whether this arises from profuse hæmorrhage or accompanies amenorrhœa; or where profuse evacuations of any kind have occurred; under this head sexual excesses and menstruation may be included. Palpitation likewise is a symptom of mercurial erithismus; and it not unfrequently arises during convalescence from acute diseases, particularly where mercury had been freely given, and after protracted fever it is by no means rare.

4thly. In persons of what is called the nervous temperament, palpitation is common, and is readily excited by mental emotion, or by trifling causes. In hysteria, particularly in that form which gets the name of spinal irritation, it is an ordinary symptom; and in cases of hypochondriasis it is by no means unfrequent.

5thly. Palpitation is not unfrequently induced by excessive study, by late hours, by the habitual use of strong tea, or the inordinate use of tobacco in the form of smoking; by the suppression of some

habitual discharge; by prolonged mental excitement, whether of an exciting or depressing nature; or by a sudden shock or fright. In these cases it partly depends upon derangement of the digestive organs, and partly upon debility or constitutional or induced irritability.

Lastly, palpitation is a common accompaniment of various forms of dyspepsia: here it is partly due to flatulence, which, by resisting the descent of the diaphragm, impedes the free expansion of the lungs, and interferes with the motions of the heart; partly to the reflex sympathy between the parts concerned. Under this head the palpitation which sometimes occurs in persons of a gouty habit, and which

is occasionally so distressing, may probably be included.

It not unfrequently happens in practice, however, that organic disease of the heart is associated with derangement of the digestive organs; with an anæmic state of the system; with a nervous and irritable habit; or with some of the other causes capable of giving rise to palpitation; by which the difficulty of the diagnosis will be considerably increased.

In the following table the more prominent characters of the palpitation, which depends upon organic disease of the heart, are contrasted with those of palpitation arising independent of disease of this organ:—

Palpitation depending upon Organic Disease of the Heart.

1. More common in the male than the female.
2. Palpitation usually comes on slowly and gradually.
3. Palpitation constant, though more marked at one period than at another.
4. Impulse usually stronger than natural; sometimes remarkably increased, heaving, and prolonged; at others irregular and unequal.
5. Percussion elicits a dull sound over an increased surface, and the degree of dullness is greater than natural.
6. Palpitation often accompanied by the auscultatory signs of diseased valves.

7. Action of the heart regular, irregular, or intermittent; not necessarily quickened.

8. Palpitation often not much complained of by the patient; occasionally attended by severe pain, extending to the left shoulder and arm.

9. Lips and cheeks often livid; countenance congested; anasarca of lower extremities common.

10. Palpitation increased by exercise, by stimulants and tonics, &c.; relieved by rest, and frequently, also, by local or general bleeding, and an antiphlogistic regimen.

Palpitation independent of Organic Disease of the Heart.

1. More common in the female than the male.

2. Palpitation usually sets in suddenly.

3. Palpitation not constant, having perfect intermissions.

4. Impulse neither heaving or prolonged; often abrupt, knocking, and circumscribed, and accompanied by a fluttering sensation in the præcordial region or epigastrium.

5. The extent of surface in the region of the heart, which yields naturally a dull sound on percussion, is not increased.

6. Auscultatory signs of diseased valves absent; bruit de soufflet often present in the large arteries, and a continuous murmur in the veins.

7. Rhythm of heart usually regular; sometimes intermittent; its action generally more rapid than natural.

8. Palpitation often much complained of by the patient; more readily induced by mental emotion, and frequently accompanied by pain in the left side.

9. Lips and cheeks never livid; countenance often chlorotic; anasarca absent, except in extreme cases.

10. Palpitation increased by sedentary occupations; by local and general bleeding, &c.; relieved by moderate exercise, and by stimulants or tonics, particularly the preparations of iron.

Immediate Cause of Palpitation.

The blood which circulates through the cavities of the heart is considered to be the natural stimulus to the contractions of the organ: thus, in a state of health, when this fluid is returned to the right side of the heart more quickly than in ordinary, its action becomes more rapid. In this way exercise increases the frequency of the heart's contractions, and hurries the circulation, which returns to its normal fre-

quency after the patient has remained at rest for a time.

In states of plethora, in which blood is rapidly formed, palpitation we have seen to be a frequent symptom; there is here an increased amount of blood in the vessels, and this fluid is of a more stimulant nature, perhaps; the palpitation which ensues in these cases is not accompanied by quickened action of the heart. In the very opposite condition to this—viz., anæmia—where the blood is poor and watery, and

deficient frequently in quantity, palpitation is also extremely frequent, and the heart's action is more rapid than in health: here the heart appears to contract more frequently, to compensate for the deficiency in the quality of the blood.

Again, when the valves or orifices of the heart are diseased, and the passage of the blood out of or through its chambers is impeded, palpitation ensues; the organ here is excited to increased action in order to overcome the obstacle to the transmission of the blood. Increased action leads eventually to increased nutrition; the parietes of the ventricles become hypertrophied under such circumstances, and the palpitation more marked.

When the parietes of the ventricles are thinned, or when the muscular tissue of the heart is softened, the contractile power of the ventricles is diminished; they are too feeble to propel the blood contained in their cavities, which become distended or overloaded: here the palpitation is of the passive kind; for although it is often distressing to the patient, the impulse of the organ is frequently too feeble to be felt, and its rhythm is often intermittent or irregular.

In all these cases the explanation of the occurrence of palpitation appears to be easy: this phenomenon is, however, frequently witnessed independent of any of the foregoing circumstances, as in mental emotion of various kinds: here the nervous system is generally considered to be the exciting agent, though the exact *modus operandi* is not quite clear.

Influence of the Coronary Circulation.

In considering the immediate cause of palpitation, writers do not appear to me to have taken sufficiently into account the coronary circulation, although there can be no doubt that it performs an important part, and that derangement of its circulation will be followed by derangement of the functions of the heart equally as when the circulation through the chambers of the organ is impeded or deranged.

The integrity of a muscle, and its healthy action, depend mainly, we know, upon its receiving a due supply of arterial blood: if this is sufficient, and of a good quality, the functions of the muscle will be performed with vigour; while, if the supply is too small, or if its quality is deteriorated, the functions of the muscle will be impaired. Now, the heart being a muscular viscus, requires, for the vigorous performance of its functions, an efficient supply of arterial blood: hence, although the blood which circulates through the chambers of the heart is the ordinary stimulus to the contractions of the organ, if the coronary arteries which convey the materials for its

nutrition carry it in insufficient quantity, or of a deteriorated quality; or if, on the other hand, it is conveyed in increased quantity, or is of too stimulating a quality, the functions of the organ will suffer.

Thus, in *anæmia*, the quality of the blood being deteriorated, and a given amount containing less of the materials of nutrition, it is necessary that a greater quantity should pass through the coronary vessels in a given period, and the action of the organ is accelerated in order to compensate for the deficiency. Again, in states of *plethora*, where blood is rapidly formed, the heart is excited to increased action, very probably in consequence of the stimulant nature of the blood which circulates in its vessels, and its increased amount.

Lastly, in cases of valvular or other disease of the heart, where considerable obstruction exists in the pulmonary circulation, and the venous blood does not undergo the necessary changes in the lungs, the coronary arteries will convey a mixture of venous and arterial blood to the tissue of the heart; the functions of the muscle become impaired, its irritability diminishes, and it is no longer obedient to the stimulus of the blood which distends its cavities: hence its action becomes irregular and unequal;—indeed, in the advanced stage of valvular or other disease of the heart where the pulmonary circulation is greatly impeded, the cessation of the heart's action, and the death of the individual, is probably owing immediately, in many cases, to the coronary arteries conveying venous blood to the tissue of the heart.

Before dismissing the subject of palpitation, I may observe that this symptom frequently is present in a marked degree in the class of patients who present themselves at hospital, without their appearing to be conscious of it. When questioned, they allow that they feel some oppression in the præcordial region, but hardly admit that it amounts to palpitation, though, when we come to examine the chest, the action of the heart is often much increased, and its impulse sometimes so strong as to raise the head of the observer. This, no doubt, arises in some measure from the sensibility being blunted by the ill oxygenation of the blood, and in some measure, also, from the parts having had time to accommodate themselves to their altered condition. On the other hand, it is no less remarkable how often, when palpitation depends simply upon functional derangement, the patient's attention is directed mainly to it; he is most unhappy in consequence, and can scarcely be made to believe that he is not the subject of organic, and therefore, in his opinion, of incurable, disease of the heart.

Original Communications.

REMARKS ON THE
VITAL STATISTICS OF THE
BOROUGH OF PLYMOUTH,
FOR THE YEAR ENDED ON THE 30TH OF
JUNE, 1850.

BY DR. W. HAMILTON.

[Continued from p. 536.]

THE births in the summer of 1848-9 and 1849-50 were 349 and 354, being an increase of 5 in the latter. In the autumn quarter this increase was still greater—from 391 to 411, or 20, giving a nett increase for the half year of 25. In the winter quarters, however, the numbers were 449 in 1848-9, and 409 in 1849-50, indicating a balance of 40, or 60 per cent., beyond the previous increase, against the first quarter, of the last half year of 1849-50.

On carrying our comparison to the spring, or last quarter of each year, we find the births in 1848-9 to have been 455, and in 1849-50 only 397, showing a farther reduction of 58, or 18 beyond the last, being a farther reduction of 40 per cent. The total falling off of the births in the half year, in which not a trace of cholera could be detected, of 98, is two more than the former computation gave,—a difference trifling in itself, but readily accounted for by the excessive number 8 which was rejected for two births upon that occasion, while 6 only would have been nearer the truth.

Upon a comparison, indeed, of the aggregate births of the two years, we find the balance against that just ended to amount only to 73, a circumstance fully accounted for by the actual increase of 25 during the first half. But this in no manner affects our inquiry, the object of which was to determine the probable number of women who were carried off between the third and ninth months of their gestation, whom we may now fairly assume to have numbered at least 98, or 0·95 per cent. of the whole number living at the commencement of cholera, between the marriageable ages of 18 and 50.

That this is no visionary opinion, which rests only upon the unsubstantial

basis of theoretical calculation, appears to be corroborated by a fact communicated to me upon what I regard as unquestionable authority, of parturition having actually taken place while the mother was labouring under the double affliction of cholera, and this in more than one instance, although I have been unable to ascertain the precise number.

But although these cases corroborate Dr. Shapter's statement with respect to Exeter,—that cholera did not in every instance prove fatal during pregnancy—they at least justify the opinion that the deficiency of 98 births on a comparison of the six last months of the two last years with each other, arose in great part, if not entirely, from an equal number of mothers having sunk under the violence of the pestilence before nature had succeeded in relieving them from their burden,—a number equal to above 58 per cent. of the entire number that died.

Dr. Snow, of Frith Street, Soho, in a valuable pamphlet on cholera, published last year by Mr. Churchill, of Princes Street, Soho, ascribes cholera to a local affection of the alimentary canal, arising from a poison introduced into it through the medium of water contaminated by percolation from sewers and cesspools. But such contamination has existed, more or less, for a period beyond human remembrance without producing any disorder analogous to cholera, in the character of an epidemic, of sufficient importance to attract the attention of the world, and sweep with the besom of destruction so vast and so fair a portion of our globe.

Although Dr. Snow's opinion is the result no doubt of attentive and extensive observation, and although the introduction of such a deadly poison was powerfully calculated to aggravate, if not create the disease, yet there is something in the peculiar features of the cholera, and, in the absence of all medical record of its existence in this country previous to 1832, when its course to our shores was marked in unmistakeable characters across the vast continents of Asia and of Europe, we may perhaps be justified in referring it to some obscure and yet undiscovered origin. The unwholesome water may have created the predisposition, but the latent cause was wanting to call the slumbering malady into mischievous and active existence.

The same objection and the same reasoning also appear to another cause which he assigns for its production,—namely, the want of personal cleanliness; but powerful as this exciting cause is in the production of typhus, and the other common forms of epidemic, the proximate cause of cholera, in its Asiatic or malignant form, was yet wanting to call into being that hitherto unknown malady. The deadly weapon of mortality might have been loaded to its very muzzle, but the percussion cap of cholera was yet wanting to produce the explosion, and propel the morbid poison on its pestilential mission. Similar objections apply with equal force to all the other causes assigned for this destructive and mysterious malady, whose latent origin has yet been veiled from human eyes.

That want of personal cleanliness prepares for its reception, and aids its dissemination after the specific poison has been called into actual existence, as well as augments its virulence, is abundantly proved by Dr. Snow, who quotes, in confirmation of his own experience, that of Dr. Lichenstädt, who informs us, in a work which he published on cholera in 1831, and of which notice is contained in the 37th vol. of the Edinburgh Medical and Surgical Journal, that—

“At Berditscher, in Volhynia, a place of a few thousand inhabitants, no less than 900 were attended in 31 days. Among 764 of these were 658 Jews”—above 86 per cent.—“and only 106 Christians,”—or somewhat under 14 per cent.—“although the Jewish population is far from being proportionally so great; and among the Christians attacked the deaths were 61·8 per cent., while among the Jews they were 90·7 per cent. The only reason assigned by the reporter for these extraordinary differences is the excessive disregard of clean linen among the Jewish inhabitants.” And to this extract from Dr. Lichenstädt Dr. Snow adds, that—

“The first appearance of cholera in many towns of this country in 1832 was in courts and alleys to which vagrants resort for a night's lodging, where it often lingered for some time before spreading to the more cleanly part of the people,” p. 10.

With the Jews of Plymouth, however, from the superior comforts they enjoy, and the superior attention they pay to

domestic and personal cleanliness, the reverse was the case; and in the recent attack they enjoyed an exemption to a remarkable extent, only two of their number having been attacked, neither of whom died. Neither did cholera make its first appearance among us in those localities in which, both for their accumulated filth, and the character of their inhabitants, it might have been expected, had it originated from the ordinary causes of epidemics. In 1832 it made its first appearance at Coxside, in the vicinity of Sutton Pool; and in 1849 in Union Street, both undoubtedly low situations, hardly elevated above the ordinary high water mark, but neither, the latter more especially, admitting of a shadow of comparison with any of those localities in which it afterwards raged with the greatest fury. What makes this circumstance perhaps more remarkable is, the fact recorded in the Registrar's Annual Statistics for 1849, in which, in a note upon the 22 deaths from cholera, 12 of which took place in the June quarter, we are told that—

“The cholera cases reported in the three first quarters are either infantile or English cholera. Of the 12 cases that occurred in the last (June) quarter 9 occurred in the Sound, among the passengers and crew of the ‘American Eagle,’ emigration ship; the remaining 3 were those of an Irish emigrant in Stonehouse Lane, the man from Noss, and another in Leaze Street.”

And in the Quarterly Report of the Births, Deaths, and Movement of the Population for the June quarter of 1849, which appeared in the Plymouth Journal, it is remarked that—

“The health of the town has been steadily and progressively improving, and the mortality proportionably decreasing throughout the quarter, notwithstanding the destructive ravages of cholera in other parts of the country, and even in our immediate vicinity. On the 18th of April 8 cases, 2 of which terminated fatally from neglect of the premonitory symptoms, occurred in a narrow and filthy locality, close to the waterside in Stonehouse, known by the name of Water Lane; but the disease did not spread farther, notwithstanding the peculiar fitness of the locality for festering and diffusing contagion.

“An emigrant ship, bound to the United States, put in here for medical

assistance, in consequence of cholera having appeared among her passengers; but, notwithstanding the temporary alarm produced by her arrival, and the subsequent occurrence of a few deaths on board, the prompt assistance given, and the active measures adopted by the public and municipal authorities, soon arrested the progress of the destroyer, and the vessel has proceeded on her voyage with a clean bill, and without leaving us any evil recollection of her visit.

"Two deaths, we understand, occurred from cholera; one in our hospital, near Princes Square, the other in Stonehouse Lane; but, in both cases, the victims were strangers to the town,—the one a man from Noss, who brought the malady with him from that miserable and filthy village, and the other an emigrant, who arrived while labouring under the malady. In both these cases, from neglect of the premonitory symptoms, medical aid came too late, and man could only look on and lament his want of power to avert the catastrophe. In none of these cases, however, did the mischief extend beyond the immediate victims themselves, as might have been expected, had cholera partaken of the character of variola, rubæola, and other disorders which experience teaches us to regard as contagious."

Thus, not only had we the complaint raging within our waters in the dense community of an emigrant ship, from which some of the victims were brought on shore to undergo the hazardous investigations of an inquest, and lay their remains within our soil, but even a spark fell amidst the accumulated combustibles of Stonehouse Lane without producing a conflagration. But when the flame burst out it was not in the spot where the spark had fallen, and where the materials for combustion were ready for ignition; but in a different and far less insalubrious locality, where no materials were prepared for its reception, and where its appearance was least of all to be apprehended.

Again, the case of H.M.S. Apollo, in which cholera broke out without an apparent cause after she had been a week at sea, as reported in a letter from Mr. Charles Kidd, of the 26th July last, inserted in the *Medical Times* of the 3d of August, spreads a darker veil of mystery over the origin of cholera than ever; and seems to show that most, if

not all, of the causes to which we have been accustomed to ascribe it, have obtained their reputation from a fortuitous coincidence, rather than any real connection,—as will appear when we come to inquire into the atmospheric phenomena to which some visionary imaginations have referred it.

Mr. Kidd, in his letter, acquainted us that he saw the 59th regiment, embarked in the Apollo, "leave Ireland without a man sick,—the men, in fact, picked out for their general health,—and proceed to sea. When a week from shore they were attacked by cholera in its worst possible form, the disease, from the 18th of June to the end of that month [when, I believe, they were only off Teneriffe], and all through July, up to the 11th of August, when they sighted the coast of South America, wearing one frightful shape, and cutting down the men with little distinction. Out of 512 men, no fewer than 320 were on the sick list; and Dr. Frazer, who kindly furnished us with the particulars, seems to have known true Asiatic cholera, from long acquaintance with it in the East. The vessel arriving off Rio Janeiro, you will recollect, and the cholera still cutting down the troops, who crowded about the decks cheerless and spiritless, their eyes sunken in their sockets, and their faces pale and ghastly. They were disembarked on an island, lent for the purpose by the government of Brazil, when the cholera, as if by magic, at once disappeared. My impression at the time, from this single fact," continues Mr. Kidd, "was that it was true Asiatic cholera, from causes merely sporadic." Amid such a diversity of opinions, and such a multiplicity of conflicting facts, it would ill become me to offer a dogmatic opinion. But, in the hope of throwing some glimmering of light on a subject so enveloped in darkness, I have been led to place in juxtaposition such opinions and facts as were within my immediate reach, leaving the conclusions to those more competent to form them.

Cholera, having once effected a firm lodgment in the town, committed, as might have been anticipated, the greatest ravages in those localities in which the soil was best adapted to foster and retain it; where the filthy, dissolute, and intemperate habits of the inhabitants, their extreme poverty and wretchedness, the density with which they were

crowded together in a small and unwholesome space, joined to the most defective sewerage and ventilation, singularly predisposed, them for the reception and propagation of disease. What these particular localities more especially were it would be superfluous now to point out, since they are sufficiently known to the medical and municipal authorities, upon whom the responsibility devolves of guarding against future invasion.

With the exception of these localities, however, in which filth, poverty, and vice severally contended for ascendancy, I cannot find that cholera was influenced by any particular temptations in its choice of the localities from which its victims were culled.

Selecting from the statistical tables of the registrars twelve of the most salubrious localities in each of the two registration districts, corresponding as closely as possible in the amount of their mortality, their elevation above the sea, and other circumstances, in order to ascertain the effect of altitude, dryness, ventilation, &c. upon their respective mortality, I found that the 19 selected in St. Andrews gave me 74 deaths in 72 days; the 12 in Charles, 46 deaths in 38 days, marking a somewhat greater amount of intensity in the latter.

To present in a more intelligible form the amount of concentration or diffusion in each of these, I shall enumerate the localities, assigning to each its proportion of mortality and the date of its occurrence, commencing with those in which it was greatest in each of the two divisions; after which I shall exhibit them, with their corresponding elevations above the mean level of the sea, in a tabular form.

And first, in St. Andrews, Morley Place and Lanes head the list with 11 deaths on 10 days—namely, on the 15th and 17th of July, and the 15th, 19th, and 30th of August, a single death on each of these days; 2 deaths on the 12th of September; a single death on the 16th, 21st, and 25th of that month, and on the 18th of October. Cambridge Street and Lanes follow next, with 10 deaths in 9 days—namely, 1 each on the 18th of July, and on the 12th, 22d, 28th, and 29th of August; 2 on the 8th of September, and 1 each on the 11th and 13th of October and 3d of November. Claremont Street, 9 deaths in as many days—namely, on the 20th of July, 12th,

13th, 14th, and 17th of August, and 9th, 11th, 12th, and 17th of September. West Hoe and Sandy Cove, 9 deaths in 7 days—namely, 1 each on the 25th, 26th, and 31st, and 2 each on the 28th and 29th of July; and 1 each on the 18th and 19th of September. Frankfort Street, 8 deaths in as many days—namely, 12th and 15th of August, and 6th, 12th, 16th, 18th, 23d, and 28th of September. York Street and Lanes, 7 deaths in an equal number of days—namely, 19th and 23d July; 24th, 27th, and 29th of August; 6th of September, and 29th of October. William Street and Lanes, 5 deaths in as many days—namely, 16th of July; 11th, 15th, and 28th of August, and 25th of September. The west or St. Andrew side of Cobourg Street and Lane, 5 deaths in as many days—namely, 11th, 25th, 28th, 29th, and 30th of August. Eldad Place, 4 in as many days—namely, 22d, 23d, and 29th of July, and 11th of September. Grosvenor Place and Melbourne Street, 2 deaths; 1 on the 15th of July, and 1 on the 24th of August. George Place and Mulgrave Place the same, on the 24th and 31st of August; and Whimble Street the same, on the 22d of August and 24th of September.

In Charles district we have Gasking Street, occupying the declivity between Gascoyne Terrace and Higher Street, where cholera appeared in its most abrupt and concentrated form; and hence possibly deriving an amount of insalubrity from which its rapid descent might have been expected to exempt it, furnishing 10 deaths in the brief interval of 4 days, coinciding closely with the mortality in Higher Street—namely, 4 on the 16th, 3 on the 18th, 1 on the 19th, and 2 on the 24th of September. Ebrington Street and Place, and Garden Street, 8 deaths in 6 days—namely, on the 16th, 17th, and 18th of September 1 each, on the 19th 2, on the 21st 1, and on the 29th 2. John Street and Lane, 6 deaths in as many days—namely, the 28th and 31st of August, and the 2d, 7th, 10th, and 11th of September. Green Street 5 in 5 days—namely, 5th, 18th, 22d, and 26th of August, and 22d of September. Treville Street 4 in 4 days—namely, the 16th of July, and the 4th, 7th, and 11th of October. Gibbon Street 3 in 3 days—namely, 3d of August, and 8th and 10th of October. Gilwell Street the same—namely, the 7th and 18th of August, and 27th of September. Clarence

Street 1 on the 2d of September. East side of Cobourg Street 2 in 2 days—namely, 22d and 23d of September. Buckwell Street the same—12th of August and 26th of September. Borough prison, nearly on the highest and most airy part of the town, isolated from other

buildings, and but newly completed, 1 death on the 8th of August; and Saltash Street 1 on the 18th of July. In the following table the altitudes are taken from the mean level of the sea, and were roughly deduced from observations made with the aneroid barometer.

Table of Mortality—Intensity of the under-mentioned Places.

DISTRICT OF SAINT ANDREW.				DISTRICT OF CHARLES.			
	Deaths.	Days.	Altitude.		Deaths.	Days.	Altitude.
Morley Place & Lanes	11	10	70	Gasking Street	10	4	73
Cambridge Street & Lanes, 102 . .	10	9	90	Ebrington Street & Garden St. .	8	6	79
Claremont St. 102 .		9	90	John Street and Lane		6	99
West Hoe (& Sandy Cove)	9	7	110	Green Street . .	5	5	82
Frankfort Street .	8	8	46	Treville Street .	4	4	
York Street . . .	7	7	118	Gibbons Street .	3	3	82
William Street, 82 .	5	5	82	Gilwell Street . .	3	3	82
Cobourg Street . .	5	5	100	Cobourg Street .	2	2	100
Eldad Place, 82 . .	4	4	82	Buckwell Street .	2	2	
Grosvenor Place & Melbourne Street	2	2	82	Borough Prison .	1	1	239
George Place and Mulgrave Place .		2		Clarence Street .	1	1	82
Whimble Street . .	2	2		Saltash Street . .	1	1	80
Total	74	70		Total	46	38	

Hence it appears that in an equal number of places corresponding as nearly as possible in elevation and other particulars, the mortality was nearly equal, being at the daily rate of 1·06 in St. Andrews, and 1·2 in Charles.

When I resided in Oxford Street in 1892, one of the most airy and driest streets of the town, running from east to west along the ridge of the acclivity up which Cambridge and York Streets ascend, at an elevation of more than ninety feet above the mean level of the Sound, two deaths from cholera took place in that street, neither of the victims of which belonged to those classes whose confined dwellings and extreme poverty rendered them peculiarly obnoxious to attack. On the contrary, they were both ladies moving in the higher classes of society, living in well-ventilated houses, and surrounded by all the abundances, comforts, and conveniences of genteel society. Of these deaths, one took place in the house adjoining my own, both of which, standing on the north side of the street, and fronting down Cambridge Street, en-

joyed, without the possibility of interruption, the fullest play of the sea breeze from the south as it swept over the Hoe from the waters of the Sound. The circumstances attending the death of this lady are sufficiently remarkable to deserve notice here.

Mrs. Fortescue, the lady in question, was, as I should judge from her appearance, between forty and fifty years of age, in good health, and had two sons; the eldest I should imagine about four-and-twenty, and in holy orders. This lady, about the time that cholera was at its height, took a walk out one evening with some friends, and returning home through Bilbury Street passed the funeral of a poor person whose coffin was conveying on a bier to the burying-ground of Charles, having recently fallen a victim to the prevailing pestilence. On passing at the distance of some feet from the funeral procession, when nearly abreast of the coffin, Mrs. Fortescue exclaimed with a shudder, "Faugh! what a horrible stench." This observation attracted but little notice at the time, since the stench of which she complained was

unperceived by any other of the party; and the circumstance would have soon been forgotten but for the results which followed.

Soon after her return to her own house, Mrs. Fortescue, who had no recollection of any other exposure to the unknown causes of the malady, and who up to that moment had conceived herself to be in the enjoyment of the most perfect health, and had no mental apprehension which might have predisposed her to the reception of the complaint, became conscious of the presence of unusual symptoms for which she was unable to account. These rapidly increased, until, in a very brief space of time, cholera stood fully revealed in all the unmistakable horrors of its most malignant form; and before the dawn of another day arrived, she was herself added to the number of helpless victims who had fallen beneath the axe of the destroyer.

After the performance of the last offices of piety to the sad remains of their surviving parent, her sons gave up the house which had witnessed their loss, and removed from Plymouth, since which time I have not seen or learned anything about them.

Here, then, was a case of cholera immediately succeeding exposure to a narrow stream of pestilential gas escaping from some unclosed openings in the coffin containing a body in a state, no doubt, of rapid decomposition, and not to be traced to any other of the causes to which it has been commonly assigned, and occurring in a locality the least favourable to the production of what are regarded as diseases of contagion, but singularly in accordance with a statement contained in the Medical Times of the 3rd of August, 1850, which shows that cholera is little affected by climate or elevation, and passes without difficulty those limits which the worst forms of typhus have never yet been known to transgress. The statement is as follows, at p. 126:—

“According to letters received from the Andes, it would appear that cholera was prevalent at Bogotá, an elevation of 7,800 feet; this is higher by 6,000 feet than the point beyond which yellow fever has never passed in these countries.”

Hence it is clear that the proximate cause of that peculiar form of mortality, of which the only authentic account

dates back less than three-quarters of a century, and which has been so fatally known among ourselves by the name of the Asiatic or malignant cholera, is yet to be sought for among those capricious mysteries of nature which Heaven has, no doubt wisely, been pleased to place as yet beyond the grasp of human penetration.

But whatever that latent cause may be, not a doubt can be entertained that imperfect sewerage, filth, and defective ventilation, constitute the soil in which the malady most readily takes root and flourishes in the greatest luxuriance; and that consequently, as Dr. Snow, Dr. Shapter, and Dr. Roe very justly observe, attention to these is the only probable method of averting the danger or diminishing the mortality. Of this, Exeter furnishes an instructive and valuable example; for, bearing its full share of the affliction of 1832, it escaped nearly scatheless in 1849, in consequence of the attention paid in the intermediate space of time by the proper authorities to the sanitary condition of the town, and fully corroborating the opinion expressed by Dr. Shapter at the 281st page of his valuable work, where, speaking upon the authority of his own personal experience in 1832, he says, that—

“I feel justified in asserting the Asiatic cholera to be an epidemic, rendered general and fatal by external circumstances; and that without these it is, comparatively speaking, a manageable, if not a preventible, disease.”

There can be little doubt that Exeter owes its comparative exemption from this scourge in 1849 to the removal of those external circumstances of which Dr. Shapter speaks: and had the municipal authorities of Plymouth used equal diligence, that town would have enjoyed, in all human probability, an equal amount of exemption, and not had to mourn the loss of 819 of her most valuable citizens in addition to the 779 swept off in 1832.

[To be continued.]

APOTHECARIES' HALL.

NAMES of gentlemen who passed their examination in the science and practice of medicine, and received certificates to practise, on Thursday, 28th September, 1850:—
William Hemchman Chubb, Diss, Norfolk
—Dox, Bea, Heckmondwike.

CASE OF
SLOUGHING OF A MALIGNANT
TUMOR WHICH CONTAINED THE
FEMORAL VESSELS:

CICATRIZATION OF THE WOUND—DEATH
FROM RETURN OF THE DISEASE.

By J. A. KINGDON.

(Read before the Abernethian Society, December 18th, 1849).

THE following case is of value as well for its surgical importance as its pathological interest:—

Evan Davies, æt. 47, policeman: a robust man, of florid complexion, who had always enjoyed good health, was admitted into Henry's Ward, October 5, 1849, under the care of Mr. Lawrence. He stated that he first perceived a swelling in his right thigh nine months previous to his admission, and that it appeared a fortnight after the receipt of a kick on the part: during the last ten weeks it had remained stationary, but had grown rapidly before that time. He has taken mercury, and been treated locally with leeches and blisters.

On examination it was found that the tumor was hard, unyielding to the touch, and deeply seated, though apparently (from its free lateral motion) unconnected with the bone; it was situated rather above the middle of the thigh, and rendered the integuments prominent close above the inner border of the sartorius muscle. The femoral vessels were supposed to lie either behind or to the inner side of the tumor, and to be unaffected, as the pulsation of the anterior tibial arteries was felt to be equal in both feet. He could walk five or six miles without inconvenience, but much exercise prevented sleep, by causing numbness of the leg, and some pain in the tumor. He complained likewise of a shooting pain at the inner side of the knee and ankle; and although the tumor was not universally tender, yet pain was produced by pressure towards the inner side. He was ordered to take Pil. Saponis Opio, c. gr. v., that night, and to apply a linseed-meal poultice to the tumor.

October 12.—The propriety of an operation was acknowledged by the other surgeons of the hospital in con-

sultation with Mr. Lawrence, although, from the position of the tumor, doubts were entertained regarding the facility of its removal. Pulse 84, regular; tongue clean; bowels open.

13th.—1½ P.M. (Saturday): the patient being fully under the influence of chloroform, Mr. Lawrence made an incision about eight inches in length over the tumor, in the long axis of the limb: the sartorius muscle was exposed and divided, and the more superficial part of the tumor loosened from its connections. In consequence of the deep attachments, a transverse incision became necessary, by which the saphena vein was divided, and immediately compressed above the knee. By these incisions the tumor was fairly exposed, and detached from the surrounding textures for some depth on either side: it presented a smooth, circumscribed outline, and was hard, though elastic to the feel; a slight incision made into its substance was followed by active arterial hæmorrhage. To loosen the mass from its deeper adhesions its distal margin was supported by the left hand of the operator, and an incision made close to its substance; this stroke of the knife was followed by a sudden and voluminous gush of florid blood, as from a wound of a large artery; the bleeding orifice was soon seized, and secured by ligature; yet much blood was lost. Owing to the deep and obscure attachments of the tumor, its extreme vascularity, and probable malignancy of character, it was deemed inexpedient to pursue the operation farther, as that step would have involved the necessity of tying the femoral vessels close above the morbid growth,—a course which, in the exhausted state of the patient, seemed to be very dangerous. He was removed to bed in a very low state; he vomited much, and complained of pain in the region of the knee, but not in the seat of the disease.

4½ P.M.—Still continues sick, but is free from pain; complains of chilliness, although he is perspiring freely; pulse 90, small and feeble. There has been no hæmorrhage since the operation: the edges of the wound were approximated by sutures, and water dressing applied. An effervescing saline draught was given.

14th.—Slept but little last night on account of sickness: the vomiting and nausea still continue: the wound looks

well, and he states himself to be free from pain.

15th.—Sickness again prevented his sleeping; he feels hungry, but can retain no food; there is no perceptible difference in the temperature of the feet; skin warm and soft; pulse 114, small and feeble; tongue pale and moist; bowels not open; there is slight redness around the wound, which is otherwise healthy; the stitches were removed. Ordered, of Scheele's Hydrocyanic Acid miss. to be taken in an effervescing draught three times daily.

16th.—Continued vomiting prevented sleep; but in other respects he remains in the same condition as yesterday; the redness of the wound has slightly increased, and suppuration has commenced feebly. Ordered a mustard poultice to the epigastrium, and a bread and water poultice to the wound; to continue the drops.

17th.—He was much relieved by the mustard poultice, and slept for five or six hours last night: he has had but little sickness this morning; the wound looks well, and is suppurating freely; the surface of the tumor, as well as the rest of the wound, is red and granulating.

18th.—Slept tolerably well last night, and is no longer sick: he has taken some bread and cheese and porter (for which he craved) with relish: pulse 112, soft; tongue thinly furred and moist; bowels not open.

20th.—The bowels were freely relieved after a dose of the ordinary house medicine; and in consequence he reports himself as feeling much better to-day. A pint of porter daily.

22nd.—He has not slept well for the last two nights: there is an erythematous blush on the upper and outer side of the thigh, and slight enlargement of the limb below the knee, with pain in the ankle; the wound looks well, and the secretion is copious and puriform; skin hot; pulse 120, small and weak; tongue pale and moist: he has no appetite, and is thirsty.

24th (twelfth day after operation).—Dozes occasionally, yet gets no sound sleep: he moans continually, but not from pain; the redness of the thigh has extended somewhat, and is rather more intense; the cut edges of the wound are looking well, and suppurate healthily; but there is a *black spot in the centre of*

the tumor, and the granulations around are pale and indolent.

29th.—The erythema has greatly subsided, and he is now able to sleep comfortably; the leg is less edematous, and quite free from pain; the wound suppurates freely; *the whole of the tumor* is converted into a black, ragged, and *sloughing mass*, embedded in, and partly overlapped by, the healthy granulating surface of the surrounding parts: it is without sensation.

30th.—He was ordered to take half a drachm of Battley's Liquor Cinchona Cordifoliae three times daily: a mutton chop daily.

November 2nd.—He eats his mutton chop with relish, and says that he feels better; he sleeps well, and is free from pain; the wound looks healthy; a bent probe passes down three inches on either side of the blackened mass; there is a small bed sore over the sacrum.

10th (twenty-eighth day after operation).—For the last week he has been gradually improving: pulse 100, still feeble; tongue clean and moist; bowels open; his appetite is good; the wound discharges healthily; the slough is separating.

At this day's visit Mr. Lawrence removed, without using violence, the mortified tumor, by elevating the mass with forceps, and separating the more firm attachments with the scissors. On minute inspection, the slough, the size of an ordinary orange, was found to be perforated by a very large artery; and more towards the centre of the mass another canal with less distinct coats lying parallel to it: these, from their size and relative position, were evidently the femoral vessels.*

12th.—The wound is filling rapidly; his appetite has increased.

21st.—The limb has been rolled from the foot to the groin for the last week, and is much diminished in size: cicatrization advances.

December 13th.—Walks about with the aid of a stick: the wound has nearly closed.

19th.—Discharged well.

On leaving the hospital, he went to his house in the north of Wales; and in a letter received from him a fortnight after his dismissal, he stated that he had

* The preparation is in the Museum attached to the Hospital.

borne the journey well, but that he had not left the house since his arrival, in consequence of return of pain in the thigh. Tidings of his death were brought to the hospital at the expiration of six weeks, accompanied with sections of the disease, which had reappeared in his thigh, and shewn itself in the lungs also. A post-mortem examination was made; but as the gentlemen who conducted it were not fully aware of the circumstances of the case, the chest and thigh only were examined. The lungs on both sides were studded with masses more or less considerable of a yellowish-grey substance, which presented the appearance of firm medullary cancer; the diseased mass which had returned in the thigh possessed the same characters as those found in the lungs. It is much to be regretted that neither the condition of the femoral artery, nor the extent or attachments of the tumor, were investigated.

Among the many points of interest presented by the foregoing history the following appear to be more especially worthy of notice:—

Diagnosis.—The subject of the disease was a man who had all the appearance of robust health; his face was ruddy, and free from that tawny-yellow tinge that so often accompanies a malignant diathesis. The tumor, situated rather above the middle of the thigh, did not cause much uneasiness, although lancinating pains were sometimes felt shooting towards the ankle. It was deeply seated, but moveable, hard, and not materially tender when touched: there was no distension of the veins of the skin over it, nor any enlargement of the limb below; the inguinal glands were unaffected.

These symptoms could scarcely be said to indicate a malign nature; the locality of the disease (the bone being unaffected) was not one in which such tumors generally occur, although perhaps the depth might have favoured the supposition; and the lancinating pains were thought to be occasioned by the pressure of the mass upon some branches of the anterior crural nerve. Most patients are apt to ascribe the commencement of their maladies to some special cause; but the value of their theories is often very questionable. In the present instance the origin was attributed to a blow, which (granting the patients to

be correct in their assertions) is certainly a very common cause for many local affections; yet there are many surgeons of eminence who consider it a matter of diagnostic importance, more especially in doubtful mammary tumors; for it is well known that in cancer of the breast the patients almost invariably attribute the commencement of the disease to some local violence, while in other affections of the organ such an origin is neither so frequently nor so positively stated.

Operation.—From the free lateral mobility of the tumor, and from its seemingly well-defined boundary, no great difficulty was anticipated in its removal, although the neighbourhood demanded caution. But when a free exposure of its anterior surfaces was made, and small nodules of disease were found stretching up towards the crural ring, and when the depth of its attachment was ascertained by dissection, then was the severity of the undertaking in some measure appreciated. The case was not even at that time thought to be desperate; and, although it was considered somewhat strange that the femoral vessels had not become apparent, still further was it from the mind of the operator to conceive that the pulseless mass which he was handling and dissecting out was permeated by them. When, however, at length this truth was discovered, there remained but one alternative,—to tie the iliac artery, and then extirpate the disease,—or leave the patient as he then was. The operation had lasted about the half of an hour, and the patient had lost a considerable quantity of blood; it was without much hesitation, therefore, that Mr. Lawrence stayed his hand.

Sloughing of tumor.—The prognosis of the man was decidedly adverse to the patient's well-doing; and certainly so happy a termination as the sloughing of the disease was not anticipated; for, under the ordinary circumstances of a cancerous growth, such a result is by no means frequent, although occasional instances are to be met with in the works of surgical writers. Abernethy* relates the case of a man, aged forty, who was affected with medullary sarcoma of the glands in the left groin; the integuments ulcerated over the tu-

* Classification of Tumors, p. 60.

tumor, and the mass was separated by sloughing; profuse hæmorrhage ensued, which was stopped by pressure; granulations sprang up, and the wound cicatrized. A similar occurrence happened soon after in the opposite groin, but before the cicatrization was complete the patient died exhausted. Travers* speaks of more than one case where cicatrization has been complete after the sloughing of cancerous tumors; and relates the case of a lady who remained well for several years after the occurrence.

Boyer† gives the case of a Russian princess, who, after having consulted most of the eminent surgeons of Europe for cancer of the breast, at last tried the mineral waters of Barèges as an external application. While under this treatment mortification ensued in the tumor, which was of a large size, and by this process it was entirely separated. The very extensive wound healed completely; but shortly after cancerous tubercles were developed in great number in the vicinity of the cicatrix, and the patient died at Paris eight months after the event, which had inspired her with the hope of a radical cure.

Bérard‡ mentions briefly the case of a woman, in which cicatrization of the ulcer took place after sloughing of the tumor, although the system was generally affected, and cancer occupied the internal organs. He relates another case also, where sloughing took place in a large cancerous tumor situated in the thigh of a woman; but that was speedily followed by death.

Richerand§ too, affords an example of cicatrization after the sloughing of a cancerous tumor:—A woman, aged 48, of strong constitution, was admitted into the Hospital of St. Louis with a cancerous breast. The very hard mass softened, a violent inflammation of the skin and cellular tissue covering the tumor was set up, mortification followed, and the whole mass was separated: cicatrization was complete in less than two months.

Sir Everard Home, and indeed most surgical writers who have treated of cancer, mention similar cases: but of all that have been related the foregoing are perhaps the most complete.

In the museum attached to this hospital there are two specimens of cancerous tumors which were separated by sloughing.* The one is the case of a woman 47 years of age, from whom a cancerous breast was removed by operation. The wound healed readily, and remained well for nearly three months; then, however, a tumor began to grow under the cicatrix, which rapidly increased, and was separated by sloughing at the end of three months. She died exhausted nine months after the operation. Hard white tumors of cancerous appearance were found in the lungs.

The other specimen is from a woman 40 years old, in the middle of whose back a large brain-like medullary tumor was situated. The integuments sloughed over it at the end of four months; and, as she was endeavouring to raise herself in bed, the whole mass fell out through the slough: it was followed by profuse hæmorrhage, and she shortly after died.

Now in all these examples the sloughing was spontaneous,—i. e., it occurred as a process of the disease, and was not induced by any medical interference; therefore he argued that their analogy to the case related above was not good. But it must at the same time be remembered that the tumor did not at first present signs of death; it was, on the contrary, covered with healthy granulations up to the twelfth day after the operation, and, as was afterwards discovered, contained within itself the very source of its existence.

The mortified mass was so thoroughly disorganized that, under the microscope, little else could be discovered than a crowd of molecules and granules, which had the peculiar appearance of oil-globules, and an occasional cluster of such shining particles held together by an enveloping membrane. Similar appearances have been figured by Professor Bennett, of Edinburgh,† and considered by him to be degenerated or disintegrated cancer cells, identical with "fatty degeneration," and an evidence of decay. Such, too, appears to be Paget's‡ interpretation of Rokitsky's "saponification of cancer." Now Rokitsky holds this to be one of the means whereby

* Cat. of St. Barth. Hospital, Anat. Museum, vol. i. series 24, spec. 25.

† Ibid, series 25, spec. 60.

‡ On Cancerous and Canceroid Growths, &c. 18, 41, 42.

§ Lectures on Nutrition, &c. MED. GAZ.

* Med.-Chir. Trans. vol. xv.

† Mal. Chir. t. vii. p. 191.

‡ Diet. de Med., art. Cancer.

§ Nosogr. Chir. t. i. p. 516.

a spontaneous cure of the disease may be effected; whereas most surgical writers on the subject look upon it as an unfavourable symptom, and one that is rapidly followed by the development of the disease in internal organs. Ledran,* Rouzet,† Richerand, and Boyer, entertain this view, and their opinions are furthered by the present case.

To discuss every point of interest in the case—the position of the tumor, the cause of the sloughing, the process by which rather more than an inch of the femoral artery was separated without hemorrhage, and the rest—would be no light undertaking, nor one in accordance with the object of this paper; so that upon the mere record of the facts must depend its value.

ON

PNEUMONIA IN THE TROPICS.

By E. H. A. HUNTER,
Staff-Surgeon.

[Continued from page 221.]

Pneumonia.—This, as we have already had frequent occasion to show, is a rare disease within the tropics, at all ordinary elevations, inasmuch that we can only remember two or three well-marked cases, and even these were associated with cardiac disease (hypertrophy with dilatation) in a high degree. Only one had reached the stage of "purulent infiltration" (See "India Journal of Medical Science," Calcutta, 1836-6). Pneumonia, however, and generally of a lower lobe, is occasionally met with as a complication of fever in the more northern provinces of India, during the cold or winter season; and in Scinde, but particularly in Afghanistan, in the form of pleuro-pneumonia, united with fevers of the remitting and intermitting types, it forms endemics (epidemics) of the most fatal tendency. On emerging from the Gundava Pass, from Beloochistan, in December 1839, and for two or three months afterwards, the 2nd, or Queen's Royal, and 17th Regiments, suffered much from a fever of this kind (See "Annual Report" of these regiments for 1839-40, in "the Bombay Transactions," No. iii.); and in a still higher degree, H. M. 40th Regiment, some years subsequently, at Kwettah,

immediately above the Bolan Pass. It is true both in that and the 17th Regiments the disease was returned simply as pneumonia, as it was also at first in the 2nd, or Queen's. By the returns the mortality in the 40th Regiment in 1841-2, was not less certainly than 50 or 60 per 1000 of strength, from this disease alone. In these fevers we found quinine the grand remedy, the almost "sine qua non," indeed; a moderate venesection in the first instance, and cathartic, but afterwards calomel, antimony, and opium, alternately (every second hour or so), with some saline febrifuge during the exacerbation, quinine in the remission or intermission; not waiting for either being very perfect, however, as there is no time to lose. We would now try it upon the present West India system, during the exacerbation also. Two cases of this fever in Scinde we saw terminate fatally in a few hours, with much of the coldness and blueness characteristic of malignant cholera ("febris algida"), and without the least effort at reaction. In both these, on admission, the left side on percussion sounded dead almost as a log, and on the post-mortem examination the lung was found to be nearly equally solid, and of immense size, apparently both lobes being involved. The greater number, however, presented a pleuritis and pneumonia more or less combined, the pleuritis latterly predominating; that is, as we approached the sea coast, Kuratchee; and this not only with the 2nd, or Queen's, but also with the 17th about a month after. (To explain: immediately after our arrival at Kuratchee with the 2nd, or Queen's, we were sent back to take medical charge of the 17th Regiment, and thus had an opportunity of seeing and watching the disease in both regiments to its close.) The excellent effects of quinine we saw verified in several cases on the "per contra principle:" but of these we shall here only particularize three. Two, treated as pneumonia by copious bleeding, &c., were transferred, as was supposed in a hopeless state, with black parched tongues, diarrhoea, and extreme prostration. No quinine had been exhibited: nevertheless, within ten days, both were embarked on the Indus, convalescent, and fully recovered. Small doses of Hyd. c. Creta, and Dover's powder, alternately with quinine, were exhibited as medicine. Sago and wine

* Tr. des Opér. de Chir.

† Rech. sur le Cancer.

for diet. The third was just the opposite of these. The patient had been admitted in the usual way, with the fever, pain in the side, &c., but next morning was so perfectly apyrexia and well, apparently, that quinine was not administered, under the impression that the results latterly might be due rather to some favourable change in the nature of the disease itself. At 11 A.M. the fever and pulmonary symptoms returned, and at 5 P.M. he was moribund.

Pneumonia in Afghanistan occurs also in an uncomplicated form in the winter season, but not very often comparatively; owing, perhaps, to the dryness of the climate. At all events, we had only three cases with the 2d, or Queen's, during October and November, notwithstanding in these two months we marched nearly 500 miles, the greater part over untrodden mountain passes, under a burning sun by day, but with the thermometer often at 14° in the tents by night. Coming along to the east of the Suliman range,—indeed, from the Great Salt Water Lake to the source of "the Larah,"—that is, from about lat. 32° 20, to 30° 30, and eight or nine thousand feet above the level of the sea, the water in the basins was usually frozen to the very bottom, so as to admit of being thrown out in the morning a solid mass; whilst some estimate of the dryness of the climate may be formed from the fact that a flame of electricity was emitted on even hastily raising the quilt from the bed. This was first pointed out to me by a native Indian servant in September at Caubul, who fancied the bed was on fire when he went to make it down; but it continued afterwards to be observed, more or less, till we finally left the country in the end of November.

Pleuritis.—Of this we can only remember three well-marked cases which could fairly be said to have originated within the Tropics, and our reasons for assuming even these to have been idiopathic are by no means unequivocal; whilst the non-proclivity to such diseases in these climates may be deduced from the fact of the immediate disappearance of the pleuritic complications on our leaving Scinde for Bombay in March 1840, and still more so from the improvement, under the most unfavourable circumstances, of three of these cases embarked with the right wing of the 17th Regiment. Though we were

shipwrecked and cast ashore, not only without tents or other covering, but many even minus part of their ordinary clothing, and drenched in salt water, without medicine, and for some days almost without food, they did not die, as we had anticipated, but continued to convalesce almost as if there had been no such occurrence. They did not finally recover as the others, it is true, but they were still improving when we left the regiment about the 1st of October.

Bristol, August 23, 1850.

POISONING WITH SECALE CORNUTUM.

DR. PRATSCHKE was called on the 12th October, 1844, to a woman who, with her three children, had been taken ill the day before. Five days previously they had eaten bread which contained a large proportion of blighted corn. The mother, forty years of age, complained of uneasiness and heaviness in the head, oppression at the stomach, loss of appetite, and diarrhoea; but she did not feel ill enough to take to her bed. The eldest daughter, eighteen years of age, complained of a violent sense of burning in the hands and feet, and especially in the fingers and toes, which were bent and stiff. The lips were retracted so as to expose the teeth; the tongue white and moist; the skin dry and cool; the pulse 90, and small: the patient was very restless, and expressed urgent thirst; the abdomen was soft; the bowels acting; the urine pale. She died on the following day in violent convulsions.

The second daughter, seven years old, had the same affection of the lower extremities, which in her case occurred periodically: her appetite was good: she had also diarrhoea.

The third child, four years old, suffered only from diarrhoea.

An emetic was given to all in the first place; camphor was afterwards administered.

The mother suffered for a few days from tetanic cramps, and continued to complain of great anxiety, loss of appetite, and diarrhoea. After the exhibition of valerian, muriate of ammonia, and ipecacuanha, followed by extract of nux vomica, these symptoms disappeared, but were succeeded by anesthesia of the soles of the feet.

The second daughter was restored to health, with the exception that she had not perfectly recovered the use of her legs.

The youngest child suffered from tetanic cramps for several days; and considerable stiffness of the limbs remained, so that she frequently fell in walking. This subsided under the use of aromatic baths.—*Casper's Wochenschrift.*

X

MEDICAL GAZETTE.

FRIDAY, OCTOBER 4, 1850.

WE have hitherto delayed noticing the letters which have recently passed between the Home Secretary and the Royal College of Surgeons, under the supposition that the correspondence would be continued. It seems, however, that the subject has been allowed to drop, and the Medical Reform question is again *in statu quo*. Still there are some points in the questions of the Home Secretary and the replies of the College which call for notice at the present time; and it is to these that we shall now direct our attention.

Sir George Grey objects to that resolution of the College which excludes from eligibility to the Council, every fellow who resides at a greater distance than *five miles* from the General Post Office, or who practises, or has within *five years practised pharmacy*, either directly or indirectly. He tells the College that these two conditions, if carried out, would create much dissatisfaction.

It will be perceived that the disqualification refers to *distance* as well as to the *practice of pharmacy*. We subjoin an extract from the reply of the College as to the ground of disqualification from distance:—

"In reconsidering the provision of the present charter, which excludes from the Council 'every fellow of the College who resides at a greater distance than five miles by highway or road from the General Post Office at St. Martin's le-Grand,' the Council see no reason to recommend any alteration therein. They are of opinion that fellows 'actually practising their profession' could not perform the duties which devolve upon them as members of council should their residences be at a greater distance from the metropolis; and they beg to remind Secretary Sir George Grey that

a seat in the Council is not a mere honorary distinction, or that it requires only regular attendance at stated meetings of the Council, but that a large amount of the business of the College is necessarily transacted in committees, and that the meetings, both of the Council and of committees, are often the result of unexpected calls. The meetings of Council during the past year have been no less than twenty-two; and the meetings of committees during the same period have amounted to thirty-four. From this statement it will be evident that the exclusion, which, it is presumed, will cause dissatisfaction, arises simply from the fact that fellows of the College residing at a distance from London cannot perform the duties which devolve upon them as members of Council: and it is quite clear that there is no assignable reason for excusing them from the duties which they would be bound to fulfil."

The resolution of the College is, as it is stated by the Secretary, merely a copy of the tenth article of the charter granted in 1843, upon the recommendation of Sir George Grey's predecessor. This article also includes the disqualification respecting the practice of pharmacy and midwifery; and the only difference in the late resolution to which the Home Secretary objects is, that the practice of midwifery shall not in future render a fellow ineligible. As this charter has now been in operation seven years, it is really a question whether the tenth article in its practical working has given rise to that dissatisfaction which Sir George Grey predicts as likely to follow from the provisions of the resolution. Either the Home Secretary has entirely forgotten the articles of the charter now in force, or he has stated his objection to the late resolutions, which are conceived in a more liberal spirit, in a very obscure manner.

We think that the two grounds of disqualification—*distance* and the *practice of pharmacy*—should be held distinct. The former would, undoubtedly, have the effect of excluding good provincial sur-

geons, who might do honour to the College; and promote as much as metropolitan fellows the advancement of surgical science. As *amici curie* in this matter, and without any desire to take a prejudiced view of the question, we do not see why, in a Royal College of the kingdom, the provinces should not be represented as well as the metropolis. The objections of the College affect merely the position of the elected member; and they may be reduced to this. If a surgeon lives beyond five miles it is impossible to give him due notice of the meetings of Council, Committees, &c.; and the distance at which he resides would be an obstacle to the punctuality of his attendance and the regular transaction of business. The College assert that provincial surgeons thus situated, could not perform the duties which would devolve upon them as Members of Council, and proceed to argue that there is no assignable reason for excusing them from the duties which they would be bound to fulfil. In these days of railways, electro-telegraphic and postal communication, these reasons appear to us insufficient to justify the exclusion of fellows on the ground of distance. Many provincial members might easily attend at twenty-four or forty-eight hours notice, and the electric telegraph might be used in cases of such extreme emergency as those suggested by the Council. The College might, therefore, fairly and reasonably abandon the objection on the ground of distance; and thus satisfy the profession that there is no desire to monopolize for the metropolitan fellows only, all power and control over the surgical profession in England and Wales. They have in their hands a very simple remedy for neglect of duty on the part of members when once elected—namely, that if any member, whether metropolitan or provincial, absent himself from a given number of meetings of Council or Com-

mittees, except on the ground of illness or other sufficient cause, distance of residence from London not being admitted a sufficient excuse, he shall cease to be a member of Council, and a new election take place. At present, the Collège, without having made a trial, contend that a provincial surgeon cannot perform the duties of a Member of Council: the provincial practitioners, on the other hand, in expressing their dissatisfaction through Sir George Grey, assert that they can perform these duties. As there is a clear and reasonable remedy in the hands of the College, if their view should prove to be correct, we really do not see why this disqualification on the ground of distance should not be at once surrendered. We do not think that the concession could in any way whatever affect injuriously the interests of the College; and, according to Sir George Grey, one of the causes of dissatisfaction would be removed.

The other disqualification—*i. e.*, the *practice of pharmacy*—is based on totally different grounds. In a Royal College of *Surgeons*, founded especially for the promotion of the science of *Surgery*, it would, we believe, be an unprecedented course to admit to membership of Council, and thereby to a complete control over the affairs of the College, those members or fellows of the College who practise pharmacy either directly or indirectly. This rule of exclusion is, we believe, strictly enforced in the Royal Colleges of Surgeons in the sister countries of Scotland and Ireland; and we ask why the *English* College is to surrender its privilege on this subject, when such a demand is not made, and could not reasonably be made, of the other two British Colleges. Sir George Grey states in his letter that this disqualification creates much dissatisfaction. We should like to have in "facts and figures" a statement of the number of members of the College of

Surgeons who have expressed their dissatisfaction with the arrangement now in force and proposed to be continued. A statistical summary of the profession, in England and Wales, published in our last volume,* shows that there were practising throughout the country in 1849, about 5000 members of the College of Surgeons, including 1127 who had no other qualification than that membership. Have even 2000 of these members recorded their dissatisfaction at the Pharmacy clause of the old charter, and its continuance in the new one? We do not believe that one-half of the members would wish to see a new College founded, merely because fellows of the old College, who practise or have practised pharmacy within five years, are not eligible to seats in the Council. There may be a difference of opinion about the number of years of cessation from this branch of practice. Some may think one or two years sufficient; but we believe that the majority of English members will be found to adopt the view that it would be unjust and unfair to force upon the English College rules of government which do not exist in the Scotch and Irish Colleges of Surgeons, and which, in our opinion, could not safely exist in any College intended for the special cultivation and improvement of *surgery*. We fear that those who would thus forcibly alter the constitution of the English College are really desirous of indirectly overthrowing it. Let the question be considered in this light: the admission of pharmaceutical surgeons to seats in the College Council would probably amount to no more than a small minority out of twenty seats. In this case nothing whatever would be gained by a removal of the disqualification. On the other hand, these members might gradually acquire a majority in the Coun-

cil, and then the College of *Surgeons*, as such, would cease to exist. With the present mode of election, the former alternative would be the more likely to come into operation under the change. The votes and voices of two or three fellows practising pharmacy would be drowned in the wishes of the surgical majority. The consequences may be easily predicted: there would be an immediate resolution that the interests of general practitioners were not attended to, and that some alteration in the mode of election and in the right of voting was indispensably necessary. In fact, the members of this party would not be contented until they had attained, in one way or another, a majority in the Council. Let our readers rest satisfied: it is a mere struggle for power. The admission of three pharmaceutical fellows to the Council would certainly not remove that dissatisfaction which is said to exist, when, as it would happen, their votes were invariably neutralised by seventeen opponents. It is not simply an eligibility to seats in the Council that is desired, but an absolute and despotic control over the affairs of the College. The surgical profession would be merely exchanging what is now pronounced to be one form of despotism, for another which it is our belief they would find far less bearable.

We cannot agree in the assumption that the interests of general practitioners are, as it is alleged, entirely neglected in the Charter as at present constituted. A Fellow of the College who has desisted from practising pharmacy for five years is eligible to a seat in the Council. Is it to be assumed that such a Fellow has lost all consideration for his brother practitioners, merely because he has retired for a few years from this laborious branch of practice? Is a general practitioner of thirty or forty years' standing, if he desist from the practice of phar-

* Vol. xiv. page 567.

macy for five years, so completely transformed into a "pure" surgeon, that he has lost all interest in the concerns of his former fellow-labourers in the profession? It may be said that under the present mode of election such Fellows would stand no chance of obtaining seats in the Council, when they had to compete with men who had devoted themselves through life to the practice of surgery only. We decline to admit the imputation which such an assertion would throw upon eight hundred educated gentlemen, until it has been clearly proved that one who was fully qualified for a seat had been set aside for another who had no other claim over his competitor than that he had never practised pharmacy. A large number of the Fellows practise or have practised pharmacy, and we believe that they would successfully resist any attempt to coerce their wishes on this point.

On this part of the question the reply of the College is, we consider, most satisfactory. They tell the Home Secretary that this rule of exclusion already exists under the Charter of 1843, and contend "that those who have established the proof of their especial and successful devotion to the study and practice of surgery are best fitted to act collectively for the advancement of surgical science, and therefore best qualified to be entrusted in a collegiate capacity with the functions of superintending the education, and of conducting the public business, of the surgical profession." The only question open for the reconsideration of the Council on this subject is, whether a cessation from pharmaceutical practice for *one or two*, in place of *five years*, might not meet all the requirements which they deem necessary for maintaining the surgical character of the College.

Another subject on which the Home Secretary desires information is com-

prised in the following extract from his letter:—

"While the Council proposes certain alterations in the Charter of the Royal College of Surgeons with reference to members of the College, they do not state any clear proposition with regard to that class of medical practitioners *not members of the College*, who have been termed general practitioners, and whose incorporation forms one part of a scheme which has been strongly pressed."

In all new acts or charters there should be no retrospective injustice; but we take this inquiry on the part of the Home Secretary to involve a most singular want of acquaintance with the real state of the medical profession. A regularly qualified medical practitioner, or "general practitioner," we understand to be one who has the diploma of the College and the license of the Hall. Of these there are in England and Wales 4633.* There are some members of the Scotch and Irish Colleges, and 1038 Licentiates of the Society of Apothecaries, who are not members of the English College, but who practise, and are known under the name of "general practitioners." As to the Scotch and Irish members, we think there would be no difficulty in their admission to the membership of the English Colleges on an *ad eundem* principle, if it could be shown that they had received a *surgical* education and had been *examined in surgery*. The same remark would apply to graduates of the Universities of Edinburgh and London; but with regard to the licentiates of the Apothecaries' Society who can produce no test of their having received a surgical education, or of their having been examined in surgery, the case is widely different. That they should claim the membership of the College as a matter

* See our last volume, p. 667.

of course, would not only be fraught with injury to the public, but with injustice to the 5000 gentlemen who have taken out the double qualification. They may, it is true, have studied and practised surgery, and have termed themselves general practitioners; but in what respect, so far as a *qualification in surgery* is concerned, do they differ from one who has not even the license of the Hall in his possession? We intend no disrespect to those members of a useful body who have entered into practice with an Apothecaries' qualification only, but we assert that in any future legislation they have no more right to be enrolled as "surgeons" than the members of the Royal College of Surgeons have to be enrolled as licentiates of the Royal College of Physicians, or M.D. graduates of Oxford, Cambridge, or London. If this general admission is to take place, irrespective of any inquiry into *surgical* education or examination on the part of the claimants, merely because they have for some years practised surgery with an Apothecaries' license, and have been loosely designated "general practitioners," then we do not see how the many unqualified persons who infest the country, but who have also practised surgery for some years, are to be excluded from the enrolment. Under any reform measure, the licentiates of the Apothecaries' Society might as reasonably claim to be placed on the roll of the College of Physicians as on that of the College of Surgeons; and indeed with greater plausibility, because they have received a *medical*, and not a *surgical* education.

If the suggestion involved in the inquiry made by the Home Secretary were to be carried out, *surgical* education should from this moment cease throughout the country. The curriculum of the Apothecaries' Society is less costly than that of the College; it requires no attendance at lectures on surgery or on

surgical practice; it involves no examinations on this important branch of the profession; and of what profit would it be to a student to make this additional outlay, and prepare himself for a College examination, when with the Apothecaries' license he may term himself a general practitioner, and hereafter claim to have his name enrolled among *surgeons* on the College list? It would be a gross injustice and a positive injury to the profession to hold out to young men the expectation that the license of the Hall shall *de jure* entitle them to the rights and privileges of members of the College of Surgeons.

If, then, the inquiry be made, in what relative position all gentlemen now "terming themselves" general practitioners are to remain, we reply, let their present rights and privileges be intact. They who had received no surgical qualification might retain the same position with regard to the College of Surgeons as that which was assigned by the Apothecaries' Act to gentlemen in practice before 1815. Those who had the double qualification would of course be enrolled as members of the College. It is greatly to be regretted that such a system has been permitted to grow up, as that men with a license for one branch of the profession should have been allowed to practise another on which their qualifications had not been tested. It is an evil which has long since required correction: and had this been attended to, the Home Secretary would not now have had to ask in what position the College of Surgeons intended to leave those practitioners who had no surgical diploma, and who had given no proof of having received a surgical examination.

Reichetes.

Researches on Magnetism, Electricity, Heat, Light, Crystallization, and Chemical Attraction, in their relations to the Vital Force. By KARL BARON VON REICHENBACH, Ph. Dr. Translated and edited, at the express desire of the author, with a Preface, Notes, and Appendix, by WILLIAM GREGORY, M.D., F.R.S.E., Professor of Chemistry in the University of Edinburgh. Parts I. and II., including the second edition of the First Part, corrected and improved. 8vo. pp. 468. London: Taylor, Walton, and Maberly. Edinburgh: Macleachlan and Stewart. 1850.

Physico-physiological Researches on the Dynamics of Magnetism, Electricity, Heat, Light, Crystallisation, and Chemistry, in their relations to Vital Force. By BARON CHARLES VON REICHENBACH. The complete Work from the German second edition, with the addition of a Preface and Critical Notes, by JOHN ASHBURNER, M.D. 8vo. pp. 290. London: Baillière. 1850.

THE very full notice we gave of Baron von Reichenbach's researches at p. 911 of vol. ii., new series, has put our readers in possession of the novel character of his discoveries. In order to save them the trouble of reference, and ourselves the space which would be occupied by going over the subject again, we may quote the following passage from that review:—

"Our readers must not suppose from the title, that this essay is written in support of mesmeric quackery. It is true, that many of the results obtained by the author appear to establish the existence of a new imponderable capable of affecting inorganic and organic, or dead and living, matter; possessing, according to his experiments, powers of conduction, isolation, refraction, polarization, and diffusion, and endowed with dualism, according to laws which are as yet imperfectly ascertained. This force or power is, however, wholly distinct from magnetism in its properties: it does not attract iron, nor is it influenced by terrestrial magnetism. . . . This force is found to reside in powerful magnets; for it is stated that, when these are drawn along the living body downwards without contact, they are found to produce certain sensations in a certain proportion

of human beings, whom the author calls *sensitive*."

This extract presents as closely as possible the substance of the first part of Reichenbach's researches, as set before English readers by Dr. Gregory's translation in 1846. The volume now before us contains a second edition of the former researches, and comprises a more detailed account of the experiments whence those results were deduced, than it was then thought advisable by Dr. Gregory to place before the British scientific public: with this are joined investigations which have been subsequently published in Germany by Baron von Reichenbach.

Dr. Gregory remarks in his preface, that although in this country the scientific press has received the startling and novel statements of the author with "that respectful and becoming attention to which, from the known scientific reputation of its author, it was justly entitled,"—a reception different from the irrational and unwarrantable attacks they met with in Germany,—yet that he has occasionally, in private, "met with persons disposed, like the German critics, to reject the whole investigation, without even giving it a careful study, or indeed any study at all."

With such persons we have certainly no sympathy; neither should we feel disposed to bestow upon them the argumentative remarks by which Dr. Gregory has endeavoured, in his preface, to convince them that no well-ascertained facts, or series of facts, can possibly exist in nature which are not worthy of our earnest study.

The second portion of Reichenbach's researches is directed to the investigation of the magnetic or "odylic" light; and, to give our readers an idea of the author's experiments, we quote the following passage from his "Introduction":—

"It has been objected to me that the five girls, whom I have chiefly employed as my reagents for the new force, are not sufficient, where the object is to establish, with a certainty, important truths in natural science. I proceeded to act on this principle, and exerted myself to extend my investigations over a greater number of persons, in various states as to health, and in various conditions of life. In this way more than two years have been employed, and to that extent the following treatise, containing a detailed investigation into the

properties of the odyllic light, as exhibited by magnets, has necessarily been delayed. But it now appears with a train of NEARLY SIXTY SENSITIVE PERSONS AS WITNESSES, male and female, mothers and maidens, low and high, poor and rich, feeble and vigorous, diseased and healthy, women during menstruation and during pregnancy; and thus we have representatives of so great a variety of physical conditions, that little more can be desired."

The author gives a list of the names of the sixty persons "who have assisted him by their observations, and supported him with their sympathy during his researches, and who possess the power, in different degrees, of observing the peculiar phenomena, the study of which is the object of this work." Among these we find twelve individuals, designated as "physicians," "natural philosophers," &c., engaged in scientific occupations; besides lawyers, clergy, &c.

The author states, with reference to the term *odyle*, "that the idea expressed by it very probably includes that which, a year later than I, Dr. Faraday introduced to the scientific world as a new force under the name of Diamagnetism," observing at the same time, that the British philosopher was not acquainted with his researches.

It is important to state the affinities and the differences between *odyle* and magnetism, as recorded by the author.

"*Odyle* is produced and vividly manifested in a multitude of cases in which magnetism, properly so called, is nowhere observed, or is yet unknown to exist,—as in part during chemical changes, in vital changes, in crystals, in the case of friction, in the spectra of solar and lunar rays, and of candle-light, in polarized light, and in the amorphous material world generally."

"In the vast majority of cases *odyle* is developed alone without magnetism. Magnetism never appears alone, but is always associated with *odyle*."

But we must here remark that it should not be lost sight of, that chemical action is always attended with electrical changes,—a fact familiarly exhibited in chemical lectures, and demonstrated by the daily use of the galvanic battery.

The experiments detailed in this part of the work are too minute and too numerous to permit of our giving even an abstract thereof: we shall be obliged to confine ourselves to the enumeration alone of the recorded results.

The *odylic* light is stated to have been perceived by "healthy sensitives," by "delicate or sickly sensitives," by "diseased sensitives," individuals of each of which classes, on whom the experiments were made, are mentioned by name. The *odylic* light appeared to these variously, as—1, a glow; 2, flames; 3, threads, fibres, or feathery down; 4, luminous vapour or smoke; 5, scintillation. The characters and behaviour, under varying circumstances, of these several forms of the light, are fully described by the author.

The author considers the *odylic* light to be "nothing else than the same phenomenon which, on the great scale, appears as the *aurora borealis*, or rather as the polar light, south as well as north." The following extract will give the grounds of this opinion, and at the same time will furnish an idea of the alleged phenomena:—

"Now that we know from the preceding researches that flaming lights exist over magnetic poles larger than the magnets from which they flow; when we learn that these flaming appearances are moveable, undulating, often moving in serpentine windings,—like those of a ribbon agitated by the wind,—becoming at every moment larger or smaller, shooting out rays, scintillating, variegated in colour, and often nebulous, vaporous, and cloud-like; when we find that with our breath we can cause it to flicker backwards and forwards; when we observe that it increases in a rapid ratio in size, intensity, and brilliancy, in rarefied air; and, lastly, when we see it followed at every step by the play of rainbow colours, &c. &c.,—there remains hardly one essential mark of distinction between magnetic light and terrestrial polar light, unless we regard as such the difference of intensity and amount of light, in virtue of which the polar light is visible to every ordinary eye; the magnetic light only to the sensitive eye" (p. 446).

Dr. Gregory, in his Appendix, confirms the Baron's statements by his own observations in several cases.

Dr. Ashburner's translation of Baron Reichenbach's *Researches* "owes its existence perhaps to one, perhaps to a series of misapprehensions with which the public have little concern." These appear to be connected with the publication of his translation without communication with Dr. Gregory; but as we are a part of the public, we have, of course, no concern with these misappre-

hensions The subjects contained in his work are necessarily the same as those contained in Dr. Gregory's edition, but somewhat abridged. Dr. Ashburner presents us more immediately with the results of Baron Reichenbach's "Researches;" but his translation is deficient in the extent of detail as regards experiments: on this part of the subject Dr. Gregory's translation is very full.

It is probable that the publication of these two treatises may lead British philosophers to examine for themselves a series of phenomena hitherto but little observed, and which, from their marvellous character, have as yet only excited distrust or incredulity.

An Inquiry, How far Consumption is Curable; with Observations on the Treatment, and on the use of Cod-liver Oil, and other remedies; with Cases. By JAMES TURNBULL, M.D., Physician to the Liverpool Infirmary, &c. 2nd edit. enlarged, 8vo. pp. 106. London: Churchill, 1850.

THAT Consumption, by which we understand tubercular phthisis, when once undoubtedly developed, is incurable, is an opinion that obtains very generally, there can be no disputing. Whether this opinion be rightly formed or not is certainly worth inquiring: there can be no doubt of its practical importance as to treatment, if the result be an affirmative answer.

We shall place before our readers an abstract of the evidence upon which Dr. Turnbull arrives at the affirmative conclusion of the curability of consumption, and we shall notice the extent to which this curability is attainable by remedial measures.

Dr. Turnbull observes:—

"The evidence in favour of the curability of tubercular disease of the lungs is derived from several sources. By inquiring of our patients closely into the history of previous attacks of illness affecting the chest, we occasionally find that at some period they have had many of the symptoms of consumption, and yet have recovered. This alone is the least important kind of evidence, but it becomes valuable when supported by some of the other kinds. Observation of the symptoms, in cases which come under our treatment, and get well, stands next in value. The examination of the chest with the stethoscope and by percussion, when the disease has ad-

vanced to a certain length, conveys to those who are well acquainted with these modes of examining the chest most satisfactory evidence of the condition of the lungs during and even after recovery. The last kind of evidence is visible and palpable, and cannot, therefore, be doubted: it is derived from an examination of the lungs after death" (p. 13).

It is undoubtedly required that all these kinds of evidence, excepting the last, be combined in a single instance before it can be admitted, during life, that a case of consumption has been cured. Dr. Turnbull speaks of those who reject the evidence furnished by the use of the stethoscope. We count their objections as equal to nothing.

Dr. Turnbull quotes Bayle, Laennec, and Andral, with reference to the recurrence of cicatrization of cavities, and dwells upon the notices of the fibrous and cretaceous transformations of tubercles, and healing of cavities, to be met with in various authors, and which are well known to all engaged in necroscopical examinations.

The author in the next place quotes the statistics of Dr. Walsh, which show that complete restoration to health, not only as regarded the symptoms, but also local evidence of active disease, was effected in 4.26 per cent. of his cases. The Report of the Hospital for Consumption states that the most favourable results have been obtained in the first stage of the disease, since nearly one-half in this stage were relieved, and in ten per cent. all symptoms had disappeared, the patients feeling themselves well, and being able to follow their ordinary occupations. The disease was arrested in two out of fifty-three cases in the second stage; and in ten out of two hundred and ninety-five in the third stage.

Such is the evidence from pathology which Dr. Turnbull adduces in proof of the curability of consumption; and such, we may remark, has long been admitted. The facts adduced afford conclusive evidence of the possible occurrence of so desirable a consummation.

Dr. Turnbull in the next place furnishes us with evidence from therapeutics, and enumerates several remedies which he has found beneficial: among these cod-liver oil is the chief. The Report of the Hospital for Consumption, and Drs. Williams and Bennett, are

also quoted with reference to the efficacy of cod-liver oil. A series of cases of cure is given in illustration of the beneficial influence of cod-liver oil over tubercular disease. But, at the same time, it must be observed that other treatment was found requisite in all cases, especially such as ministered to the improvement of the general health. Great care was taken with reference to diet and exercise, and in some cases the patients enjoyed the advantage of Sydenham's *palmarium* remedy, horse-exercise.

Dr. Turnbull has brought together a sufficient number of facts to encourage us to hope that success may in some instances attend our efforts to arrest this generally incurable disease, by the use of remedies. The author appears, however, more sanguine than the number or character of his facts warrants; but his publication may be of service by inducing fresh efforts at the hands of others.

Memoir on the Influence of Hypertrophy and Dilatation on Diseases of the Heart; and on some Points in their Diagnosis and Treatment. By A. HALLIDAY DOUGLAS, M.D., Fellow of the Royal College of Physicians, Edinburgh; Physician to the Royal Infirmary, Lecturer, &c., &c. Pamphlet, 8vo. pp. 49. Edinburgh: Sutherland and Knox. 1850.

THIS memoir consists of a series of cases and observations, which have appeared in the Edinburgh Monthly Journal of Medicine; and are arranged under five sections.

1. Hypertrophy and Dilatation; 2. Aneurism of the Heart; 3 and 4. Contraction of the Valves; 5. Incompetence of the Valves.

With reference to the first section we may extract the following passage, as embodying very much of the gist of the author's opinions with respect to diseases of the heart:—

"Dilatation has been esteemed dangerous, chiefly because believed to indicate that obstruction of the circulation has been extreme; and the cause of that obstruction from which the dilatation arose has been regarded as the cause of all those secondary evils and distresses which so invariably occur in the last stage of cases of disease of the heart. Now, I think that an error has been committed here, and that the dilata-

tion exerts a material and direct influence in the development of these secondary evils, while the pre-existing cause of obstruction, whether it be valvular or other is comparatively unimportant, except in so far as it must be regarded as the origin or commencement of a series of changes which is to result so formidably. In short, while dilatation is *secondary* in regard to the period of its development, it becomes *primary* in its ultimate importance, acting as the *chief* cause of all the most dreaded evils of the diseases of the heart." (p. 1.)

This quotation contains the leading object of the memoir, which is to show the supreme importance of hypertrophy and dilatation, especially the latter, in the diseases of the heart; at the same time it points out their frequent, if not invariable co-existence, and their relation to valvular disease.

The cases are carefully selected, and well reported. The accompanying observations form a useful practical commentary on some forms of disease of the heart. We commend this essay to all clinical students as containing matter that they will find of service in their studies.

DANGER ATTENDING THE USE OF CHLOROFORM.

DR. HAYWARD, U.S., who is evidently a strong advocate of the use of ether, says, "I am satisfied that there are already on record at least twenty well-authenticated cases of death from the inhalation of chloroform; and I know not how a conscientious man, knowing this fact, can willingly take the responsibility and expose his patient to this fearful result. One of the conclusions to which M. Malgaigne arrives, in his report on chloroform, to the Academy of Medicine of Paris, cannot be too strongly impressed on the minds of those who feel inclined to use it. 'Chloroform possesses a toxic action peculiar to itself, which has been taken advantage of in medicine by arresting it at the period of insensibility, which action however, may, by being too much prolonged, cause immediate death.' The danger is, that we cannot always know the precise time to arrest it, and that the fatal blow may be struck before we make the attempt. In other words, chloroform is a poison, and the insensibility which it produces is only the first stage of its poisonous action."—*American Journal of the Medical Sciences*, July 1850.

Proceedings of Societies.

ACADEMY OF MEDICINE, PARIS.

Sept. 10 and 17, 1880.

Cholesteritis—Scintillations of the Eye.

M. DESMARRES stated in a note, that in several patients suffering from scintillations he had extracted minute floating bodies from the anterior chamber of the eye, and had found them to consist of cholesterolina. Conceiving the symptoms, therefore, to depend upon a morbid state of the secretions of the serous membranes of the eye, he had given to the cases the name of *cholesteritis*.

Chemical Tests of Cerebral Matter.

M. LASSAIGNE informed the Academy that, on repeating Orfila's interesting experiments,* he had been enabled to add one more to the chemical characters of cerebral matter indicated by Orfila—viz., the formation of phosphoric acid by the calcination of cerebral substance in the air.

Extirpation of a Bronchocele.

M. ROUX related a successful case of extirpation of a goitre, which he had performed two months ago. The patient was a strong, healthy man, 30 years of age. The tumor had existed between twelve and fifteen years, and had attained the size of a large fist. It extended more on the left than on the right side, reached above to the os hyoides, below to the sternum and clavicle, and was prolonged a little behind the latter bone. It was scarcely moveable, being firmly attached to the larynx. The carotid artery was not in connection with the tumor.

The operation was only undertaken at the urgent desire of the patient. It was performed in the presence of MM. Rigal, Sautin, and Gerdy.

The tumor was exposed by a long vertical incision extending from the os hyoides to the sternum, and was removed without any difficulty. The vessels tied were numerous; but little blood was lost; syncope did not occur. At one moment during the operation the patient experienced severe dyspnoea with aphonia: this was apparently caused by the section of the recurrent nerves, as the voice has since remained feeble and hoarse. Recovery took place rapidly, the vertical cicatrix being the only remaining trace.

The tumor weighed 325 grammes (=

4975 gra., or about 10 oz. Eng.). Its larger or vertical circumference measured from 27 to 28 centimetres (= about 11 in. Eng.): its transverse measurement was 23 centimetres (= 8.617 Eng. in.).

Use of Chloride of Sodium in Ague.

M. PIORRY stated that, among the various remedies he had investigated as substitutes for cinchona, he had found none exert so decided an influence on enlargements of the spleen as chloride of sodium. This influence had been witnessed in eight cases. In several instances two doses had sufficed to reduce the spleen to its normal dimensions.

ACADEMY OF SCIENCES, PARIS.

September 2nd and 9th, 1880.

New Application of Apparatus termed Hydrophora.

M. FOURCAULT read a note relating to certain apparatus introduced by him some time since for the purpose of applying dry heat and anhydrous cold to all parts of the body in the treatment of internal and external disease, and also as a mechanical means of rendering women barren.(!) The particular modifications of the instrument submitted on this occasion by M. Fourcault were the uterine and the vaginal hydrophora. These instruments being constructed of vulcanized caoutchouc, form a cul-de-sac which is connected with a syringe, by means of which a double current of cold water can be introduced into the vagina or uterus without coming into actual contact with the mucous membrane. By an adaptation of the principle of this apparatus to the caoutchouc pessary provided with a diaphragm of gutta percha, caoutchouc, or of some soft spongy substance, M. Fourcault has contrived the means of presenting a voluntary obstacle to fecundation!

Sulphuret of Arsenic in Mineral Waters.

M. BLONDEAU stated that he had discovered sulphuret of arsenic in all mineral waters possessing energetic properties.

The Phosphene or Luminous Spectre produced by Compression of the Globe of the Eye, an Indication of the Condition of the Retina.

M. SERRÈS (of Alain) transmitted the first part of a memoir in which he proposes to regard the luminous spectre produced by compression of the eye as an indication of the state of functional life in the retina, and to apply the information thence ob-

tained to the diagnosis of diseases of the eye. M. Serres produces the *phosphene* by pressing with a hard angular body, repeating the pressure at least twice in a second of time. The nasal aspect of the globe is the part of the organ preferred by M. Serres, as he has usually found the luminous spectre more brilliant than when pressure is made on other parts of the globe. If, after repeated trials made during several days, no luminous spectre is thus produced, paralysis of the retina may be inferred, and thence the conclusion that operations on the cornea, iris, or lens, will be useless.

Poisonous Action of Agents protective of Dead Animal Matter against Spontaneous Combustion.

M. ROBIN communicated a memoir on this subject, of which the following observations constitute a summary:—

All those agents which protect dead animal matter from slow combustion by oxygen with moisture, are poisonous both to plants and animals; and they are so, not by their direct action on the nervous system or heart, nor by the coagulation of albumen, but by their opposing during life, as well as after death, the slow combustion of animal matters by oxygen.

Every agent possessing this power, if introduced in sufficient dose into the circulation, in the same manner prevents the oxidation of the protein elements of the blood, and thus produces a sedative poisonous action, or causes death by asphyxia.

Those agents which exert this protective power in the highest degree are in general the most active poisons.

The poisonous action of many substances which is exerted in this way (*e. g.* hydrocyanic acid, the ethers, chloroform, camphor, benzoin, arsenic) has been attributed to a direct action on the nervous system. This assertion M. Robin regards as gratuitous, and considers that the chemical action to which he refers, being in strict relation with the phenomena observed during life and after death, affords a more correct explanation of their mode of action.

MONTPELIER ACADEMY OF SCIENCES.

On the Preparation of Collodion. By M. F. LUTRAND.

AFTER some introductory remarks on the value of collodion, M. Lutrand states that he has made a great number of experiments with the view of ascertaining the best for-

mula for the preparation both of the cotton and the ethereal solution.

Preparation of the cotton.—M. Lutrand recommends that the cotton should be placed in a mixture of suitable proportions of dry nitrate of potash, concentrated sulphuric acid, and fuming nitric acid. In this way he unites the two processes generally employed—*vis.* that of Kopp, who uses a mixture of monohydrated nitric acid and concentrated sulphuric acid, and that of Mialhe, who employs a mixture of nitrate of potash and sulphuric acid. In this case the sulphuric acid exerts a two-fold action: first, it seems to decompose the nitre; secondly, it keeps the nitric acid in a monohydrated state: the cotton imbibes the acid more readily, and the prepared cotton offers greater uniformity of composition throughout its whole mass.

Preparation of collodion.—M. Lutrand states that the two formulæ generally employed in the preparation of collodion are those of Mialhe and Bouchardat. That of Mialhe gives too firm a product; whilst that of M. Bouchardat, on the contrary, is too fluid. M. Lutrand recommends a combination of the two processes. Instead of using for every 1000 parts of ether 64 parts of prepared cotton, as recommended by Mialhe, or only 22·37 parts, according to the formula of M. Bouchardat, M. Lutrand employs 32 parts, or one half of the proportion indicated by M. Mialhe, with the addition of 80 parts of alcohol.

FRACTURE OF THE NECK OF THE HUMERUS.

A MAN, 75 years old, fell from a stool and broke his arm. Two months afterwards he died. An examination of the limb showed that the fracture had taken place close to the head, and that reparation had made but little progress. The capsule of the joint was filled with sanguineous gelatinous fluid. The space between the fractured surfaces was occupied by a cartilage half an inch thick, around which the periosteum formed a capsule so firmly attached that it was with difficulty separated. Beneath the periosteum, around the bone, was a callus-like capsule, which gradually united with the new cartilage. This capsule of provisional callus exhibited, under the microscope, the fibres of the periosteum connected with new osseous cells; separate points of ossification could also be detected. The attempt at union was demonstrated in a circle of earthy deposit around the end of each bone.—*Casper's Wochen-schrift.*

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Hospital and Infirmary Reports.

FELLOWS' PRIZE ESSAY.

Summer Session 1845.

By C. H. F. ROUTH, M.D. Lond.

[Continued from p. 491.]

PART II.

IN the pursuit of the inquiry as to whether excess of urea is ever passed in 24 hours, it became important to ascertain by actual experiment whether a given quantity of urine contained such excess. For the most part specimens of high specific gravity were selected, and the rough test applied to them.

Several tubes of the same size were procured. An arbitrary measure was taken of 48 minims. One half, or 24 minims, were tested with an equal quantity of nitric acid, and, the tube being kept in a glass of cold water, set aside to crystallize. At the end of 24 hours the quantity of urea so obtained was collected, dried, and weighed, and then, by the simple rule of three, the amount of urea passed in 24 hours calculated.

The experiments were made upon 52 cases, in which the specific gravity varied from 1021 to 1039. In many, however, of these cases no crystals of nitrate of urea were found. This was the case with one of delirium tremens especially, with a specific gravity of 1039, while in others of much lower specific gravity the quantity so formed was very considerable. The inference necessarily was, that, as far as the rough test was concerned, it was not to be depended upon. In comparative experiments we were enabled to make out four causes of this:—

1st. If the water in which the tube was placed was not of a certain low temperature the crystallization did not occur. The difference of temperature in the water, even when placed under the same circumstances, was found to vary as much as 10° F.—usually 5°; and this was sufficient to determine a great difference in the amount of crystallization.

2d. Excess of colouring matter seemed to prevent the determination of the crystallization, especially when the colour was of a red amber; but at other times no such connection could be traced.

3d. Liebig has shown that if to a solution of urine which is very full of lithates nitric acid be added, a portion of the uric acid is decomposed, and nitrate of urea formed in the combination. Such a complication would generally (in the cases selected) vitiate the result.

4th. The nitric acid is liable to vary in strength and kind. If very concentrated, and especially if containing in solution the slightest quantity of peroxide of nitrogen, it at once determines the decomposition of the urea.

a. A knowledge of these sources of fallacy at once satisfied us that no data so obtained could be relied upon, and analysis was therefore had recourse to in every case in which it appeared probable *a priori* excess of urea was passed in the 24 hours.

b. The next consideration referred to the best method of analysis. It may be well here, therefore, to enumerate each process which we tried, enumerating in the description the several difficulties and sources of fallacy in each.

1st. *Oxalate of lime process*.—1. A given quantity of urine is taken. This is evaporated to the consistence of a syrup, taking care that the temperature does not exceed 200° Fahr. at which the urea would be decomposed. This is then saturated with alcohol, or naphtha (which answers equally well), and filtered. The solution again evaporated, almost, if not entirely, to dryness, avoiding a temperature, however, above 200° Fahr.

2. The residue so obtained, consists, for the most part, of urea or lactate of urea, with some colouring matter, some chlorides, lithates, and other salts having been separated by the former process. To this a little hot water below 200° F. is added, and the solution so obtained is completely saturated with oxalic acid. This is again evaporated at a low temperature, and set aside for a few hours. The crystalline matters so obtained consist of excess of oxalic acid, and oxalate of urea.

3. This crystalline matter is again dissolved in hot water, and precipitated by carbonate of lime: oxalate of lime is thrown down, urea and carbonic acid remaining in solution. After filtering and evaporating this solution to dryness, the carbonic acid is expelled, and crystals of urea, almost entirely deprived of colouring matter, remain behind.

This method was frequently tried, and in the following table some of the results that were obtained by this process are shown:—

2d. *Henry's method*.—1. To a given specimen of urine a saturated solution of acetate of lead is added, until no further precipitation occurs: this is filtered. In this process the phosphates and carbonates of lead are thrown down; the chlorides also, which, though soluble in water, are insoluble in saline substances. The potash, soda, &c. and the other salts, remain in solution as acetates, together with the urea, much of the colouring matter having been thrown down with the precipitates.

TABLE OF SEVENTY-TWO ANALYSES FOR UREA MADE ON SPECIMENS OF URINE VARYING FROM A SPECIFIC GRAVITY OF 1008 TO 1022.

No.	Name.	Sex.	Disease.	Diet at the time.	General character of the urine.	Sp. gr.	Gr. passed in 24 h.	Gr. of urea in 1 oz.	Gr. of urea passed in 24 h.
1	Bisney .	F.	Albuminuria.	—	Very pale.	1008	64	1.61	108.04
2	Sherring .	M.	Paralysis of portio dura.	—	Do.	1010	62	4.03	249.86
3	Collins .	"	Rheumatism.	—	Do.	1011	42	4.03	169.28
4	Hookley .	F.	Albuminuria.	—	Do.	"	32	3.22	103.04
5	Fuller .	M.	Do.	Full : chop for 3 days previous.	Do.	"	64	2.49	159.36
6	M.A. Young	F.	Do.	Middle.	Do.	"	28	4.14	115.92
7	Fuller .	M.	Do.	Do.	Do. almost colourless.	1012	60	4.14	248.40
8	Millsom .	"	Paralysis.	Full.	Very pale.	1013	64	1.84	117.76
9	Sherring .	"	Paralysis of portio dura.	—	Do.	1015	50	5.99	299.50
10	Stedham .	F.	Ozena.	Full for 10 days.	Do.	"	52	5.03	261.58
11	Lockett .	M.	Rheumatism — lead colic.	—	Do.	"	56	4.03	225.68
12	Caldwell .	"	Eczema Rubr.	Milk.	Not clear ; hazy ; pale.	"	38	2.97	112.86
13	Reynolds .	"	Lead Colic.	Middle.	Pale straw ; clear.	"	52	3.66	190.32
14	Lockett .	"	Rheumatism — lead colic.	—	Light amber ; clear.	1017	50	2.98	149.00
15	Copeland .	"	Rheumatism.	Full.	High colored ; clear.	"	32	3.45	110.40
16	Coles .	"	Eczema Syph.	Milk.	Pale straw ; hazy.	"	44	4.28	188.32
17	Terry .	"	Tumourinneck.	—	Lt. amber ; clear.	1020	39	6.25	243.75
18	Tiedemann .	"	Rheumatism.	Full 1 wk.	Pale straw ; clear.	"	32	7.02	224.64
19	Southgate	F.	Do.	Low.	Dk. br. yel.	"	12	4.14	49.68
20	Newman .	"	Phthisis.	—	Do. with lithates.	1021	20½	8.64	177.12
21	Hughes .	M.	Hæmoptysis.	—	Palecl. amb.	"	21	7.02	147.42
22	Herbert .	"	Pneumonia.	—	Do.	"	44	5.66	249.04
23	Bell .	M.	Pleuritis.	Milk Oij. Middle.	Do.	1022	32	6.45	206.40
24	Tiedemann .	"	Rheumatism.	—	Dark do.	1023	30*	6.68	200.40
25	Knight .	"	Secy. syphilis.	Middle.	Pale do.	"	26	5.52	143.52
26	Beckett .	F.	Consolidation of apex of lung.	—	Dk. amber.	"	24	6.68	160.32
27	G. Jones .	M.	Rheumatism.	Low.	Very dark brown.	"	18*	7.37	132.66
28	Green .	"	Pleuritis—morb. cordis.	Do.	Very high coloured.	1025	20½	4.10	82.50
29	Bagley .	"	Phthisis.	Full.	Pale ; full of lithates.	"	28	9.44	264.32
30	Knight .	"	Rheumatism.	Do. chop.	Very high coloured.	"	24	11.28	270.72
31	Hunter .	F.	Anæmia.	Middle.	Pale ; hazy.	"	36	6.45	232.20
32	Hurdle .	M.	Rheumatism — morb. cord.	Full.	Rather high coloured.	"	16	8.52	136.32
33	J. Miles .	F.	Anæmia.	—	Do.	1026	24*	9.30	227.60

* About.

No.	Name.	Sex.	Disease.	Diet at the time.	General characters of urine.	Sp. gr.	Urea in 100 parts of urine.	Gra. of urea in 1 oz.	Gra. of urea passed in 4 h.
34	M'Manus	M.	Meningitis.	Low.	Very dark.	1026	24	7.71	185.04
35	S.A. Padley	F.	Dysentery.	Sago, beef tea, Oij.; Middle for 3 ds.	Do.	"	12*	6.91	82.92
36	Tiedemann	M.	Rheumatism.	Full.	Pale, but full of lithates.	1027	32	8.75	230.00
37	Stagg	F.	Metritis.	—	High col.	"	12*	9.44	113.28
38	Green	M.	Pleuritis—morb. cordis.	—	Do. lithates.	"	20	9.23	184.60
39	Southgate	F.	Rheumatism.	—	High col.	"	30*	8.75	262.50
40	M.A. Pollock	"	Intestinal disorder.	—	Do. lithates.	"	16*	8.76	140.16
41	Bell	M.	Pleuritis.	—	Pale amber.	1028	13	9.95	119.40
42	M.A. Pollock	F.	Intestinal disorder.	—	—	"	18*	11.05	198.90
43	J. Knight	M.	Secy. syphilis.	Middle, but living freely before admission.	Dk. amber.	1029	26*	9.44	245.44
44	Robinson	F.	Eczema of lung.	Milk.	Do.	"	14	8.06	112.84
45	Hall	M.	Erysipelas.	—	Pale & clear.	"	18	10.36	186.48
46	Mills	F.	Anæmia.	—	Pale.	1030	20	7.37	147.40
47	Hill	M.	Aneurism of aorta.	Full 5 ds.	Pale & clear.	"	18	8.29	149.22
48	G. Jones 1	"	Rheumatism.	Low.	Very high coloured.	"	20	9.90	198.00
49	Elliott	"	Pneumonia.	Do.	Do.	"	12	10.96	121.52
50	Nicholson	"	Sciatica.	Middle.	Do.	"	18	12.39	223.02
51	Hill	"	Aneurism of aorta.	Full 10 days.	Pale & clear.	"	32	8.98	287.36
52	Taylor	"	Peritonitis.	Low.	Very high coloured.	"	35	12.67	443.45
53	Ward	"	Del. tremens.	Middle.	Do. lithates.	"	24*	7.83	187.92
54	G. Jones 2	"	Rheumatism.	Low.	Very high coloured.	"	42*	10.13	425.46
55	Taylor	"	Peritonitis.	Do.	Do.	"	28	10.36	290.08
56	M.A. Griffin	F.	Anæmia.	—	Amber col.	1031	24*	10.13	243.12
57	Copland	M.	Rheumatism.	Low.	Dark do.	"	18*	13.82	148.76
58	Lee	"	Tabs, phthisis.	Chop; Middle.	Clear, high coloured.	"	26	10.36	269.36
59	Padley	F.	Dysentery.	Beef-tea, milk, aa. Oij.; arrowrt.	Do. lithates.	"	12	5.99	71.88
60	Reynolds	M.	Lead colic—epilepsy.	Middle.	Do.	"	12	9.13	109.56
61	Ward	"	Delir. trem.	Do.	Do.	"	12	10.27	123.24
62	Elliott	"	Rheumatism.	Low.	Do.	"	20	8.98	179.60
63	Raven	"	Otitis.	Middle.	Very dark.	1032	12	11.75	141.00
64	Elliott	"	Pneumonia.	Low.	Do.	"	18	13.35	240.30
65	G. Jones 2	"	Rheumatism.	Middle.	Do.	1034	16	12.48	199.68
66	Elliott	"	Do.	Low.	Do. lithates.	"	18	7.84	141.12
67	Slade	"	Lead colic.	Middle.	Do.	1035	12*	13.38	160.56
68	Baynes	"	Ulcer of leg.	Low.	Do.	1036	16	9.44	151.04
69	P. C.	"	Healthy.	Meat daily; port. Oij.	Amber col.	1035	24	11.52	276.48
70	Taylor	"	Peritonitis.	Low.	Do.	1037	12	11.53	144.36
71	Hayden	"	Delir. trem.	Full.	Dk. amber.	1039	"	"	"
72	Pearson	"	Chorea.	—	Do.	"	"	"	"

2. The clear solution is then re-precipitated by the cautious addition of sulphuric acid till no further precipitation occurs. It is stated that, after a second filtration, the clear solution remaining will on evaporation yield clear crystals of urea. Such is, however, not exactly true, as the sulphates of soda, potash, &c. remain in solution also, and the process is therefore apt to yield erroneous results.

3d. *Third method.*—This consisted in boiling first with animal charcoal, before proceeding with the preparation of the nitrate of urea, the object being first to get rid of the colouring matter. There is, however, one objection here,—that in this boiling a portion of the urea is apt to be decomposed.

4th. *Nitrate of urea process.*—1. The urine was prepared as in the first part of the process before described—by oxalate of lime.

2. To this residue add a sufficient quantity of dilute nitric acid to dissolve it; then place the solution so obtained in a cool place. When quite cool, add to it, drop by drop, pure strong nitric acid till no further crystallization occurs.

3. The crystals so obtained were dried and weighed, and the quantity of urea calculated from the nitrate.

This method, being the least tedious, was generally preferred.

In all, 105 specimens of urine were examined. Unfortunately, however, 33 of these were found vitiated by the absorption of moisture by some of the saucers in which they were weighed. The table here given therefore only includes 72 cases, for the most part made during the last three weeks of the competition, the other 33 being rejected as erroneous.

N.B. The numbers given at column No. 9 were obtained by analysis of different but always proportional quantities of urine. All these specimens of urine were acid, except M. O. Manners's case, No. 34; and, generally speaking, most so in the dark and turbid specimens than in the clear and pale. In No. 34 it was distinctly alkaline.

From a careful consideration of the above table it appears, that although, relatively to the quantity of urine examined, there was sometimes an excess of urea, still absolutely there was generally a deficiency, as where the specific gravity of the urine was high the quantity voided was invariably small. This remark applies to all the cases, except two—No. 52, G. Jones, and No. 54, Taylor. These, however, it is believed, are not even, after all, exceptions: in Jones's case a note of interrogation is put opposite the quantity xxij ; by the nurse's measurement only xxij had been passed. The additional number of ounces

was put down because he stated positively that he had passed xxx . besides in the closet, or thereabouts. The specific gravity of this quantity, supposing it not exaggerated, was not fairly taken,—only inferred; and, after what has already been said, no dependence can be placed on the result.

In the case of Taylor, on the day in which the xxxv of urine were saved, there had been the day before retention of urine, and about xxij of this urine were drawn by the catheter; so that, had the power of making water persisted, throughout, it is probable there would not have been so much urine passed in the 24 hours in question: part of it would have been distributed over the preceding and succeeding 24 hours.

Among the females there are no instances in which it can be clearly stated that an excess of urea occurred; perhaps because in very few was an actual measurement of the quantity of urine passed possible; but even in these cases the actual amount of urea excreted in 24 hours is probably exaggerated.

In as far, therefore, as the tables of the above cases go, it is confirmatory of Bequerel's opinion, that excess of urea is not excreted in disease in a period of 24 hours, yet it is believed that this opinion requires occasionally a modification.

1st. *A priori*, we can conceive an excess to be excreted in 24 hours in cases similar to that of Whitehead, where urea for a few days, owing to renal congestion, has not been properly excreted from the blood, giving rise at the time to the peculiar narcotic symptoms of this poison; suddenly, owing to remedial agents, the kidneys resume their functions. We believe we have seen examples of this excess in two or three cases; but, as no analysis was made at the time, the opinion cannot be confirmed by actual experiment. Extended over a period of one week, however, even in these cases there would be no excess.

2d. There are cases of excessive diuresis in which it is also probable excess of urea may be secreted in a space of 24 hours, and for several days consecutively. There was a patient some time back under Dr. Williams, Adam Plummer by name, who, on recovering from an attack of intermittent fever, and in whom the spleen was permanently enlarged, used to pass some seven pints a day of urine, and that of a specific gravity not lower than 1015, and occasionally as high as 1020. In this case, without doubt, an excess of solid matters of all kinds was excreted in the 24 hours, and it is highly probable that the urea also bore a high proportion in the quantity excreted.

3d. There are cases of cholera in which, during the convalescence, the patient may

void an unusually large quantity of urine from the same causes.

These opinions, however, rest alone on their plausibility, as we have no experiments to confirm them: indeed, as far as these go, they completely negative the question proposed.

Correspondence.

LIFE INSURANCE CERTIFICATES—SUGGESTION TO MEDICAL REFEREES.

SIR,—The subject of giving answers to the inquiries of Insurance Companies seems to me to admit of a natural division into two parts—first, what is due to my patient; and secondly, what is due to myself.

To my patient I owe the same implied secrecy on the subject of his personal health which a lawyer owes to his client on the subject of his personal affairs; and I should never think of answering any inquiries about him without his express permission.

To myself I owe the right of obtaining a reasonable remuneration for my trouble in furnishing to an Insurance Company my professional assistance, and thus enabling them to decide on the fitness of my patient as a subject for life-assurance.

I have therefore adopted the subjoined form of answer to the inquiries of insurance offices; and I shall be happy to join any gentlemen who approve of such form in having a number lithographed.

I am, sir,

Your obedient servant,
C. R. WALSH.

42, Half Moon Street, Piccadilly,
September, 1850.

SIR,—I have received the permission of _____ to answer any questions which may be put to me in confidence respecting his health for the purposes of life-insurance. I am ready, therefore, on receiving the usual fee, to place at the disposal of your office whatever information I possess on this subject.

I am, sir,

Your obedient servant,

To the Secretary of the _____
Insurance Company.

THE ROMAN HOSPITALS.

THE *Giornale di Roma* of the 19th September publishes a Papal *motu proprio*, instituting a commission of hospitals, and consolidating all the hospitals of the State into one institution, by subjecting them to uniform rules of conduct and community of interests.

Medical Intelligence.

UNIVERSITY OF LONDON.

FIRST EXAMINATION
FOR THE DEGREE OF M.B., 1850.
PASS EXAMINATION.

Monday, August 5.—Morning, 10 to 1.

Anatomy and Physiology.

Examiners, Mr. KIERNAN and Prof. SHARPEY.

1. Give an account of the cervical portion of the Vertebral column, pointing out the characters which distinguish the several parts of the bones of this region from the corresponding parts in the dorsal and lumbar regions; and describe specially the Atlas and Dentata. Enumerate the ligaments, and mention the movements which take place in this region, including those between the Cranium and the Atlas, and between the Atlas and Dentata.

2. Describe in their order of position the parts which would be brought into view in dissecting the origin and course of the phrenic and recurrent nerves on the right side of the neck. The phrenic to be traced only as far as the first rib, and the recurrent to the lower border of the larynx.

3. Give an account of the Inguinal and Femoral canals, describing their orifices, boundaries, and contents; and give the origin and course of the Epigastric Artery, the varieties in the origin and course of the Obturator, and the relations of these vessels respectively with the canals. Explain the mode of formation of Congenital, and of the other forms of Hernia which occur in this region.

4. Describe the inferior surface of the Liver, mentioning the parts with which it is in relation. Give an account of the form, situation, and structure of the Gall-bladder and ducts, and describe the vessels as far as seen outside the liver; mentioning their condition at the time of birth, and the changes which they undergo afterwards. Explain the circulation of the blood through the Liver.

5. Give an account of the Tympanum of the Ear; comprehending a description of its osseous parietes and of the parts contained in it in the recent subject.

Afternoon, 3 to 6.

1. Describe the Knee-joint, giving an account of the extremities of the bones which enter into its formation, of the cartilages, fibro-cartilages, ligaments and synovial membrane, and of the movements which take place in the joint; mentioning the muscles by which they are severally effected. Compare the knee and elbow

joints as regards their construction and movements.

2. State the dissection required to display the Musculo-spiral nerve, and its branches, from the point where it leaves the axilla to the wrist; mentioning the different parts exposed in the process, and describing their situation relatively to the nerve and to each other. The attachments of muscles not to be given.

3. Describe in their relative position the parts which would be brought into view on removing the following muscles, viz. :—

1. On the Foot—the Flexor brevis digitorum, Abductor pollicis, and Abductor minimi digiti.

2. The upper half of the Trapezius, the Splenius capitis et colli.

3. In the supra-hyoid region of the Neck—the Platysma myoides, the anterior belly of the Digastricus, and the Mylo-hyoideus.

4. Give a description of the form, relative situation, connections, and structure of the Bladder and Prostate Gland.

5. Mention the principal situations in which nervous ganglia are found. Give an account of the outward appearance and intimate structure of these bodies.

Tuesday, August 6.—Morning 10 to 1.

Chemistry.

Examiner, Professor BRANDE.

1. Give the process and theory of alcoholic etherification, and specimens of the formulæ of the several combinations of Ethyle.

2. One of the Cornish antimonial ores is a triple sulphuret of Antimony, Lead, and Copper: how would you determine the respective quantities of its four components?

3. What are the leading distinctive constituents of the urine of graminivorous, as opposed to that of carnivorous animals? Give the formulæ of those constituents, and the best modes of obtaining them in a pure state.

4. Describe the modes of preparing the compounds of Iron and of Copper contained in the London Pharmacopœia, and give their respective formulæ.

5. In Galvanic combinations rendered active by dilute sulphuric acid, and consisting respectively of Iron and Tin, Iron and Copper, Iron and Zinc, and Iron and Lead, what is the direction of the electric current, and to what practical purposes is the knowledge of that direction applicable?

Afternoon, 3 to 6.

Materia Medica and Pharmacy.

Examiner, Dr. PERKINS.

1. Give a sketch of the chemical changes

which medicines and the organism mutually suffer by their reciprocal action on each other.

2. In what do disinfectants and antiseptics differ from each other? Enumerate the chief agents used as disinfectants, and explain their *modus operandi*.

3. How is cod-liver oil obtained? By what chemical character are liver-oils distinguished from the oils of other parts of the animal body?

4. Describe the method of making and of purifying chloroform. Mention the specific gravity of this liquid; and state the tests by which its purity may be ascertained.

5. From what species of Cinchona is yellow bark obtained; and where does it chiefly grow? How is disulphate of quina manufactured? by what tests can the purity of this salt be determined? and what are the characters by which quina and cinchonin are distinguished from each other?

6. Describe the medicinal properties, uses, doses, and modes of administration of bichloride of mercury, arsenious acid, hydrocyanic acid, and iodide of iron.

7. Enumerate the chief medicinal agents which affect the movements of the iris; distinguishing those which cause dilatation, from those which produce contraction, of the pupil. And state what indications, for the use of these agents in cerebral diseases, have been drawn from their known effects on the pupil.

8. What vegetable articles of food are admissible, and what are objectionable, in a case of diabetes?

9. Describe the botanical characters and medicinal qualities of *Conium maculatum*.

Wednesday, August 7.—Morning, 10 to 12.

Botany.

Examiner, Rev. Prof. HENSHLOW.

1. Define the terms Endocarpium, Pericarpium, Papilionaceous, Diadelphus, Paracchyma, Vernatio.

2. Give such diagnoses of the following Orders as may be sufficient to include our British genera :—

Umbelliferae, Scrophularinae, Cucurbitaceae, Colchicaceae, Graminae.

3. Give such diagnoses of the following Genera as will include our British species : Hyoscyamus, Vicia, Primula, Arum, Iris. N.B. Any Candidate who may prefer describing the specimens on the table, to replying to the last questions, is at liberty to do so; confining himself to the following particulars :—

1. Leaves. Arrangement, stipulation, composition.

2. Inflorescence. General character, bracted appendages.

3. Peculiar condition of each Floral whorl; and more especially,

4. Insertion of Stamens and Corolla.
5. Reasons for referring each specimen to a particular Order.

EXAMINATION FOR HONOURS.

Thursday, August 15.—Morning, 10 to 1.

*Anatomy and Physiology.***Examiners, Mr. KIRKMAN and Prof. SHARPEY.**

Commencing the dissection at the integuments, and proceeding with it as far as the pharynx and the bones forming the zygomatic fossae, describe the parts successively exposed in dissecting the space bounded above by the zygoma, below by the base of inferior maxilla, in front by the anterior margin of the masseter, and behind by the mentus auditorius, mastoid process, and upper part of the sternocleidomastoideus.

Afternoon, 3 to 6.

1. A line being drawn round the arm two inches above, and another two inches below, the bend of the elbow, describe the soft parts seen in dissecting the included portion of the limb, both before and behind, in the order in which they appear. The joint not to be described.

2. Describe the structure and arrangement of the Mucous Membrane of the Intestines, pointing out the characters it presents in different parts of the canal.

Friday, August 16.—Morning, 10 to 1.

*Chemistry.***Examiner, Professor BRANDE.**

1. Enumerate the several combinations of *Phosphorus with Oxygen*, giving their respective formulæ, and modes of preparation. Describe the relations of *phosphoric acid* to water, and to bases, stating the distinctive characters of the several modifications of that acid and of their salts, and the best methods of determining the presence and quantity of phosphoric acid in the ordinary operations of analysis.

2. What are the substances usually found in *River and Spring water*? Give an outline of the quantitative analysis of such waters, and state the circumstances which especially influence their fitness for the supply of Towns, and for domestic use.

3. What is the nature of *Flame*, and upon what does its value depend as a source of artificial, compared with solar light? What are the several circumstances which respectively influence the *Heat* and the *Luminosity* of flame? Describe the process of *Photometry* as applicable to the determination of the relative illuminating powers of different flames.

[It is expected that detailed answers be given to the above questions.]

Afternoon, 3 to 6.

*Materia Medica and Pharmaceutical Chemistry.***Examiner, Dr. PENNIE.**

1. Give a sketch of the pharmacological history of the compounds of lead; to embrace their general chemical characteristics, medicinal and poisonous effects, therapeutic uses, and antidotal treatment; and to include a notice of the preparation and special properties of those compounds which are in ordinary use as medicines.

2. Describe the botanical character, chemical properties and medicinal and poisonous effects of the seeds of *Strychnos Nux vomica*. State how strychnia is obtained, and describe the chemical characteristic of this alkaloid as well as of brucia. Lastly, mention the physical and chemical characters by which cusparia bark is distinguished from the bark of the *Strychnos Nux vomica*.

3. Describe the modes of preparing the compounds of ammonia contained in the London Pharmacopœia; and give the theories of the processes and the formulæ of the various compounds.

4. You are required to state by what characters you would detect—

a. The adulteration of scammony with chalk, starch, and guaiacum.

β. The substitution of carbonate of zinc for the oxide of that metal.

γ. The presence of the leaves of *Cyanacum Arget* and of *Tephrosia apollinea* in Alexandria senna.

δ. The intermixture of the meal of white mustard seeds with that of black mustard seeds.

e. The presence of nitrate of potash in commercial nitrate of silver.

ζ. The contamination of chlorate of potash with chloride of potassium.

η. The adulteration of powdered rhubarb with turmeric.

θ. The presence of sulphate of lime in *Hydrargyri Ammonio-chloridum*, Ph. Lond.

5. Enumerate and classify alimentary principles; give their chemical formulæ; and state the uses which they respectively serve in the animal economy.

6. Name the substances respectively marked A and B.

N.B. *The use of a Microscope will be supplied to those who desire to avail themselves of its aid in answering the last question.*

Saturday, August 17.—Morning, 10 to 1.

*Botany.***Examiner, Rev. Prof. HENBLOW.**

1. Define the terms *Carpella*, *Commisura*, *Raphe*, *Distichus*, *Thyrus*.
2. Confine your descriptions of the spe-

cimens on the table, by giving specific answers to such of the following particulars as they may be suited to exemplify:—

- a. STEM. General character.
- b. LEAVES. Arrangement.
- c. ——— Stipulation.
- d. ——— Composition.
- e. ——— Form.
- f. ——— Margin and Incision.
- g. ——— Venation.
- h. INFLORESCENCE. General Character.
- i. ——— Bracteal appendages.
- j. FLOWER. Peculiarities of Calyx.
- k. ——— Corolla.
- l. ——— Stamens (as to insertion, &c.)
- m. ——— Disk.
- n. ——— Pistil (as to Ovary, Style, Stigma, Placentation.)
- o. FRUIT. General Character.
- p. ——— Dehiscence.
- q. ——— Placentation.
- r. ——— Arrangement of Seed.
- s. SEED. Reasons for considering the Embryo to be Monocotyledonous or Dicotyledonous, if it should not be present or happens to be indistinct in the specimen under examination.

- f. PLANT. Reasons for referring it to a particular natural group (Order or Genus) to which it may belong or nearly approaches.

3. What are the chief adventitious inorganic constituents found in plants, naming a few examples in which one or other of them are peculiarly abundant?

4. What are the conditions essential to the germination of seeds?

5. State some of the most remarkable phenomena attributed to the peculiar irritability manifested in some plants.

ABOLITION OF INTRAMURAL INTERMENTS. —THE WALKER TESTIMONIAL FUND.

A HIGHLY respectable body of gentlemen have associated themselves for the purpose of raising a public subscription as a reward to Mr. G. A. Walker for the services he has rendered to the cause of sanitary reform, in promoting the abolition of interments in the metropolitan graveyards. We extract the following paragraph from the report recently issued by the Committee:—

"It has been justly remarked, that 'of all the great undertakings by which the era is signalized, there is none, perhaps, which so clearly stamps a character of real and essential progress as the sanitary movement; for the result of this, mediate and immediate, is a positive, a cumulative good,—a social, moral, and intellectual amelioration of a most beneficial nature, one which is destined to effect great issues in the material advancement of the people at large.'

But this reform, as regards intramural sepulture, will be attended likewise by great economic gains. By the showing of the Board of Health on this subject, the Metropolis alone will benefit by an improved system of burial without the walls to the extent yearly of *three hundred and fifty thousand pounds*; and taking into view also the savings contingent upon the adoption of this better system throughout the cities and towns of the United Kingdom, it is not unreasonable to anticipate that an aggregate annual saving will arise of at least two millions. Can, then, the householders of London, and the kingdom generally, fail to recognise and reward the man whose claim to their gratitude stands lower upon this score, than upon those other and higher considerations which have been already enumerated? We have recently seen the munificent testimonial which has rewarded the successful exertions of a fellow-labourer with that League whose united efforts repealed the corn laws. The Committee point to the £74,000 Cobden Testimonial Fund. Let it not be said that the single-handed champion, whose indomitable spirit, in the face of prejudice, corruption, and error, has overthrown the crying abomination of burying the dead among the hearth-seats of the living, will want a league of contributors to his Testimonial Fund. Let it not be said that a lesser meed of thanks is owing to the man whose incessant labours have secured to us immunity from poison and death in the air we breathe, than to him who has thrown open the gate to toiling and willing industry, or that life and death are of less intrinsic value than the means of existing prosperously. Pure air is more necessary in crowded cities than even a free supply of food, as doubtlessly the returns of the Registrar-General will ere long, now that this nuisance is abolished, sufficiently prove. Henceforth every year in London sixty thousand fresh corpses will cease to give off many thousand times the bulk of their own bodies in the corruption of the atmosphere. We give testimonials to the brave soldier, the gallant sailor: let us not be behind-hand in rendering the tribute of justice to the meritorious citizen who has applied his knowledge of the science of medicine to the acquisition of a peaceful victory over the united powers of ignorance, superstition, self-interest, and prejudice."

* * * We regret to find that the Government has declined making any remuneration to Mr. Walker for the important services which he has rendered to the public. We trust the Committee may meet with that success which such a laudable undertaking deserves.

SOCIETY FOR RELIEF OF WIDOWS AND ORPHANS OF MEDICAL MEN IN LONDON AND ITS VICINITY.

THIS Society was formed in the year 1788, with the object of establishing a fund for the relief of those widows and orphans of deceased members who might need and be deserving of assistance.

It unites the advantages of a Provident with those of a Benevolent Society. It is provident, as the members may through it protect their families from destitution; and it is benevolent, as its benefits are conferred on those who are left in indigent circumstances. All duly qualified members of the profession residing within the limits of the Society are eligible for proposal, and the mode of admission is by ballot.

The Society's affairs are managed by a President, twelve Vice-Presidents, three Treasurers, and twenty-four Directors, who are elected by the members annually, and whose services are gratuitous.

The Society has been greatly assisted by legacies and contributions from members and others, and has especially to acknowledge the gracious patronage of various members of the Royal Family.

The permanent efficiency of the Society depends on the maintenance of a due proportion between the available income and the claims made on the funds. Computations have been made by experienced actuaries on all points susceptible of calculation by way of average.

The proportion of members whose families may become claimants for relief scarcely admits of calculation; but the experience of more than fifty years has proved the wisdom and benevolence with which the Society was planned, and the prudence and efficiency with which its affairs have been conducted.

The Society is enrolled among Friendly Societies. The capital stock, converted into sterling money, is invested in the Bank of England at compound interest, and amounts to more than £45,000.

80 widows and 37 children have been relieved since 1792. 31 widows and 20 children are now receiving relief, which amounts to more than £1,400 per annum.

Laws, lists, and forms of proposal, may be obtained from the Secretary, and will be sent by post on receipt of two stamps.

C. E. WALSH, Secretary.

43, Half Moon Street.

THE HAHNEMANN HOSPITAL AND ITS SUPPORTERS.

At a general meeting of the Board of Management held at the Hospital, 39, Bloomsbury Square, on Saturday last, the following gentlemen were elected medical

officers:—Jas. Chapman, Esq., M.A., Cantab., and M.D.; E. C. Chepmell, Esq., M.D.; P. F. Curie, Esq., M.D., Paris and Aberdeen; R. E. Dugeon, Esq., M.D.; Thos. Engall, Esq., M.R.C.S.; Jos. Hands, Esq., M.R.C.S.; A. Henriques, Esq., B.L., M.R.C.S.; H. Kelsall, Esq., M.D., F.R.C.S.; H. V. Malan, Esq., M.D.; M. Roth, Esq., M.D.; D. Wilson, Esq., M.R.C.S. The Hospital, which has for its President that amiable nobleman, Lord Robert Grosvenor, is to be opened for the reception of patients on the 16th of October.

If we record this event, it is to express our regret that M.D.s, F.R.C.S.s, and M.R.C.S.s, are to be found lending their support to principles of treatment which are based on dogmatic assumption, or on the most palpable absurdities connected with the alleged action of medicines. It is highly creditable to the Apothecaries' Society that not one of their licentiates is to be found in this list. When men who must have received a regular medical education thus put themselves forward as supporters of a gross delusion, the public mind is likely to be unsettled regarding the teaching and practice of medicine in our schools and colleges. If Hahnemann's principles be *bonâ fide* carried out in the new Hospital in cases of acute disease, we predict that the emoluments of the coroner for Middlesex will be very considerably increased.

THE CHOLERA AT MALTA.

LETTERS from Malta of the 22d September announce the death of Surgeon Thompson, of the 69th Regiment. The non-commissioned officers and men have requested permission to subscribe three days' pay for the erection of a monument to him. The cholera had sensibly abated in Malta as to the number of persons attacked, but its power on those attacked is not diminished, death but rarely following within eight hours.

ON THE PER-CENTAGE OF NITROGEN AS AN INDEX TO THE NUTRITIVE VALUE OF FOOD. BY DR. A. VOELCKER.

THE object of this paper was to show that the usual estimation of the nutritive qualities of an article of food is frequently attended with inaccuracies, which renders it desirable to modify our present methods in this respect in many cases. A circumstance which leads to considerable error is the presence of ammoniacal salts in the juices of plants. In order to prove experimentally the presence of ammoniacal salts in larger quantities than hitherto suspected, and to avoid the objection that they might result from a partial decomposition of albuminous substances during the

analysis, the author chose fungi for his experiments, which are rich in nitrogen, and known as being highly nutritious. The species used was *Agaricus premellus*, a species which is edible, and remarkable for forming most beautiful fairy rings. After having separated all soluble protein compounds by means of basic acetate of lead, which re-agent throws down these completely, the amount of nitrogen still present in the juice of these agarics in the form of ammoniacal salts was found to be 0.204 per cent. for the fresh fungi, or 1.68 per cent. for the dry fungi. The whole amount of nitrogen in the same agarics, collected at the same time, determined by combustion, was found to be 0.74 per cent. for the fresh fungi, or 6.61 per cent. for the fungi dried at 212° F. Deducting from the last stated numbers the quantity of nitrogen found to exist in the juice in the form of ammonia, we find that only 0.586 per cent. of nitrogen in the fresh, or 4.799 per cent. of nitrogen in the dry fungi, exists in the state of protein compounds, and that nearly one-third of the nitrogen obtained by direct combustion exists in the form of ammonia in the juice, or, at all events, in the form in which the nitrogen adds nothing to the nutritive value of the fungi. The nutritive value of fungi has thus been overrated considerably; and there can be little doubt that the same is the case with many vegetables, which, according to the author's experiments, contain sometimes considerable quantities of ammonia in the form of ammoniacal salts.

Dr. CHRISTISTON remarked that he had long been convinced that there was a considerable fallacy in the methods of determining the value of nitrogen, and he hoped Dr. Voelcker's communication would direct inquiry in a more satisfactory direction.—*British Association*, 1880.

ON THE REMOVAL OF THE INCrustATION WHICH FORMS IN THE BOILERS OF STEAM ENGINES.

At the recent meeting of the British Association at Edinburgh, a paper by Dr. G. Wilson, On the Incrustation of Boilers, was read by Dr. Davy. Dr. Wilson observes—"Considering the composition of the incrusting matter, and the properties of its principal ingredient, the sulphate of lime—a compound soluble in water and in sea water, and deposited only when the water containing it is concentrated to a certain degree—there appears to be no difficulty theoretically in naming a preventive. The certain preventive would be the substitution of distilled or rain water in the boiler for sea water. Of this we have proof in the efficacy of Hall's condenser, which returns the water used as

steam, condensed, after having been so used; but, unfortunately for its practical success, the apparatus is described as being too complicated and expensive for common adoption. Further proof is afforded in the fact that the boilers of steamers navigating lakes and rivers in the waters of which there is little or no sulphate of lime, month after month in continued use, remain free from incrustation. This I am assured is the case with the steamers that have been plying several summers successively on the lake of Windermere. And it may be inferred, that in sea-going steamers in which sea water is used in the boiler—or, indeed, any water containing sulphate of lime—the prevention of deposition may be effected with no less certainty by keeping the water at that degree of dilution at which the sulphate of lime is not separated from the water in which dissolved. From the few trials I have made, I may remark that sulphate of lime appears to be hardly less soluble, if at all less, in water saturated with common salt than in perfectly fresh water. This seems to be a fortunate circumstance in relation to the inquiry as to the means of prevention, and likely to simplify the problem. If these principles be sound, their application under different circumstances, with knowledge and judgment on the part of the directing engineer, will probably not be difficult. His great object will be, in sea-going steamers, to economize the escape of water in the form of steam, and thereby also economize heat and fuel; also, when fresh water is available, to use it as much as possible; and further, to avoid using sea water as much as possible near coasts, and in parts of seas where sulphate of lime is most abundant. From the incrustation on the boilers of sea-going steamers, the attention can hardly fail to be directed to that which often forms, to their no small detriment, in the boilers of locomotive railway engines, and of engines employed in mines and in the multifarious works to which steam power is now applied. These incrustations will of necessity be very variable, both in quantity and quality, according to the kind of ingredients held in solution in the water used for generating the steam. Hitherto I have examined two specimens only of incrustations taken from the boilers of locomotive engines, and a single one only from the boiler of a steam engine employed on a mine—a mine in the west of Cornwall. The latter was fibrous, about half an inch thick, and consisted chiefly of sulphate of lime, with a little silica and peroxide of iron, and a trace of fluorine. The former were from one-tenth of an inch in thickness to one inch. They were laminated, of a grey colour, and had much the appearance

of volcanic tufa: they consisted principally of carbonate and sulphate of lime, with a little magnesia, protoxide of iron, silica, and carbonaceous matter—the last two, the silica and carbonaceous matter, probably chiefly derived from the smoke of the engine and the dust in the air. From the engineer's report it would appear that the thinnest—the incrustation of about one-tenth of an inch—had formed in about a week, during which time the locomotive had run about 486 miles, and consumed about 10,000 gallons of water.

OBITUARY.

On the 17th of July, at 7, Allason Terrace, Kensington, of apoplexy, Mr. Francis Morgan Walker, surgeon, late of Chesterfield, Derbyshire.

On the 15th of April last, at Hobart Town, Van Diemen's Land, Thomas Gibson, Esq., Donno, Scotland, surgeon, R.N.

On September 12th, at Carlou, Launceston, Armstrong, Esq. M.D. late of Dublin.

Of small-pox, Joseph Robinson, M.D., of Omagh.

Selections from Journals.

ON THE EPIDEMIC CEREBRO-SPINAL MENINGITIS OBSERVED AT VAL-DE-GRACE, 1848, 1849. BY DR. MICHEL LEVY. CHIEF PHYSICIAN.

THIS epidemic has manifested so fatal a predilection for the French Army, that there is scarcely one important garrison that has not suffered from its ravages. Paris, which had before escaped, has become the seat of its influence during the last sixteen months.

The first case appeared in the Military Hospital at Val-de-Grâce in Dec. 1847: from this date to Feb. 1849, a period of fifteen months, ninety-nine cases were presented.

The following table indicates the progress of the disease, and the rate of mortality:—

1847. Dec.	1 Case.	1 Death.
1848. 1st. q.	12	6
" 2nd "	26	12
" 3rd "	12	5
" 4th "	23	12
1849. Jan.	11	9
" Feb.	14	12

Dr. Levy then refers to the epidemic constitution of the previous year in relation to the prevalence of this cerebro-spinal meningitis.

From the statistics of the cases admitted into Val-de-Grâce, it appears that, of six thousand four hundred and ninety-eight patients received from Jan. to Dec. 31st, 1849, there were only ninety-eight cases of meningitis; that during the third quarter, when the number of admissions was at its maximum, there were only twelve cases of

meningitis; that not only did the ordinary pathological features of the year manifest themselves in the increase of the affection of the respiratory and digestive organs during the winter months, but that other epidemics of greater intensity prevailed simultaneously; such as typhus fever, dysentery, variola, measles, and scarlatina. The meningitis now under consideration cannot, therefore, be regarded as a true epidemic, because it has neither attacked a large number of individuals, nor are its attacks limited to a restricted period; neither have they modified the characters of other epidemic diseases. It has been known occasionally at Val-de-Grâce for many years, but has increased in frequency since the close of 1847, and has not prevailed in other hospitals. The constitution of 1848 was seen in the tendency to miasmatic and infectious diseases marked by a typhoid and adynamic character. Dr. Levy assigns the origin of the diseases of this year to a prevalent morbid condition of the blood.

It must be observed that this meningitis appeared between, and was to a certain extent merged in or complicated with, the influenza of 1847, and the cholera of 1849.

Mortality.—Of the ninety-nine cases received into the Val-de-Grâce, fifty-eight died; a proportion of 1 in 170. The mortality greatly declined towards the close of 1848, but rose again greatly in Jan. and Feb. 1849.

Of fifty-three deaths, thirty-eight occurred between the ages of seventeen and twenty-five years; and only fifteen between those of twenty-five and thirty-one years. The young men recently incorporated with their regiments furnished the greater portion of the fatal cases. This may in part be attributable to the excitement of novel duties, exercises, &c.; and in part to the irregularities and political excitement of their preceding life.

Precursory symptoms.—In twelve cases out of fifty-seven the attack was sudden, but in general the following precursory symptoms were observed: headache, vertigo, lassitude, horripilations, tremors, general malaise, nausea and vomiting.

Symptoms.—In most cases there was intense and intolerable frontal headache. Pain in the course of the spine, for the most part in the lumbar region, was produced by movement or the slightest pressure, and in some instances existed without being so excited. Pains in the limbs were constant symptoms. Rigors occurred during the convalescent stage, or when the disease was verging into a chronic state. At this period, a state of torpor, or general languor and debility, somewhat analogous to a certain stage of cholera, would frequently supervene. Impairment of the senses of sight and

hearing not unfrequently remained after the attack. Rigidity of the spinal muscles of the cervical and dorsal regions existed in the majority of instances. Trismus, opisthotonos, pleuristhotonos, strabismus, contraction of the limbs, and tetanic spasms, were occasionally met with. In some instances delirium accompanied the early symptoms, and was followed by coma. Vomiting was a frequent symptom: constipation was constantly observed, and extremely difficult to overcome. The tongue was generally normal at first, then becoming whitish passed through several shades till it presented a typhoid aspect. In twenty-six cases a peculiar pearly appearance of the gums was noticed. Anorexia was constant. The pulse throughout the disease was hard and sharp, and varied in number from 60 to 120, in individual cases. The face in many cases was florid. The blood in most instances was cupped, when drawn at the commencement of the disease, and generally exhibited an excess of fibrine. Respiration was hurried; the forehead hot, the surface of the trunk was warm, the extremities cold. During return to health it was not easy to maintain the warmth of the body. In twenty-eight cases various cutaneous eruptions, *e. g.* erythema, erysipelas, petechiae, sudamina, herpes, rubecloid spots, &c. were met with.

The complications were for the most part of a typhoid or choleraic character.

Course of the disease.—Distinct stages were not observed. The disease was usually ushered in by smart febrile symptoms accompanied with delirium, in some cases alternately with coma. The cases varied according to the predominance of any certain set of symptoms. The nearest division that could be made was that of complete and incomplete meningitis. In some cases only headache, cervical rigidity, vomiting, and constipation, were observed until febrile reaction changed or developed the full characters. In other cases the entire characteristic symptoms were rapidly developed. The epithet "typhoid" was applied to those cases in which a state of torpor analogous to the collapse of cholera occurred.

For all practical purposes the epidemic meningitis may be divided into the three forms of congestive, purulent, and hydrocephalic; the first form being that of the sudden seizures and rapid cures occasionally met with. Under either form, when the course of the disease was prolonged, it manifested remissions in the symptoms, and even a periodicity giving an intermittent character.

The course of the disease seemed in some cases to be cut short by an increased discharge of urine. The urine did not contain

albumen, but a considerable quantity of uric acid and urates was noticed when the critical discharge occurred. In a few cases profuse perspiration preceded a favourable termination. Haemorrhage and diarrhoea exerted no favourable influence on the course of the disease.

The duration of the disease varied from eight to sixty days.

In ten cases, the period of the disease was from thirty to a hundred and fifty-one days, convalescence having been interrupted by pleurisy in one case, by obstinate headache, irregular or periodic, in four cases, twice by typhoid symptoms, once by deafness, and twice by persistent vomiting.

Pathological anatomy.—Dr. Levy's observations under this head are deduced from forty-four autopsies.

In fifteen there were noticed fulness of the vessels, injection of the pia mater, and sanguineous extravasation within the arachnoid.

In nine cases there was a marked abundance of bloody points in the out cerebral surface.

In five, injection of the ventricular surfaces.

In twenty-seven, purulent effusion on the convexity and at the base of the brain.

In twenty-seven, purulent effusion, situated almost exclusively on the posterior surface of the spinal cord; eight times on both anterior and posterior aspect, and four times it extended to the *cauda equina*.

In eighteen cases there was effusion of serum and lymph into the cavities of the ventricles.

In eleven instances thoracic disease was found: viz. two, pneumonia; nine, pulmonary congestion; one, sero-purulent effusion into the pericardium.

In the latter case inflammation of the joints co-existed.

Treatment and results.—Of sixty cases noted, thirty-one deaths occurred from the natural course of the disease; seven from hydrocephalic disease; twelve were completely cured; and ten incompletely cured.

The means generally employed were venesection, leeching, cupping, blistering, and calomel.

Conclusions.—1. The cerebro-spinal meningitis which has presented itself at Val-de-Grâce during 1848-49, is identical with that which has been observed at Versailles, Strasbourg, Metz, &c.

2. It has prevailed concurrently with typhoid fever, but is otherwise distinct in its symptoms, course, lesions, and mortality.

3. It has not assumed a true epidemic character; at the time that it has prevailed, typhoid and other eruptive fevers have been met with in considerable numbers.

4. It has not been developed under the miasmatic conditions which give rise to periodic fevers.

5. With the exception of one or two cases of sudden seizure, the pathological lesions have generally borne a close relation to the symptoms, which they have thrown light upon; and this is not a feature in typhoid disease.

6. The rapid and extensive formation of pus is amongst its most constant and remarkable lesions.

7. The tendency to the formation of pus is the generic feature of a class of affections in which the blood becomes diseased, and is also a part of this cephalo-rachidian meningitis. The special tendency to the deposition of pus in the cerebro-spinal axis constitutes its specific character.

8. The medical constitution of 1847-48 explains the prevalence of this class of diseases.

9. By the demonstration of the non-identity of this cerebro-spinal meningitis with typhus, its non-identity with typhoid fever is also demonstrated.—*Gazette Médicale*.

. An epidemic of a similar character to the above has been noticed by Dr. Duncan of Dublin, and of which mention will be found in our review of Dr. Duncan's Clinical Lectures, at p. 552 of our 44th volume.

PROCESS FOR THE QUANTITATIVE EXAMINATION OF OPIUM, AND SEPARATION OF MORPHIA. BY M. GUILLEMOND. LYON.

TAKE 15 grammes (=8·8 Eng. drachms) of the opium to be examined; and, having cut it into small pieces, mix it in a mortar with 60 grammes (=15·4 Eng. drachms) of alcohol, at 180° Fahr. This mixture is to be filtered through fine linen, and the dregs to be treated again with 40 grammes (=10·3 Eng. drachms) of alcohol, at the same temperature as before. The two tinctures are to be mixed in a large wide-mouthed flask, 4 grammes (=1·028 Eng. drachms) of Liq. Ammoniac being added. The result of the process is obtained in twelve hours. The morphia will be separated, and found lining the sides of the vessel in tolerably large, coloured crystals, of a gravelly feel. Mixed with these in a small proportion are the pearly, needle-like crystals of narcotine. These crystals thus mixed are to be placed on linen, and washed several times with water, to remove any portion of meconate of ammonia which may be adherent to them. The crystals are then to be collected, and placed in a tube full of water. The narcotine, which is very light, will remain suspended in the water, while the heavier morphia will be de-

posited, allowing the narcotine to be separated by decantation. The quantity of morphia in any given specimen of opium can thus be ascertained by the weight of the deposit. Good opium should yield a proportion of $1\frac{1}{4}$ to $1\frac{1}{2}$ of crystals of morphia to 15 of the solid opium—i. e., about ten per cent.

M. Miahle has found the preceding mode of analysis succeed to his satisfaction; but he proposes as an improvement, that, having dried the mixed crystals, they should be triturated in a small quantity of ether, whereby the narcotine will be more readily and more completely separated from the morphia.

M. Miahle states also, that by the employment of this process in the examination of various specimens of opium he has confirmed the statement of Vauquelin and Dublanc, that indigenous opium contains a larger proportion of narcotine than of morphia, and is therefore not adapted for therapeutic purposes.

M. Miahle further states that he has found as great differences in various specimens of official opium as from a half to ten grammes of morphia in 100: so necessary is it that the examination of the drug should precede its employment. To constitute good opium, the proportion of morphia should be from six to nine per cent., or, as a mean, $7\frac{1}{2}$ per cent.—*L'Union Médicale*. X

ON GLAIRINE, GLAIRIDINE, AND ZO-IODINE IN MINERAL WATERS. BY M. BONJEAN.

THE gelatinous matter frequently met with in mineral waters was regarded by Borden as sometimes a fatty matter, and sometimes as of a bituminous nature. Vauquelin stated that it was analogous to albumen.

Different names had been given to this substance, by various authors, taken from the characters of the waters in which it had been met with. Anglada has recognised the identity of all these varieties, and has named the substance *glairine* from its glairy aspect.

M. Bonjean has discovered a variety which he designates *glairidine*; and has also discovered another product of a violet colour, which he denominates zo-iodine.

M. Duby, an eminent Genevese botanist, has examined the substance at the request of M. Bonjean, and declares it to be an extremely delicate plant of the finest texture, and so folded on itself as to have the appearance of animal detritus.

M. Bonjean gives the following results of his investigations into the nature of *glairine*:—

1. It contains very little nitrogen.
2. It contains no iodine.
3. It is slightly soluble in water, alcohol,

and spirits of turpentine; more so in the concentrated acids, whence it is again precipitated by alkalis in the form of bluish flocculi. Heat, in all cases, favours its solution.

4. It is entirely insoluble in ether.

5. When separated from a mineral water it rapidly becomes of a greyish colour; but in contact with nitric or hydrochloric acids, chloride or bromine, it immediately regains its white colour.

6. Strong alkalis change its colour to green.

7. It is inodorous in water. Separated from that fluid it acquires a repulsive odour.

8. By drying it loses its odour, and at the same time is reduced to one-tenth its previous weight.

9. Hydrochloric acid separates a portion of iron. Iodine gives it a brick-red colour. *Glairidine* differs but little from *glairine*. Its colour is naturally grey; it emits no odour. Acids and alkalis do not change its colour as they do that of *glairine*: it contains traces of iodine.

When *glairine* is separated from its mineral water, the water which passes through the filter is at first nearly colourless, and at the end of two or three hours deposits spangles of a beautiful violet colour, and strongly iridescent. These are the new substance *zo-iodine*, inodorous, insipid, insoluble in water, permanent in the air, reddened by acids, and made brown by alkalis; when burnt yielding the odour of burnt horn.

M. Bonjean has also detected *glairine* in some earths and minerals.—*Journal de Chimie Médicale*. X

BOOKS & PERIODICALS RECEIVED

DURING THE WEEK.

- The Assurance Magazine. No. 1, Sept.
On the Origin of Inflammation of the Veins, &c.: being the Jacksonian Prize Essay for 1850. By Henry Lee, F.R.C.S. &c.
Hydrocephalus re-considered, and its Relations to Inflammation and Irritation of the Brain defined, &c. By Thomas Weedon Cooke, M.R.C.S. &c.
Practical Observations on the Diet of Infancy and Childhood. By T. H. Barker, M.D.
Oppenheim's Zeitschrift für die gesammte Medicin. Januar to März 1850.
The British and Foreign Medico-Chirurgical Review. No. 12, October.
London Journal of Medicine. No. 22, October 1850.
Edinburgh Medical and Surgical Journal. October 1850.
Edinburgh Monthly Journal of Medical Science. October 1850.

Pharmaceutical Journal and Transact.
October 1850.
The Veterinary Record and Transact.
October 1850.

BIRTHS & DEATHS IN THE METROPOLIS

During the Week ending Saturday, Sept.

BIRTHS.		DEATHS.	
Males....	708	Males....	8
Females..	733	Females..	6
1441		14	

CAUSES OF DEATH.

ALL CAUSES	14
SCANDALOUS CAUSES	2
1. Symptomatic (or Epidemic, Infant Contagious) Diseases	8
Sporadic Diseases, viz.—	
1. Dropsy, Cancer, &c.	1
2. Brain, Spinal Marrow, Nerves, and Senses	1
4. Heart and Bloodvessels	1
5. Lungs and organs of Respiration	1
6. Stomach, Liver, &c.	1
7. Diseases of the Kidneys, &c.	1
8. Childbirth, Diseases of Uterus, &c.	1
9. Rheumatism, Diseases of Bones, Joints, &c.	1
10. Skin	1
11. Premature Birth	1
12. Old Age	1
13. Sudden Deaths	1
14. Violence, Privation, Cold, &c.	1

The following is a selection of the most Deaths from the most important special

Small-pox	8	Convulsions	1
Measles	13	Bronchitis	1
Scarlatina	15	Pneumonia	1
Whooping-cough	30	Phthisis	1
Diarrhoea	57	Lungs	1
Cholera	4	Toothache	1
Typhus	43	Stomach	1
Dropsy	11	Liver	1
Hydrocephalus	37	Childbirth	1
Apoplexy	97	Uterus	1
Paralysis	17		

REMARKS.—The total number of births was 41 below the average mortality of the last out of ten previous years.

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer	30
" " " Thermometer	54
Self-registering do.	Max. 64 Min. 4
" From 12 observations daily.	54

RAIN, in inches. 45.—Sum of the daily variations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature the week was 1° below the mean of the month.

NOTICES TO CORRESPONDENTS.

The communication of Mr. E. W. Roberts will be inserted.
The pamphlet forwarded by Dr. Kennedy to receive our attention.
Dr. F. Steel's letter and enclosure have come to hand. The subject shall be noticed in a future page.
Mr. H. C. Johnson.—The Address will be inserted and the request of our correspondents complied with.

Lecturers.

INTRODUCTORY REMARKS

to

*The Opening of the Session 1850-51, at
St. George's Hospital.*

BY HENRY CHARLES JOHNSON, ESQ.
Assistant-Surgeon to the Hospital, &c.

SIR,—I could have wished that one of the officers of this establishment more competent than myself had been selected for the purpose of opening the proceedings of this day. Most sensible am I that my powers are inadequate to do full justice to the subjects I have to submit to your consideration.

Since the existence of a school connected with this hospital, it has been the custom of the lecturers, at the commencement of each winter session, to address to those gentlemen who honour them by their presence, some introductory remarks, either on the general conduct of the school, or on matters connected with the plan of education to be pursued by those about to enter upon their professional career.

Of the utility or expediency of such annual addresses many opinions prevail; but custom has hitherto held its sway, and no school in this metropolis considers that it has fulfilled its duties, or the expectations of its supporters, without its annual introductory lecture.

My colleagues, however, have deemed it advisable to discontinue this practice, and to open the present session by distributing to the meritorious students of this school, those rewards and distinctions which they have gained by their industry and talents. They have considered that it would be more gratifying to this meeting, and more especially to those who have received their education at this hospital, to witness the success attendant on the efforts of some of the junior members of our profession, rather than listen to the dry and uninteresting detail of an ordinary introductory lecture. They felt, moreover, that a wholesome stimulus would be given to the exertions of those about to commence their studies, to gain by their efforts similar rewards and distinctions, to those which will be conferred this day on the most deserving students of preceding years.

Before I present to you the successful candidates for the various prizes offered in this school, I may perhaps be allowed to make a few remarks on the institution

which these gentlemen have adopted as their alma mater, and point out to them the distinguished position they may attain, if they do but follow in the track of many of their predecessors, who, with much fewer opportunities than those which they possess, attained a fame and eminence in their profession to which I would have them aspire.

It is little more than a century ago that St. George's Hospital reared its head from being a small infirmary, in Petty France, in Westminster, and subsequently in Chapel Street, to the rank and standing of a metropolitan hospital. In 1733, the subscribers finding that their patients increased, and that a corresponding improvement took place in their funds, exerted themselves to find a house in a more airy and convenient situation, and at length procured a lease of Lanesborough House, at Hyde Park Corner, at an annual rent of £80, as being, on account of the largeness and strength of the building, and airiness of the situation, most convenient for the purposes of the charity. The improvement in the funds soon afterwards enabled the governors of the institution to extend their benevolent views, and for that purpose to purchase, of the Dean and Chapter of Westminster, the freehold of the ground whereon the hospital now stands, in order to settle the charity on a permanent foundation. For a century Lanesborough House remained much in its original form; it was then raised to the ground, and the present splendid edifice in which we are now assembled, was, by the munificence of the governors and the liberality of the charitable public, erected in its place. Upon the incalculable benefits thus bestowed on the sick poor I need not dwell on the present occasion; they are echoed through the homes of the afflicted and the destitute.

From the foundation of this hospital, not only have the sick and lame received aid, comfort, and skill, in the alleviation and cure of their diseases, but the practice, the science, and the literature of our profession have been enriched by the labours of those who have gleaned their information within its wards.

In the early part and towards the middle of the last century, William Hunter flourished, and enriched our knowledge on many important subjects connected with anatomy, surgery, and midwifery. From him we learned the uses of the absorbent system. To him we are indebted for some interesting pathological facts connected with the joints, the discovery of the tubuli testis, of the ducts of the lachrymal gland, and other useful researches in anatomy and medicine too numerous to mention on the present occasion. His splendid ma-

scum, and his work on the Gravid Uterus, place him in the foremost ranks of the eminent of our profession. His life and persevering zeal may be considered as a model for future men to emulate and to admire. He, sir, was a pupil of this hospital!

After his time a long succession of illustrious names adorn the annals of St. George's. John Hunter soon followed his brother William; and in this institution did he collect those facts and materials from which, by his acumen and powers of classification and arrangement, he deduced those great principles which, I may justly say, have proved the beacon and the guide in the treatment of disease, to the admiring and enlightened countries of the world.

Nor, sir, do we cease with the fame of the Hunters. It was here that Cheselden operated for the stone, which was considered in his time, and justly so, as an operation of danger and difficulty. A curious resolution stands on the books of our hospital about this period, which says:—

"That the cutting for the stone be left to such surgeons as think proper to perform the same." The mode of operating, and the instruments wherewith it was performed, although determined by Cheselden, were improved by Sir Caesar Hawkins; and the gorget which he here employed still remains in use amongst many of our best surgeons of the present day.

Towards the conclusion of the last century and the commencement of this, we find the followers of John Hunter securing and extending their reputation by his precepts and his example. Sir Everard Home may be said to be the pupil of John Hunter, and from him gleaned many of the materials for producing his magnificent work on Comparative Anatomy, which must ever remain a monument of his industry and his fame.

Nor, sir, is it fitting in me to hold up alone for imitation the great surgeons who have adorned this institution.

By one of the physicians of this hospital was that stimulus and zest given to the study and cultivation of morbid anatomy, which has raised to the level of the higher sciences the practice of our profession. Till the time of Baillie, morbid anatomy, the most important of all investigations in the knowledge and treatment of disease, was to a certain extent neglected in the schools. From his time, however, pathological inquiry has been carried on with an energy and success which has spread from hence to all the great capitals of Europe where facilities are afforded for its culture and pursuit. To morbid or pathological anatomy must we look for the abolition of empiricism, and to more per-

fect and surer methods of relieving human suffering.

Drs. Pemberton and Heberden, sir, afford us examples, in the strict and lucid manner in which they portrayed disease, in their forcible and elegant dissertations on some of the most interesting and important points in the practice of medicine; and Dr. Hope, more recently amongst us, may be said to have contributed largely to our information respecting the diseases to which the heart and circulation are prone.

Nor, sir, has general science failed to gather its triumphs from those who have gone before in their useful career at the bedside of the patients of this charity. The names of Young and Wollaston enrich the catalogue of our predecessors. The former brought to bear all those great truths with which natural philosophy furnished him, upon his efforts to alleviate the miseries of those submitted to his care. True philosophy and the profoundest learning marked the character of the late Dr. Young.

The latter, Dr. Wollaston, was pre-eminent for his chemical knowledge and research; and though he failed in obtaining his wish to become an officer of this establishment, yet we enjoy the merit of ranking him amongst the most renowned and popular of those who have studied in this school.

If, sir, we consider the advantages which are enjoyed by the sick poor, and by the profession to which we belong, from the existence of public hospitals, we shall find that they weigh but light in the scale against the real and substantial good which accrues to the country at large, and its numerous dependencies abroad, through the medium of the schools of medicine which are everywhere connected with them.

This hospital in its earlier days laboured under the disadvantage of possessing no real property on which to found its basis. Its very existence depended on its good name, and on the charitable wishes and benevolent views of its original founders and supporters. A delicacy (and though a mistaken a very natural one) prevailed lest the feelings of those who were unfortunately the subjects of disease might be intruded upon and offended by extending the knowledge of their sufferings beyond the observation of their actual medical attendants.

But happily, sir, for society at large, these highly to be commended, but mistaken notions, no longer exist, and in our present more extended views and ideas on the dissemination of knowledge, a school of medicine is considered as much an integral part of a public hospital, as any other adjunct which may be employed for

the benefit of the sick poor who come within its walls.

Sir, so fully has this become a general rule and principle, that in some establishments devoted to learning and education, a school of medicine has been formed long before those institutions possessed efficient hospitals of their own to illustrate the precepts which they taught, the school being considered as the surest foundation on which to raise the superstructure of their future asylums for the sick and needy.

The necessity of teaching, and consequently of a school of medicine connected with St. George's Hospital, was determined by a resolution of the governors as far back as the year 1793. Amongst other regulations, the following appears:—

"That an operation be performed on a dead body, attended by explanations, or a lecture given on some of the principal parts of surgery, once a week, by one of the surgeons of the hospital in rotation, for nine months in the year."

Here, sir, do the governors enforce on us the necessity of teaching. From that time to the present has a school of medicine gradually developed itself, and, as it now exists, may vie with the oldest and most wealthy of this metropolis, affording as it does every facility for the successful study of disease.

The school, moreover, has received the fostering care of the governors at large, who by their liberality have lately supplied us with suitable accommodation for teaching anatomy and chemistry, in premises set apart from the hospital itself. These, together with the other appurtenances of lecture-rooms and museums within this building, have placed the necessities of the school in a most efficient form.

While the Governors have thus offered inducement to the students of this hospital for the successful cultivation of medical science and pursuits, the teachers have not been backward in holding out rewards to industry, talent, and good conduct. Scholarships and prizes have been instituted as premia to knowledge and exertion. Nor, sir, have others, at one time connected with this hospital, and who feel that an impetus should be given to medical education and aspiring talent, been wanting in their generous efforts to promote the welfare of the School. Your own name, sir, and that of Dr. Chambers, stand on the list to-day as bestowing prizes on the subjects of clinical medicine and clinical surgery.

In selecting, sir, as I have done, a few of the most remarkable individuals to whose lives and labours I would direct the junior part of my auditory, I would have them bear in mind that we have yet amongst us those whom they would do well to imitate,

as examples of professional zeal and professional success,—those who have contributed by their works to establish principles in the treatment of disease, which will long outlive their worldly career, and secure a lasting reputation for themselves, and for the school in which they have taught and laboured.

In the present remarks I have offered no novelties to attract your attention. I considered that a brief sketch of the origin and progress of this splendid charity, which may well be ranked amongst the best and proudest of this great city, might not be altogether uninteresting to some of my auditory; and that to the junior part I might point out the paths which are now open for the exercise of their talents and industry, that they may be led to zeal and emulation by the example of those ornaments of our profession, who have toiled in the same field they are about to occupy, and to stimulate them to advance, by their instrumentality, the good intentions of the supporters of this hospital, in promoting the advancement of science, and the amelioration of the sufferings of their fellow-creatures.

In conclusion, I must congratulate these gentlemen in receiving at your hands, sir, the distinctions which will be conferred on them this day. It must ever be a gratification to them in after life to feel that the honourable position which they hold in this School, was known to one who, by his industry, talent, and upright bearing, both in his private and professional life, has gained for himself the good wishes of the multitude, and conferred on this hospital and on British surgery a distinguished and an undying fame.

After the prizes had been delivered to the students, Sir BENJAMIN BRODIE made the following observations:—

Although several years have elapsed since I resigned the office of surgeon to the hospital within whose walls we are now assembled, I hope that I need not assure you of the great interest which I still feel in the prosperity of the hospital itself, and in the reputation of the medical school which is connected with it. It would indeed be strange if it were otherwise. It was here that I began the study of my profession, the practice of which has been the main object of my life. Whatever knowledge I have been able to obtain, whatever advancement professionally, indebted to my son; and it is any agreeable with my colleagues in Google

suits, the hopes and fears, and aspirations of my early life.

It was my good fortune to be elected assistant-surgeon to the hospital at a very early period; and partly as assistant-surgeon, and partly as surgeon, I was attached to it during the space of thirty-two years. In that interval great changes were accomplished. The old and inconvenient building (but which, nevertheless, afforded the means of affording a great reputation to Sir Cæsar Hawkins, Dr. Heberden, Sir Everard Home, Dr. Baillie, and a still greater one to Hunter) was exchanged for this handsome and commodious edifice, having all those advantages over the former one which greater experience and the more advanced state of science could afford. Let honour be given where honour is due! In adverting to this subject it is but just that I should add that it is chiefly to one individual that we are indebted for this great improvement in our condition. It was Mr. Fuller who first proposed that the hospital should be rebuilt. Many thought it a hopeless undertaking; and it may well be doubted whether, at that time, it would have been accomplished if it had not been for his determination and perseverance, and for the spirit which he infused into those who, in the first instance, were less sanguine than himself.

It is indeed to me a source of great satisfaction to find the St. George's Hospital of the present day, so much better adapted than that of former times, to all the purposes for which a hospital is required, whether as furnishing relief to the humbler classes of society when suffering from accidental injury or disease, or as a place of instruction for those who are being educated to the medical profession. There are, however, other recollections of a less pleasing character which force themselves upon me when I recur to the period of my early connection with this institution. Of those under whom I myself studied; of those who were my fellow-students; of those who were my colleagues; and even of those who were junior to myself, how many have disappeared! how many have passed into that region from whence "no traveller returns!" One individual, however, who was assistant-surgeon here when I was myself a student, and who was my colleague afterwards, still remains among us. During the period of thirty-two years Mr. Keate and myself laboured cordially together; and I may, I believe, venture to say that not only no unkind word, but that scarcely even an unkind thought, ever passed between us. It is not for me to determine how far we were successful; but I may say that we were alike anxious, and neither one of us more than the other, to render the

hospital more and more efficient, both as a receptacle for the sick, and as a medical school. You must excuse me if I take this opportunity of expressing how much I am myself indebted to the example which Mr. Keate has afforded me as an accomplished surgeon, a man of integrity, and a gentleman. Nor are these observations altogether irrelevant on the present occasion, as I am thus led to point out to the younger members of our profession whom I see around me, by what rules they should shape their course, if they would enjoy the regard and respect of others as they advance in life.

It is not, however, to those who already belong to our profession, but to those who are now engaged in study, and who aspire to belong to it hereafter, that I now more especially address myself. Many of you are here in the very outset of your career. You are for the first time in the metropolis for the purpose of attending lectures, of dissecting, of studying disease in the hospital. You are entering on an unknown region in which you have no experience of your own to guide you; and it is reasonable to suppose that you may derive some advantage from the experience of an older person, who has formerly been in the same situation with yourselves.

And first let me impress on your minds that the next three or four years are probably the most important and critical period of your lives. You are now to lay the foundation of that knowledge on which your future character—nay, your very subsistence, is to depend. Let these few years be wasted, and you can never redeem their loss; ceaseless but unavailing regrets will haunt you through the remainder of your days. But let the opportunities which are now offered to you be properly cultivated, and twenty years hence you will reap the advantages resulting from the labour which you may now undergo, and the endeavours which you may now make to keep within due bounds the very natural desires of youth for leisure and amusement.

It is well before you enter on these new pursuits that you should satisfy yourselves as to what will be really required of you, in order that those who feel that they have it not in them to make the necessary sacrifice may turn aside to some less arduous, though probably less honourable occupation.

You must be regular and constant in your studies: you must miss nothing; for what you learn one day is not only important in itself, but is necessary to the right understanding of what you are to learn on the following day.

*"Pater ipse colendi
Hand facilem esse viam voluit."*

At no time could any one qualify him-

self for our profession without devoting himself wholly to the task. But if that were true a century ago, how can it be otherwise now, when the competition is so much increased, and when there is so much greater a mass of knowledge to be obtained than formerly!

I leave it to your respective teachers to tell you what lectures to attend, and what hours you are to devote to the dissecting-room and hospital. The advice which I shall offer you as to the conduct of your studies is of a more general nature.

Take notes of your lectures, however brief, not, in the first instance, for the sake of transcribing them (for that is better done at a more advanced period of your education), but for the purpose of keeping up your attention to the lecture at the time, and of improving you in the habit of fixing your attention afterwards. The incapability of properly fixing the attention is one of the greatest difficulties under which medical students generally labour, especially those who do not come directly from school or college. Much here depends on the structure of the individual mind; much on early habit. Where the difficulty exists, some will overcome it more easily than others; but whatever may be the amount of effort that is necessary, the effort must be made; and I venture to say that, unless there be some actual deficiency of intellect, it will not be made in vain.

For many years past I have offered to the students of St. George's hospital a prize for the best series of clinical notes of cases, with commentaries on them. I was led to do so because I was satisfied that it is only by taking notes at the patient's bed-side that a student can pursue his hospital studies with advantage. You may go daily round the wards, staring at what is going on, and every now and then listening to an observation made by the physician or surgeon, but you will never obtain any precise knowledge, either of the progress of disease, or of the effect of remedies, unless you investigate cases for yourselves, with your note-book and pencil in your hands. The notes thus taken should be transcribed in the evening afterwards. You will find them the best things to refer to (as far as they go, very much better than books) in after-life; and in transcribing them you will find that many things occur to you which would not have occurred otherwise.

And this leads me to another subject. In what are called the Golden Verses of Pythagoras we are told that before we sleep at night we should review the whole of our proceedings during the day, with reference to our moral conduct; and very good advice it is, worthy of a Christian,

though it comes from a heathen philosopher. But I would carry it still further: always in the evening review your studies of the previous day, and endeavour to recal to mind the principal facts which have been presented to your observation. This will not only impress them on your memory, but it will give you the habit of thought and reflection. For observe, that it is not sufficient for you to store your minds with knowledge: a member of the medical profession, above most others, must learn to arrange his knowledge—to view facts in their relation to each other—in a word, he must be accustomed to think. If knowledge be necessary for him, it is not less necessary that his mind should be so trained that he may know how to use it. However well-informed he may be, he will otherwise prove to be a very inefficient practitioner.

There is one other subject on which it may be well that I should offer some advice to the junior part of my audience. You are now placed, for a time at least, in this great city, where there is much to interest you—much to attract your attention—besides those higher objects for which you are assembled here. You will have many inducements to be idle as to your studies; but let me urge you to be careful not to yield to the first temptation. If idle habits are once contracted it is difficult to alter them; and it is not less difficult to be rid of idle companions. I need not repeat to you, that idleness is incompatible with the acquirement of knowledge. But there is much more behind. Idleness leads to dissipation, and dissipation to extravagance; and then come debt, disgrace, loss of self-respect, moral degradation. Be careful in choosing your companions; exercise a proper self-control in the first instance: make it your business to pass through the period of your professional education not only with the character of diligent students but of gentlemen, in the best sense of the word; and you will be sure of being amply rewarded by the enjoyment hereafter of professional success and the respect of the world at large.

Gentlemen! I offer you this advice not because you are medical students, but because you are young men. I do not believe that you are more in want of such admonition than others of the same age. I know not what medical students may be in other schools; but, as to those belonging to the school with which I was formerly so long and so intimately connected, I feel it but just that I should express my sincere conviction that there was always as large a proportion of young men diligent in the pursuit of knowledge, and decorous in their general conduct, as is to be met with in

any class of society. Of course there were, and always will be, exceptions to the general rule; and you and I, and all of us, who would maintain the high character of our profession, must be anxious that these exceptions should be as few as possible.

To you, gentlemen, who have been the successful competitors for the prizes given annually by the different teachers of this school, I offer my sincere congratulations. If you have gained honour for yourselves, you have also done good to others; for example is better than precept, and there is no one among you who has not exercised a wholesome influence on his fellow students. Let me advise you to pursue the same course through life, recollecting that even as practitioners you must still be students. Knowledge is endless; and the most experienced person will find that he has still much to learn, many opinions to modify, and errors to correct. You are entering on a profession, which is good or bad according to the manner in which it is pursued. Let me offer you some suggestions as to your conduct in it. On no occasion allow anything to interfere with the strict performance of your professional duties. Whatever you undertake to do, that do to the very best of your ability, sparing neither thought nor trouble, whether it be in the case of the poor man, to whom you give gratuitous assistance, or of the rich man, who remunerates you liberally for your attentions. Consider yourselves as being engaged not in a trade, but in the cultivation of a noble and interesting science. Let it be your first object to deserve and obtain the good opinion of all classes of society with whom you come in contact, not only as being skilful practitioners, but as men of honour and integrity. You will then be in that independent situation which will place you above the caprice of the foolish, and also above the necessity of stooping to obtain the favour of any individual. Do justice to others; but do justice also to your profession and yourselves; always bearing in mind that those who are in any way usefully and honourably employed, have a much higher place in the scale of existence than those useless and selfish persons, who live only for themselves, however high their rank—however large their fortune.

TWO CASES OF PURULENT DEPOSITS IN THE HEAD, REMAINING LONG UNATTENDED WITH BAD EFFECTS.

1. A HUSSAR, a healthy man, 26 years of age, in the beginning of 1840, fell from his horse, and struck his forehead. Some weeks afterwards he engaged himself as a labourer. In 1842 he again entered a

hussar regiment. A slightly-elevated tumefaction remained as the only trace of the accident. His health was good, and no impediment existed to his admission into the army. He performed all his duties for three years, during which time also he received promotion. He was very temperate and orderly. In the spring of 1848 he took part in very severe service: the pressure of his cap irritated the tumor on his forehead. In December 1848 he was suddenly seized with symptoms of cerebral oppression. During the night the tumor had burst, and several spoonfuls of offensive pus had been discharged. The opening was further enlarged, and other treatment adopted, but he died on the 31st December.

On inspection there was found a fistulous opening into the cavity of the cranium, with thickening and discoloration of the dura mater, especially at its attachment to the crista galli. Between the dura mater and the frontal bone were about two teaspoonfuls of fetid pus—the remains of that which had been discharged a few days before.

This case has a medico-legal interest, inasmuch as had his death followed a blow inflicted in a quarrel, his antagonist might have borne the charge of homicide.

2. A cuirassier, 21 years of age, who had been nine months in the service, and was of a good constitution, was admitted into the hospital on the 19th May. He was suffering from catarrhal fever. He did not complain of headache, but of a peculiar confused feeling in the head. The patient ate and drank, and was up and about the greater part of the day.

He had, like the preceding patient, been exposed to hard service with his company. Exposure to cold was the cause of his illness. The prognosis was good—the treatment antiphlogistic.

On the 23d May he was considered convalescent.

On the 24th he suffered from severe relapse, with symptoms of congestion of the brain. These symptoms all became worse, and he died on the night of the 25th.

On inquiring into his history it was found that some time previously he had received a blow on his head; that, four months before, he had also had syphilis; and in his youth had always been rather deaf in the right ear.

On opening the head, about two ounces of thin pus were found in the posterior fossa of the base of the cranium on the right side, lying between the dura mater and the fibrous portion of the temporal bone. Deposits of pus were found in the inner ear, and the mucous membrane of the cavity of the tympanum was destroyed. The arachnoid membrane was unusually vascular.—*Casper's Wochenchrift*.

Original Communications.

REMARKS ON THE
VITAL STATISTICS OF THE
BOROUGH OF PLYMOUTH,
FOR THE YEAR ENDED ON THE 30TH OF
JUNE, 1850.

By DR. W. HAMILTON.

[Continued from p. 573.]

IN 1846, after an interval of fourteen years mercifully granted for preparation, as we learn from the valuable report on the sanitary condition of the town, drawn up by the Rev. W. J. Odgers with that ability, philanthropy, and zeal, for which he is so pre-eminently distinguished, out of 4,045 houses at which he made personal inquiries no fewer than 1,763, or 43·58 per cent of the entire number, were either not at all drained or but imperfectly so, as is seen by the following table:—

Districts.	Undrained.	Badly drained.	Total.
Saint Andrew	512	589	1101
Charles . .	444	218	662
Total . .	956	807	1763

I have no means of ascertaining the amount of improvement effected in this respect during the last four years without a personal inquiry at each house, to which my physical strength is unequal. But although I believe it to have been considerable, much remains yet to be done; and there is but too much justice in the remarks made by Dr. E. T. Roe in a communication to the "Medical Times" of the 3rd of August, 1850, where he says, at page 117, that—

"When the disease died away, the evidence of a desire to ameliorate the condition of the poor died too; and they, neglected and forgotten, have, of course, resumed their former habits, and returned to their accustomed haunts. This town is an example of the truth of my statement; nothing was done until cholera was at our very doors;

then more was attempted than could be completed; and now the poor are rapidly sinking back into pollutions as bad and as numerous as they were twelve months ago. In saying this, I make no charge against our local authorities; they, probably, in common with many others, thought cholera might not visit us; and if it did not, expense would be unnecessarily incurred, and alarm causelessly excited, by any sanitary precautions being commenced in the time of panic; but now no such reasoning avails. The cholera came and found us unprepared: it worked out its fatal problem with unerring accuracy: the abodes of misery it desolated, and scattered their plague-struck population to carry the devastating pestilence far and wide. Should it again approach, cesspools, unventilated houses, a scanty supply of water, and undrained streets, are the internal foes that will, if allowed to continue, open our gates to the enemy. The necessary measures may be costly; but money spent in improving the physical condition of the poor is wise economy; by making their homes more healthful, we cultivate and raise their moral standard, and check the expenses of criminal justice; for it has been amply shown that the great centres of disease are at the same time the nurseries and fortresses of crime: we check the expenses of pauper relief; for a short-lived population is generally a surplus population, not only because those who are reckless of life will be careless of all its obligations, and will be poor and vicious, but because the tendency of early deaths is chiefly to shorten the existence of those who produce more than they consume, and to increase the number of those who must be dependent upon the charity of others."

It is, perhaps, known to but few that the last visitation of cholera cost this town, independently of the loss of life and injury to trade, which were its unavoidable results, upwards of £4000 in money, which, had it been gradually expended during the eighteen previous years in sanitary improvements, and in endeavours to obtain a repeal of the window tax, to which is attributable much of the insalubrity of our houses, the burthen would have been less felt by the inhabitants. Very few would have taken plan and perhaps, to

the case with the hurried and often reckless expenditure which has taken place. The following is the cholera balance sheet, extracted from the annual account presented by the Guardians to the rate-payers at the election which took place on the 14th of May, 1850:—

BOARD OF HEALTH.					
An Account incurred under the Board's Directions for Cholera.					
Dr.	£.	s.	d.	CR.	£. s. d.
For building hospitals, furniture, &c. . .	461	16	5	By paid as per Treasurer's account . .	2431 14 3½
Medical attendance, cab-hire, &c. . .	1566	18	3	Balance due to medical officers and tradesmen . . .	1984 5 3½
Drugs supplied . .	553	19	1		
Butchers, bakers, grocers, and other tradesmen, for various supplies . .	864	18	4½		
Coffins, burial fees, attendance, &c. . .	577	1	2½		
Nurses, costmen, labourers, &c. . .	391	6	3		
	4415	19	7		4415 19 7

Dr. Snow regards impurities contained in the water employed for culinary and drinking purposes among the most active means of diffusing the poison of cholera; and such, no doubt, has been the case in London and a multitude of other localities; but such a cause must have operated very partially, if at all, in Plymouth, where the great bulk of the inhabitants have their supply of water of the purest description from the town leet, which has its source at a distance of some eighteen or twenty miles in the more elevated regions of Dartmoor, whence it was brought originally by Sir Francis Drake above two centuries and a half since, and which the municipal authorities preserve most carefully from contamination: such is the purity of this water, that a quart yields on evaporation less than two grains of residuum, chiefly consisting of hydrochlorate of soda derived from the marine spray driven by the south-western winds into the interior from the basin of the Sound and the English Channel.

Until the decennial enumeration which is to take place at the end of next March shall have been completed, I am without any means of ascertaining the actual number of houses existing in the borough either at the present moment, or in 1846, when Mr. Odgers collected the materials for his important report. Judging, however, from the multitude of new

streets that are daily starting into existence in every possible direction, we might be led, not unreasonably, to consider the town as more than doubled in size since the enumeration of 1841; and it is far from improbable that such a conjecture would not err greatly from the truth. But to assume a less questionable basis for our calculation, let us compare the amount of population returned in 1841 with the additions made to it from natural causes, irrespective of the disturbing influences of immigration and emigration, of which as yet we have no means of judging, in the intervening time, and endeavour from thence to form an unexaggerated estimate of the number of houses at present in actual existence, or in progress. The population on the last enumeration was 38,527; and, according to an estimate based on the excess of births over deaths during the nine years which ended on the 30th of June last, the population had increased to 39,266 on the 1st of July, or nearly 7·5 per cent. in that time, notwithstanding the supernumerary mortality of 1849 from cholera.

In 1841 the number of houses returned was 4,298 inhabited; 228, or 5·3 per cent., void; and 38 building; making an aggregate of 4,526 habitable houses, and 38 progressing to completion. If we assume 7·5 per cent. a rate closely approximating the increase of population arising from births, as that of the

multiplication of buildings to accommodate such an augmentation of inhabitants, we shall have 5000 for the probable number of houses inhabited at present,—a number which we may well conceive to fall short of the truth by at least 2,000 or more. Of the number of houses visited by Mr. Odgers in 1848, when the estimated population was 88,405, or nearly 2·4 per cent. less than at present, 753, or about 18 per cent., were unsupplied with town or leet water; but of these a large proportion was provided with rain water or water from wells or pumps.

Now, with the exception of the well or pump water, the supply to more than nine-tenths of the inhabitants consists of the very purest quality, and placed beyond the reach of such contaminations as Dr. Snow suggests; while, from the moderation of the annual charge, graduated according to the rental of the house, the poorest inhabitants are enabled to participate in the supply: so that, with the exception of the minute fraction dependent upon wells and pumps liable to contamination from the infiltration of sewer and cesspools, the inhabitants of Plymouth enjoy a larger amount of protection from this fertile source of disease than those of most other towns throughout Great Britain.

It must be admitted, indeed, that, although not originating in them, cholera, after it had established itself, raged with its most fatal fury in those localities where the supply of water was most scanty, and the sewerage most imperfect: but this by no means establishes the fact of its having been produced by those causes: it merely shows that the soil was prepared for the reception of the seed, which, once planted, sprouted forth with more than its ordinary rapidity and vigour, as Dr. Roe very justly observes in the paper already quoted:—

“When the air of a town is infected with cholera poison, local causes, without contact, may give rise to isolated cases of the disease, and that, when imported into a town the sanitary condition of which is healthy, it will find nothing to feed upon, as has been the case at Exeter and elsewhere; but if, on the other hand, it meets with a hotbed of filth, misery, starvation, and disease, then such scenes occur as have twice within the last eighteen years been enacted in Plymouth, and death mows

down those whom a defective local legislation had allowed to rot while living.”

Such “a hotbed of filth, misery, starvation, and disease,” awaited the reception of cholera in Stonehouse and the adjoining lanes, in which, prior to 1849, there was a total absence of drainage; and, in place of feeling astonishment at the severity of the outbreak in that and similar quarters, the only subject for surprise is, that it did not commence there, and radiate from thence to other parts of the town. Bath Quarry and Stonehouse Lanes are situated at the base of the elevation upon which Eldad stands, and thus receive the full benefit of the impure filtration which percolates the soil above from the two cesspits into which the whole of Eldad, as Mr. Odgers informs us, was drained in 1840; and from that time, I believe, up to the moment of the invasion of cholera in 1849.

Quarry Lane, as we learn from Mr. Odger's report, contained at the time of his inquiries ten houses occupied by 135 individuals, or 13·5 to each house; and for the supply of these people there was then but one water-cock, and one pump. Stonehouse Lane, with 69 houses and 825 inhabitants, or 12·3 to each house, had the leet water supplied to but ten of the houses, or about 18 per cent. of the whole, the remaining houses being provided with this necessary of life by hand carriage from the leet which passes by “No Place Inn” for the service of the Royal Naval Hospital, at a considerable distance, and on the summit of the ridge; while Stoke Lane, with 21 houses and 154 inhabitants, or 7·3 to each, is wholly dependent on pumps for its supply, not one house having the benefit of the town water, and only eight of the houses, or 18 per cent. of the number, being provided with pumps, the water of which is much contaminated by infiltration. And in these three localities so peculiarly adapted to the spread of mortality, we find 77 deaths, or above one-eleventh of the whole mortality from cholera, took place. But if such was the amount of the mortality in this ill-drained, ill-watered, and most impure part of the town, ought we not to expect a proportionate amount in other places similarly situated? To answer this question, let us look to the mortality and condition of Market Alley.

Market Alley is a close, narrow, ill-ventilated passage, leading by an archway under one of the houses in Bedford Street across into East Street, from which a doorway gives an entrance into the south side of the market-place: so that at either extremity it is out off from free ventilation. The houses which form this alley are lofty, and inhabited by the poorer classes, of whom, as the report already quoted acquaints, 207, or 20·7 to each, occupy the ten houses which form it: of these ten houses, but three, or less than one-third, are provided with town water; and not one of them has a cistern or tank to retain the water for the alternate days on which there is no supply. Yet, notwithstanding this density of population, this imperfect ventilation, and this defective supply of water, we find the mortality of this unhealthy passage confined to a solitary death from cholera on the 29th of September.

In Union Street, that great artery of communication between Plymouth, Stonehouse, and Devonport, which, though little elevated above high-water mark, is one of the widest, best built, and best ventilated parts of the town, where all the houses are occupied by persons in comfortable circumstances, and well supplied with the town water; where no assignable cause for epidemic diseases presents itself to the eye; and where no impurity from infiltration can have tended to introduce the malady,—here the earliest case of cholera appeared, and here the two first deaths occurred four days before the eruption in Stonehouse Lane and its pestilential vicinity. Had the disease originated among ourselves from the causes to which it was commonly ascribed elsewhere, we should naturally have looked for it in that concentration of filth and poverty, rather than in Union Street, where the first death took place on the 4th, and the second on the 7th of July; and two which remained were on the 8th of July and 19th of September. The two first of the thirty-seven deaths which took place in Quarry Lane did not happen before the 8th of July; and the five last on the 18th, after which all cases were taken to the hospital as they arose. In Stonehouse Lane the first three out of the forty fatal cases there were on the 9th of July, five days after its appearance in Union Street, two occurred on

the day following; and from one to two deaths daily for the next nine, after which solitary deaths took place on the 24th and 27th, on the 1st, 2nd, 7th, 14th, 16th, 17th, and 21st of August; two on the 18th; and the four last on the 7th, 8th, 10th, and 21st of September.

But in Higher Street the outbreak was almost wholly condensed into the third week of the third month of the pestilence, only two deaths having taken place during the whole of July and August,—namely, on the 25th of the former, and 31st of the latter; and three deaths out of the forty-two in October, the great bulk of the mortality taking place between the 13th and the 24th, in which the deaths amounted to thirty-five, of which number as many as nine were on the 16th, six the day following, and eight on the 18th.

Higher Street occupies a position on the eastern side of the town analogous to that of Quarry and Stonehouse Lanes on the western, being at the base of the hill crowned by Gascoyne Terrace, with which it is connected by the steep ascent of Gasking Street. This street, which in 1846 contained 508 inhabitants, is classed by Mr. Odgers among those which are drained (though, he adds in a parenthesis, imperfectly), in which respect it is far better off than Stonehouse Lane, in which, prior to the late visitation of cholera, not a trace of drainage was to be found. But here it must be admitted that the existence of sewerage in the one, and its total absence in the other case, throws a shade of suspicion over the opinions of those who ascribe the mortality of cholera exclusively to defective sewerage.

The formidable outbreak of cholera in Higher Street on 14th of Sept. has been traced to a woman who occupied a room by herself at the top of one of the most densely tenanted houses in the street, containing, if I recollect aright, somewhere about 60 individuals, steeped for the most part in the deepest poverty and wretchedness. This lonely and unfortunate individual having been attacked with cholera in the silent darkness of the night, perished without help in the solitude of her room; and it was not for a considerable time after her decease that she was missed by her neighbours, who went to her door to learn the cause of her non-appearance.

After knocking and calling without obtaining a reply, they broke open the door, and found, as they suspected, that she was not only dead, but considerably advanced in decomposition. The scent, which diffused itself through the whole house immediately after the door was burst open, I understood to have been intolerable, and to have resembled that of which the unfortunate Mrs. Fortescue complained on meeting the funeral procession in Bilbury Street; and there was an equal correspondence in the results: cholera almost instantly made its dreaded appearance, and the mortality was only exceeded by the panic. Among other inmates of this ill-fated dwelling was one who kept a dairy in one of the lower apartments. To the milk in this apartment, if report speak true, the cholera communicated a portion of its destructive poison, and thus extended its ravages to other and more distant localities, where this contaminated milk was consumed. How far this statement is fact, or how far it is the creature of imagination, I have not been able to ascertain: and as the inquiry is, as I have been informed, in the hands of one better qualified for the task than myself, I willingly resign it to him, in the hope that he will not withhold the result from the world.

The origin of cholera in the malignant form in which it has depopulated the world since the latter portion of the eighteenth century is buried in the profoundest depths of obscurity, since I have not been able to find any authentic or distinct account of it farther back than the year 1781, when it made its appearance among the British troops, 5000 in number, stationed at Gangam, about lat. $19^{\circ} 20'$ N., and long. $84^{\circ} 40'$ E., on the coast of the Circars, 535 N.E. of Madras, where, in the brief space of three days, it consigned to the hospital or the tomb above half the number. In the following year it extended to Madras, although in a somewhat mitigated form; and after a delay of twelve months more reached Hurdwar, near lat. 30° and E. long. $70^{\circ} 20'$, where, besides invading both the civil and military inhabitants, it swept off no fewer than 20,000 of the pilgrims who frequented that holy place. After this we lose sight of it till 1817, a period of four-and-thirty years, during which we can hardly suppose it to have been dormant or extinct, although its ravages might

have been so far confined within the bounds of moderation as to attract little attention.

In the "Wanderings of a Pilgrim" already noticed, the talented authoress gives us the following account of this frightful scourge, obtained from conversations with her physician:—

"Our physician has just quitted us: we had a conversation on this dreadful malady that has wrought us so much misery; he, says—'Cholera is an endemic of Ceylon; from the year 1813 to 1817 I never met with it in India. In 1817 it burst out in a mad-house of which I had the charge, and the patients confined there died daily in the course of a few hours after the first seizure. The horror produced amongst the unfortunate insane was so great, that many became perfectly sane. A man who had attempted to destroy himself in a fit of phrensy, by cutting his own throat and stabbing himself, became perfectly sane, and, coming to me, intreated to be allowed to leave the mad-house, as he was sure he should die of the cholera. It was utterly impossible his request could be granted at the moment: in the course of the week he fell a victim to the malady.'

"Our medical man added, 'The causes of cholera are unknown. I know that the disease is often confined to a space of two or three hundred yards; for instant deaths occurred daily in the mad-house. In the jail, which contained five hundred prisoners, and was not three hundred yards distant, not a case appeared. I was so well convinced of the disease being confined to a certain spot, that I applied for leave to remove all the insane to a spare room in the jail: from the time of their entering the jail, not one man died of cholera. The environs, as well as the interior of the mad-house, were quite clear and pure; *no stagnant water*; nothing that could generate disease in any shape.

"I mentioned that it was supposed our beloved one had been exposed to the baneful influence of cholera in passing through Arcott, where it was known to be. 'Certainly not: *it would not lurk about a person twenty-four hours*. Until the moment she was seized, she was in perfect health. I once saw a man, previously in good health, seized with cholera: he was *sitting in a chair, talking to me*; he dropped; his nervous power was quite prostrated: he was perfectly sen-

sible the whole time, and died in a few hours.' I asked if his sufferings were not very great? The physician replied, 'I should think not; from the extreme want of life in the body. The effect of the illness is such, that the vital spark is almost extinguished while still the body breathes.'

"He said, 'You should not grieve at the speedy termination of her illness; from severe cholera it is hardly possible to recover. Those who do recover, generally linger on for twelve or fourteen days, and then expire in a melancholy state: it is better it should terminate at once.'

"It came from Ceylon, and broke out with dreadful severity in 1817, especially in what we call Bengal, which is one hundred miles around the presidency; since which it has raged partially in Oalcutta and all the lower provinces; also in the higher; but in the central provinces, in which Allahabad is situated, it is very rare, only one case of cholera having occurred during the last two years at this station. At Berhampoor it is dreadfully prevalent: the 48th regiment quitted this a short time ago by the river, and lost nine men at that place. No diet, no care, can avail. Our medical man said, 'I can compare it to nothing but a flash of lightning: its effects are instantaneous; the nerves from the first moment are powerless, dull, and torpid.'

"If I were to be seized with it to-morrow, I should only strive to resign myself quietly to my fate, feeling that to strive against the malady is hopeless: in fever you have hope; in cholera scarcely a shadow of it: it is better not indulged; but the disease is so powerful, it dulls the senses—mercifully dulls them" (pp. 202 et seq., vol. i.)

Great as the destruction of human life by cholera, the amount sinks into insignificance on a comparison with the wholesale slaughter of which we have accounts from other places, in Asia more especially.

It was not till 1817, as is stated in the passage just quoted, that cholera developed itself fully in the character of a wide-spreading and irresistible epidemic, mowing down nearly the entire population of some localities, setting at nought all the puny efforts of man, and carrying dismay into the boldest hearts.

Finding in this year an appropriate soil in the marshy and pestilential

swamps which, under the name of Sunderbunds, constitute the feverish Delta of the Ganges, it made its onslaught on the town of Jessore and its sixty thousand inhabitants, one-tenth of whom were, in the space of a very few weeks, erased from the books of the living. In less than a month it reached Calcutta, at a distance of one hundred miles southwest of Jessore, where two hundred daily victims were insufficient to sate its appetite for life.

[To be continued.]

POISONING BY THE SEEDS OF *JATROPHA CURCAS*. BY R. J. FARQUHARSON, M.D., ASSISTANT-SURGEON U. S. N.

Two of our men, being ashore at Porto Praya, Cape de Verdes, tasted the seeds of the *Jatropha curcas* (which grows in great abundance on these islands), and finding them pleasant, ate of them, one to the extent of a handful, the other being satisfied with three or four. In both cases vomiting and purging of a violent character came on in the course of an hour; and in the instance of the man who had eaten but a small quantity, the effect only extended thus far. In the other case more alarming symptoms rapidly supervened. The muscles of the extremities were contracted by violent spasms; the patient was affected with dizziness and vertigo, accompanied by great restlessness; the respiration was quick and panting; the skin became cold and moist, and the pulse small, thready, and intermittent; the heart's action was very irregular, and so weak that the impulse against the walls of the chest could with great difficulty be perceived. These effects of the poison upon the nervous system continued for the space of several hours. The seeds eaten were ripe, and of the kind used in small quantities by the inhabitants as an active purgative.

The treatment consisted in the free use of anodynes and stimulants, after the continuance of vomiting and purging for a time rendered it probable that all the offensive matter had been discharged from the intestinal canal; together with the application of a large mustard poultice during the state of depression. About five hours after the commencement of the attack, reaction occurred; and shortly afterwards the patient fell asleep, and waked the next morning with no other ill effects remaining than a slight irritability of the stomach and considerable debility.—*American Journal of Medical Sciences* for July, 1850.

OR
SOME OF THE MORE PRACTICAL
POINTS CONNECTED WITH
THE TREATMENT OF
DEFORMITIES.

BY EDWARD F. LONSDALE,
Assistant-Surgeon to the Royal Orthopaedic
Hospital.

[Continued from p. 357.]

*On the Nature and Causes of Deformities
of the Hip-joint, considered generally.*

I SHALL next proceed to consider the deformities that are met with about the hip-joint; and they admit of being divided into three classes—viz., where they arise from displacement of the head of the femur from the acetabulum, with little or no contraction; those where there is contraction only, without displacement; and, finally, where there is both displacement and contraction. The latter is by far the most common. The causes may also vary, of which four principal ones may be mentioned—1st. Inflammatory diseases, such as simple acute inflammation, rheumatic inflammation, and scrofulous inflammation, the last being the most frequent. 2d. Spasmodic or gradual contraction of the muscles, and more particularly of the flexors of the hip, depending either upon disease of the spine, upon rheumatism, or upon hysteria. 3d. Injuries in the neighbourhood of the joint, or to the joint itself, may cause permanent lameness, and alteration in the natural relative position of the femur with the acetabulum. 4th. Congenital deformities of the joint may exist, where the femur is dislocated either on one or both sides. Another cause might perhaps be mentioned—viz., a rachitic condition of the bones generally, where the pelvis becomes distorted, and the shape and relative position of the neck of the femur and trochanters also are altered to an extent to produce external deformity with regard to the shape of the joint, though the interior may be perfect and natural in structure. It will be best, then, to consider the subject as arranged in the following table:—

Varieties.	Causes.
Displacement of the bone without contraction.	Disease of the joint, with more or less destruction of the bones. Mechanical violence. Congenital malformation of the joint.
Displacement of the bone with contraction.	Disease of the joint, with more or less destruction of the bones. Mechanical violence.
Contraction of the joint without displacement.	Spasm (temporary or permanent) from rheumatic and other inflammations. Disease and irritation of the spine and spinal marrow. Hysteria.
Deformity in external shape only.	Rickets. Mechanical violence.

Displacement of the Bone without Contraction of the Joint.

The head of the femur may be displaced directly upwards, and rest upon the ilium immediately above and very slightly behind the margin of the acetabulum, causing a shortening of the whole limb, without any other alteration of its relative position. This condition of the joint is evidenced by the measurement of the limb, as well as by feeling the trochanter—and, in many cases, the head of the femur—in a position they ought not naturally to occupy. There may be no contraction of the joint forwards in a state of flexion, nor complete impediment to motion; for there may still be the power of bringing the knee backwards and forwards to a certain extent. The toe is pointed downwards, though the patient can, in many cases, bring the foot nearly flat upon the ground (of course by bending the body to this side), which seldom can be done when the displacement is attended with contraction of the hip as well; for the knee not being in a straight line with the body, the only part of the foot that touches the ground is the toes, and this also at the expense of the perpendicular bearing of the body, which has to be thrown over to this side.

In the cases of disease of the joint, where this displacement upwards of the femur exists without contraction forwards, I believe it will generally be found that the destruction of the capsule of the joint has taken place with-

out much pain or constitutional disturbance to the patient, and in most cases very rapidly; so that no lengthened confinement to bed (if any) has been required, and, from the absence of pain, the joint has not been placed in the flexed position, to take off the strain or pressure on the anterior part of the joint which no doubt exists in the acute cases where the inflammation is very active. The head of the femur, as well as the margin of the acetabulum, may also become more rapidly absorbed in some cases than in others, and so allow of the more easy displacement of the bone upwards than when the head of the bone is dislocated without being much altered in form, which then causes it to be thrown more backwards, and the knee forwards, and so cause contraction of the muscles in this direction. These cases, however, are of the rarest kind, the majority being accompanied with great pain and contraction of the hip forwards, and requiring the patient to remain in bed for a longer or shorter period, according to the severity of the symptoms.

Where mechanical violence is the cause of the displacement, as in some severe kinds of injuries, one of two things must occur: either the bone must be dislocated, or the neck of the femur must be fractured, or the greater trochanter be split very high up. In the latter cases—viz., fractures—it is not at all uncommon to find the limb much shortened from the femur being drawn upwards, without any contraction forwards—in fact, in by far the majority of cases of fractures through the neck of the bone this is the position the limb usually takes. In the former, however, viz., in dislocation of the femur upwards—it is unusual to find the limb to keep its straight position, it generally being flexed and inclined to the opposite (I am of course only speaking of dislocations upwards or backwards, or both). The cases in which it does occur are very rare, and are always, so far as my own experience has gone, in children about or under the age of puberty. The reason of its being rare is, that dislocation of the femur at this age is extremely rare; force, if applied to the bone, generally producing fracture in some part of its length, and so arresting the violence before it can tell upon the joint. When it does occur,

the absence of any contraction in the direction of flexion is to be explained by the smallness of the head of the femur at this age, as well as by the flatness of the ilium and margin of the acetabulum, which allows of the bone resting directly upwards instead of being tilted upwards and then backwards. There is generally less impediment to the motion of the bone in these cases than might be expected, for there still remains the power of flexing the thigh forwards; and, after a time, the patient can bear his weight upon the limb—of course walking lamely, from the shortening there exists.

I once saw a case where there was shortening of the limb without contraction in the direction of flexion, in the adult, after an injury, which is interesting on account of its extreme rarity. I have described it elsewhere.*

A man received a severe blow from a fall, which told in a direction on the femur to produce dislocation of the head of the bone. The patient was admitted into the Middlesex Hospital, with some though not all the symptoms of dislocation upwards in the direction of the ilium. The limb was shortened, and the trochanter was by measurement at a higher level than natural, and the head of the bone could also be felt, which of course at once indicated displacement. The position of the knee and foot, however, were natural, being neither inverted nor everted, combined with which there was power to flex the thigh forwards almost to the natural extent. The limb could not be elongated by extension, nor any crepitus be produced. The man died of other injuries received at the same time, one of which was fracture through the sacrum. On making the post-mortem, there was found to be fracture of the upper and posterior margin of the acetabulum, which was levelled off to the ilium; and upon this fractured surface the head of the bone rested.†

There are other cases also of displacement of the head of the femur by violence, as in fractures directly through the acetabulum, where it is driven into the cavity of the pelvis. These are rare.

* Treatise on Fractures, p. 267.

† The preparation is in the Middlesex Hospital Museum.

Of Congenital Displacement of the Head of the Femur.

This is the most complete form of displacement without contraction of the hip-joint that exists. It consists in a complete dislocation of the head of the bone upwards on to the dorsum of the ilium, the patient having the power of bending the joint, if so it may be called, at pleasure, being able to bear the weight upon it, and, if of an age to do so, to walk also. There is of course lameness and awkwardness in his gait, consisting of a hobbling, "waddling" motion, from side to side. The peculiarity of these cases is, the loose way in which the head of the femur appears to be connected with the pelvis, and that the patient should still have so much power over the limb, and be able to bear the weight of his body upon it. It will be found on examination that the bone can be easily drawn down, till the head of it rests in the place where the acetabulum ought to be, and then is either drawn back again to its abnormal position by the action of the muscles, or can be easily pushed there with but a very slight force.

I have not had an opportunity of dissecting a case of this nature to ascertain with certainty the precise condition of the joint. All the symptoms, however, lead to the supposition, that from some cause in utero the ligaments proper to connect the head of the bone with the pelvis are never formed, or else that they are so long as to allow of the degree of displacement that is found to exist. The bones themselves may also take some share in the deformity, either by the head of the femur not being properly developed, or by the absence of the acetabulum. The acetabulum at the time of birth, however, is always very shallow, and not sufficient to prevent displacement more frequently than is found to occur were it a cause of the displacement. Dupuytren mentions having had the opportunity of dissecting but few cases.* The appearances observed in one I shall here give; they will be found to explain the cause of the difficulty, if not impossibility, of doing much in the way of treatment in these cases:—The subject was a man of 75 years of age, who died of the effects of a long-standing stricture of the urethra, having been

born with congenital dislocation of both thigh-bones. The following is the account of the condition of the ligaments and bones which naturally form the hip-joint:—"On the left side the old cavity (the acetabulum) was not more than an inch wide in its greatest diameter; it was very shallow, rough, and filled with a fatty substance—yellow, of the consistence of oil, and almost of an oval form. The external iliac fossa had in front of the sciatic notch a wide shallow depression, covered or lined with a thick periosteum, having almost the appearance of an articular cartilage: this place was contiguous to the head of the femur. The head was diminished in volume, a little flattened, unequal, without any trace of the insertion of the internal ligament; was covered with an articular cartilage, but thinner than in the natural state. The articular fibrous capsule formed a true sac, the points of insertion of which were the superior and inferior edges of the old cavity. This sac filled the place of a bony cavity on this side, and by its length allowed the ascent of the head of the femur into the depression just spoken of, the space through which it could move being about three inches. The thickness of this sac was very considerable, its density almost cartilaginous.

"On the right side the old cavity was a little larger: the interior had the same appearance as the other. The external iliac fossa, instead of offering, as on the opposite side, a simple depression, presented in front of the great sciatic foramen, towards the edge of the space comprised between the anterior inferior and anterior superior iliac spines, a wide and deep cavity with bony margin, rough, and unequal. The head, more voluminous than that of the opposite side, had kept its form better; it was, like the other, covered with an imperfect articular cartilage, and the interior of these articulations was lined with a synovial membrane. The capsular ligament was not so thick as on the left side, although its extent was not confined to the margin of the abnormal cavity. But on this side the head of the femur lodged against the bony edge, found there a solid "point d'appui;" while on the left the great strength of the fibrous sac supported alone the ascent of the limb by its resistance to the weight of the body."

* *Lçons Orales*, t. III. p. 205.

I have quoted thus much of Dupuytren's paper, as the points detailed will explain the cause of the external appearance met with in these cases, as well as the awkwardness in the gait, and the difficulty in supporting the weight of the body. There is a peculiarity in these cases when the patients arrive at an age to walk and run—viz., that they do the latter with less difficulty than the former; they are able to run without the same appearance of awkwardness and lameness, the explanation of which, as given by Dupuytren, is no doubt the correct one. He says: "It might seem singular at first sight that running and jumping are performed with less difficulty than merely walking. It is nevertheless so: in this kind of locomotion the energy of the muscular contraction, and the rapidity of transport of the weight of the body from one member to the other, renders almost insensible the effects of the want of an acetabulum, and of the fixedness of the head of the femur.*

"The immediate cause of the displacement of the head of the femur in these cases of congenital dislocation is not so easily explained. The following questions have been asked:—Has there been any disease in the fœtus while in the mother's womb, and cured before birth? Could it have been the result of an effort, or of violence, which made the head of the femur move from the acetabulum, and this latter part have been obliterated without disease, or only because it remained without employment, and consequently useless? Could nature have forgotten to shape a cavity for the head of the femur or this cavity, which results by the joining and union of the three pieces of which the ilia are composed? Could it be imperfect, owing to any obstacle in the formation of the bone, as supposed by M. Breschet?"†

My own impression is that there is no occasion to find causes to explain the dislocation of the head of the bone in these cases, if the fact be that there has been none, which I am inclined to think the case; believing that the hip-joint has never been perfected, and consequently that the head of the femur has not been in contact with the acetabulum from the very commencement of the development of the fœtus. I look

upon it as an original arrest of development in the hip-joint, the ligaments never having been properly joined to keep the bone in the joint, or else that the bone itself is never, from some cause or other, placed in its proper position to be opposite to the acetabulum, and that the ligaments consequently accommodate themselves to this altered position, and never gain the power of fixing the bone in its natural place: were it owing to any position or movement of the child "in utero" we no doubt should meet with it more frequently. The following circumstance also favours the supposition of some original defect in the development of the joint being the cause of the want of the proper apposition in the bones—viz., that it runs in families:—Dupuytren relates the following case of the relations of a female whom he had the opportunity of seeing:—"Two of her aunts, on the maternal side, who died at 70 years, had been affected with lameness from earliest life. They said besides that they had always limped. They had high and large hips, very projecting, and they walked with their elbows out behind, hobbling like ducks. The father of Margaret had a sister deformed from birth on the right side, who died at the age of 80 years. Another sister, well formed, gave birth to a girl who had a shortening of the right limb."

The peculiar feature in these cases of congenital dislocation is the facility with which the limb can be elongated by placing the patient on his back, and then making extension upon it, or can be moved upwards and downwards without any apparent resistance being offered: the muscles draw the bone upwards again when the extension is discontinued, though they oppose little or no resistance to its being brought downwards. In many cases, as might be expected, when the patient lies down, the bone approaches nearer to the acetabulum without any extension being made, owing merely to the weight of the body being removed, which pushed the pelvis past the head of the bone when in the erect position.

The second variety of deformity of the hip-joint—Displacement of the bone with contraction.

This is by far the most common deformity met with about the hip, and in fact ranks amongst the most frequent

* *Leçons Orales*, t. iii. p. 222.

† *Loc. cit.*

of any kind of deformity met with in this or any other part of the body generally. So common is it, that not only do those connected with institutions appropriated to these cases see great numbers of them, but general hospitals are seldom without them. They are, unfortunately, at the same time one of the most unsatisfactory class of cases in their results.

The displacement of the thigh-bone in these cases of which I am now speaking is always consecutive, following some local inflammatory or general constitutional disease, the progress of which, if not arrested, terminates in the destruction of the ligaments of the joint, and often of the substance of the bones themselves.

It is not my intention to enter at any length into the description of the nature and symptoms of disease of the hip, but to confine myself to the after effects, which are generally those I am now speaking of—viz. contraction of the hip forwards, in a state of flexion varying from the slightest to the extreme degree, when the knee is brought to a right angle with the body,—adduction of the limb with rotation inwards causing the knee to overhang the opposite thigh. There is very often rotation outwards when the disease has been extensive, and when the head of the bone much destroyed, or when a false acetabulum of some depth has been formed. Ankylosis is also not unfrequently met with, when the head of the femur becomes firmly united to the ilium.

The cause of the displacement of the bone in these cases of inflammatory or strumous diseases of the hip-joint is easily explained by the action of the many powerful muscles inserted into the different parts of the femur, the tendency of some of which will be to draw the thigh-bone upwards so soon as the disease has destroyed the ligaments, and thereby removed all opposition to their action. The position of flexion is explained by the powerful muscles inserted into the trochanter minor: the patient drawing the limb upwards to relieve himself of a degree of pain by relaxing the anterior portion of the capsular ligament, the limb becomes placed in this position at an early stage of the disease, some time before the dislocation is produced by the other muscles drawing the bone upwards. The amount of displacement backwards and

upwards varies, and depends in a great extent upon the amount of destruction done to the ligaments and to the bones, as well as to the muscles. There is generally more displacement upwards than backwards in these cases of consecutive dislocation from disease: it is rare to find the head of the femur in the ischiatic notch. The shortening of the whole limb, of course, depends upon the degree of retraction and flexion; and upon the latter also depends the extent to which the toe is pointed; for when the knee can be brought nearly, if not quite, in a straight position with the body, the foot can be generally placed flat upon any artificial elevation to compensate for the shortening,—such as a high boot or other substance: but when the knee hangs forward, this cannot be done.

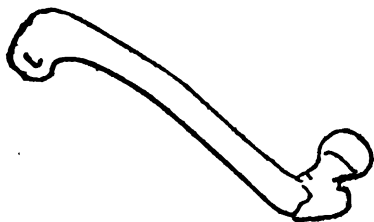
Displacement of the Thigh-bone with Contraction forwards, from Mechanical Violence.

There are only two classes of cases to be considered under this head—viz., dislocations and fractures—in neither of which can much be done without the deformity be removed at an early period after the injury; in the former case, before the bone has formed a new socket for itself, and has accommodated itself to its new position; in the latter, before the fractured surfaces have united so as to lock the portions of bone permanently in their new position if the fracture be of a kind to admit of bony union taking place.

The deformity from dislocation upwards and backwards is such as might be expected when the position of the bone is considered: the whole limb is shortened; the leg is turned inwards; and the knee inclined over the opposite thigh; the trochanter and head of the bone are felt in the position into which they have been forced by the violence that produced the injury, which is generally more posterior and at a lower level than in the dislocation consecutive to disease of the joint. The two principal deformities produced by fractures are, the one when the neck of the bone is split through *within* the capsule, when, I believe, union never takes place;*

* None of the cases that I have myself seen or heard of fracture of the cervix femoris completely *within* the capsule, the capsule itself being said to be uninjured, have the supposed bony unions been satisfactory to me that such was the case. I have discussed this subject fully in my *Treatise on Fractures*. Digitized by Google

the other, when the trochanters are split in one or more directions, producing the wedge-like fracture of the upper end of the thigh-bone. In the former case there is seldom or never contraction in the direction of flexion, the principal deformity being increased width on the injured side, with retraction upwards of the trochanter, and generally strong eversion of the foot; the limb is, of course, shortened. In the latter case, the existence or not of contraction of the joint in a state of flexion depends upon two causes—either the position in which the fractured portions of the bone are locked at the time of the injury, and from which they cannot be or are not afterwards moved, or else by the kind of treatment that is adopted. The point I refer to in the treatment is where these wedge-like fractures are placed on the inclined plane instead of in the long splint. The shaft of bone then follows the knee, and becomes flexed forwards, while the head and neck of the bone and portion of the trochanter connected with them remain in the straight position, not moving with the shaft; union then takes place; and when the patient comes to stand upon the limb, he finds he cannot bring the knee down beyond the angle at which it was placed during the treatment. I have given a woodcut to illustrate this point more clearly, as I am convinced it is overlooked by those who advocate the inclined plane for the treatment of fractures of the femur when situated *above* the trochanter minor.



In these wedge-like fractures through the trochanter there is almost always a mass of thickening felt in the situation of the injury, caused by the portions of bone being so displaced as to greatly increase the width of this part of the bone, and producing a consequent shortening of the limb as well. There is no rule as to the direction in which the foot may turn in these cases. It will depend upon the peculiar direction

in which the shaft of the bone is locked, whether it be in a state of inversion or eversion.

Of Contraction of the Hip-joint without Displacement.

The hip-joint may become contracted from different forms of inflammatory disease without the capsule being destroyed, and where the muscles become so much accommodated to their new position from the length of time in which they have been so placed, that when the inflammation has subsided there is great difficulty, if not impossibility, to overcome the opposition they create to bring the limb into its natural position again. The joint itself may become altered in structure either internally, by deposits, or even by ankylosis occurring, or by extensive thickening of the ligaments and surrounding textures, so as to keep it in the position of flexion into which it has been placed during the existence of the disease. Rheumatic and scrofulous inflammations may produce these alterations in the joint, and leave it in a condition of permanent or temporary contraction according to the severity with which they have attacked the joint. Inflammation and irritation of remote parts may cause contraction of the hip-joint, examples of which are seen in diseases of the bones of the spine, producing either psoas abscess or irritation in the spinal marrow, or both: in the former cases the matter runs a course which locally interferes with the action of the flexor muscles—viz. the psoas and iliac muscles: in the latter the irritation may exist either directly upon some of the nerves supplying these muscles, or indirectly through the spinal marrow as, to produce a degree of contraction in them that may either take the form of mere spasm or assume the condition of permanent contraction.

Obscure causes may exist in the spine, producing paralysis of the muscles of the lower extremities generally, from which the patient partially recovers, and remains in a condition in which the flexors are more contracted than the extensors, producing contraction of the hip-joint at the same time that there may be also an increased degree of adduction of the limb by the adductor muscles being similarly affected to the flexors. An unnatural degree of spasm may also take place without any apparent cause existing, or any previous

paralysis having occurred as just referred to; but the muscles may become so much contracted as to keep the two limbs in a state of permanent flexion and adduction; and most frequently in these cases it will be found that both the legs are affected at the same time, and combined very often with contraction of the muscles of the knees and feet as well, producing partial flexion of the former, and talipes equinus of the latter, or equino varus. The deformities in these cases are dependent upon the muscles and nerves, the joints themselves being perfectly healthy. The primary cause of many of these contractions depending upon the so-called irritation of the nerves is very obscure, there being no symptoms, either local or general, existing in many cases which elucidate the nature of the malady in the smallest degree. All we can do is to combat the effects with as much success as possible, which, after all, amounts only to an *improvement* in the condition of the patient far distant from an approach to a cure.

Another cause is found in the so-called hysterical condition of the nervous system, for the contraction of the hip-joint. It is sometimes met with in young women, when the flexors of the whole extremity become spasmodically affected, in many cases to the greatest possible extent, the thigh being brought upwards against the abdomen, and the leg backwards against the thigh, till the great toe touches the buttock. At other times the knee may be affected only, without the hip, the heel being brought backwards upon the thigh, the thigh itself retaining its natural position. It more frequently happens, however, that all the joints are similarly affected, viz. the hip, knee, ankle, and toes, most of the muscles affected being the flexors.*

The last cause I have to refer to of contraction of the hip-joint without displacement exists in the congenital form of the deformity. I do not remember to have seen more than one case, and this was under the care of my colleague, Mr. Tamplin, a drawing of which he

gives in his work on deformities, with the following description:—"The thighs being flexed upon the pelvis, the knees extended, and the feet thus contracted, were kept constantly lying on the abdomen, chest, and face of the little patient; and when the leg or legs were forcibly drawn down, they returned with an elastic impulse to their contracted position." The cause he gives, no doubt, is the correct one—viz. position "in utero." The child was born with the breech presentation, and had evidently, during its uterine existence, possessed very little, if any, power of moving the lower extremities out of the position in which they presented themselves, and which position would certainly adapt it to the smallest possible space it was capable of being placed in. There was no malformation or any evidence of disease existing or having existed in the nervous system."

The rachitic deformities of the hip-joint are more apparent than real; for the alteration that takes place is in the shape of the bones external to the joint, and not in the joint itself: the hips are widened, and the trochanters thicker and larger than natural, owing to the neck of the thigh-bone being more horizontal, the trochanters themselves being larger and thicker than they ought to be. Distortion of the pelvic bones also will alter the shape and width of the hips without the joints themselves being at all affected internally.

Having made these *general* remarks upon the nature and causes of deformities about the hip-joint, I shall proceed in my next paper to consider their treatment.

[To be continued.]

WHY DOES QUACKERY FLOURISH?

"PERSONS, in this strange world, frequently have more power than either *truth* or *fact*; and consequently, the legitimacy instrumentality of a physician is oftener interrupted by the unwise and unjust intermeddling of others, than by any other agencies." This we conceive to be the kernel of the whole subject. It is not by reason that medicine can be assailed; the raging vice of the times, the itch of people to give opinions upon all possible subjects, and most especially upon those they know nothing about, that is the secret of quackery in politics and religion, as well as in medicine.—Dr. Baldwin, in *American Journal of the Medical Sciences* for July, 1850.

* There is one point, amongst others, in the nervous system yet to be elucidated—Why the action of the flexor muscles seems so generally to predominate over the extensors, whether it be in cases of spasm or paralysis. In the former the joints are always brought into a state of flexion; in the latter the extensors are generally paralysed, while the flexors remain with the power of contraction.

AN ESSAY ON
UNHEALTHY INFLAMMATIONS.

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[Continued from p. 501.]

ERYSIPELAS (continued).

The arguments in favour of an erysipelatous poison—The origin and underrivativur of that poison—Its existence in the atmosphere.—Its spontaneous origin in the living body.—How accounted for in traumatic erysipelas.

BUT, if we cannot demonstrate the precise nature of the poison which I believe to lie at the bottom of the disease, let us examine the grounds upon which we may deem the evidence to be strong in favour of such (a poison) being the rationale of its phenomena.*

In the first place, when blood of a perfectly healthy description (the "good blood" of Paulus Ægineta) is determined to the skin by cold, by fire, or any other simple exciting cause, we observe it to limit itself to a particular extent of surface, to carry out all its actions within that space, to invade the part deliberately and progressively, and to decline and disappear in the same orderly manner. It is quite foreign to the behaviour of "good blood" to pounce suddenly upon, and develop itself instantly in, a new part; and it is equally opposite to its nature to quit that part suddenly and to set up a similar disturbance in some distant locality. It is just as little in keeping also with its tendencies, when once in possession of a part, to transmit itself therefrom to any considerable distance by direct extension. But, where the blood is in a diseased state, it is a prominent feature in the character of such to play all these "antics," of which "good blood" will never be found to be guilty.

In the next place, the commixture of

a poison with the blood, but more particularly of an animal poison, very generally affords early evidence of its presence in the system by its visible action upon the skin; in other words, by the production of some form of rash, blotch, or pustule. I believe this to be the rule, and the absence of such the exception. Take *typhus fever* for an example;—not to speak of the "maculated" form of the disease (an eminently ataxic form of fever), nor of others distinguished by petechiæ or by a varicelliform eruption, as described in Cheyne and Barker's work on Fever (vol. i. p. 431): the ordinary typhus of this country (even when not epidemic) will discover almost always to the naked eye a state of cutis allied to that of a general efflorescence; whilst "the desquamation of the cuticle, which usually takes place when a patient is convalescent from typhus" (Graves, p. 57), bespeaks the direct violence to which the cutis has been exposed by the morbid action of the poison. In this circumstance we behold a strong confirmation of the views which seek to enrol typhus amongst the exanthemata. In those sundry instances of adynamic types of fever, wherein mucous membrane plays a prominent part in the morbid phenomena, and the pathological condition of which after death is found to be allied to what I have just spoken of as occurring on the external skin (but which M. Broussais has elevated to the dignity of true inflammation, and invested with the importance of *originating the fever*).—in these instances, I regard the increased redness and tumefaction as *consequences of the poison*, correlative with and strictly analogous to those arising out of its influence upon the skin. If such be the correct light in which to regard them, how diverting the anathema of M. Broussais, who affirms that it is "only national jealousy which prevents the English physicians from opening their eyes to the great beauty" of his first-born—the *gastro-enterite*!

I have instanced typhus fever as an illustration of the position that morbid poisons particularly impress the skin in their play upon the actions of animal life. It is almost superfluous to introduce the formal mention of the exanthemata, as further proof of the same. The single quality of segmented colour

* "Etiam in æquivoce hæc inflammationis genere, principium morbi irritamento vires vitales excitante, et vasorum dilatationem efficiantem consistit; ejusmodi irritamentum ut plurimum est humor acris, tanquam, ascrementum, morbosus."—Cullen, *Chirurgia Moderna*, vol. i. p. 219.

which attends the family at large, denotes the irritation to which some latent cause has exposed them.

Let us next call to mind the severity of the dermoid affections ensuing upon the inoculation of the system with the matter of glanders and farcy;—the scarcely less violent affection of the skin originating in dissection wounds, where the poison introduced is much concentrated, and the blood of the recipient impure;—the thousand and one skin diseases engendered by the poison of syphilis;—the direct operation of the poisons of gout and rheumatism upon the cutis, in the struggle of the system to berid itself, and throw them off;—the morbid conditions of the cutis consequent on imperfect digestion (whether the result of disease, of the bottle, or of hereditary descent), and mal-assimilation of the food, with their natural train of evils, impure chyme and chyle, and, secondarily, impure blood;—the appearances assumed by the same structure after the absorption by the lacteals of the impurities contained in several of the fish tribe;—and lastly, those arising as a consequence of the long-continued absorption of certain medicines, as, *par. ex.*, copaiba, mercury, iodine, &c., into the blood, &c. &c.; in most of which examples the behaviour of the dermoid eruption observes a general resemblance, more or less close, so that I have dwelt upon as originating in the poison of erysipelas.

Having drawn attention, thus imperfectly, to the almost* universal rule that the presence of a poison in the blood is productive of a direct and specific operation upon the skin† in its transit through the capillaries of that

emunctory* (which operation is very generally evident to the naked eye,) I throw out the suggestion, by the way, for future inquiry, whether it be not highly probable that, in those anomalous forms of the exanthemata in which the rash is absent or defective, an explanation of the departure from Nature's ordinary rule may not be found in the supposition, alluded to a little above, that the poison is seeking other channels of elimination *within*—as the kidneys in scarlatina, the mucous membrane of the intestines and bronchi in measles, typhus,† and the like? Upon such supposition we are enabled to reconcile the occasional anomaly which we meet with in those cases in which the constitutional phenomena are present, but in which the characteristic eruption is wanting:‡

ghost, recounting to Hamlet the manner of his death, is made to say:—

“Sleeping within mine orchard,
My custom always of the afternoon,
Upon my secure hour thy uncle stole
With juice of cursed hebenon in a vial,
And in the porches of mine ears did pour
The leoprous distilment; whose effect
Holds such an enmity with blood of man,
That, swift as quicksilver, it courses through
The natural gates and alleys of the body;
And, with a sudden vigour, it doth posset
And card, like eager droppings into milk,
The thin and wholesome blood: so did it mine;
And a sweet instant tetter bark'd about,
Most faster like, with vile and loathsome crust,
All my smooth body.”

* Not the least striking illustration of this fundamental principle is the fact that erysipelas is occasionally met with in the new-born infant. † Erysipelas has been found upon the skin of the new-born infant, of which we have several instances mentioned in the Medical Commentaries as having occurred in the British Lying-in Hospital. In one of these the child was born with its whole face swelled and inflamed, the left side having a true erysipelas upon it. There was likewise an inflammation on the legs, feet, and left hand; on each tibia there appeared an oblong sough, of a dark brown colour, almost livid, that on the left being very large.” Vesications ensued, and, in some places, soughs. “The disease was observed to attack the children of weakly women, and of such as were addicted to the drinking of spirituous liquors.”—Latta's Practical System of Surgery, vol. i. pp. 136-8.

† In Cheyne and Barker's work on Fever, a case is made to an epidemic typhus in Ireland occurring in recent times, wherein increased heat of surface was often absent. Such cases usually terminated ill.

‡ “One step towards future success in these inquiries will be the relinquishment of the too exclusive attention hitherto given, even by physicians themselves, to the *eruptive part* of certain maladies.” “The translation of morbid action from internal to external parts, though far from proving identity of cause, yet indicates a series of similar morbid changes and actions analogous in their nature and progress, amongst the most important in the whole history of disease. The eruption here is not itself the disorder; but one

* The almost universal rule. A striking exception occurs in the case of hydrophobia, whose origin in an animal poison is now pretty generally assented to. Dr. Todd's views in relation to the generation of a blood-poison in the family of the neuroses, to whose immediate agency he attributes their external manifestations,—*ex. gr.* the muscular phenomena in chorea, tetanus, epilepsy, delirium tremens, &c.—would, if substantiated, augment the number of exceptions which militate against the principle contended for in the text. But, although an ardent admirer of Dr. Todd's labours in this department of medicine, I do not think he has “made out a case” for the association of these diseases with a toxic origin, although eminently dependent, no doubt, upon disordered nutrition of the nervous centres.

† Shakespeare, who was very happy in his descriptions of the play of disease and of Nature upon each other, and who delighted in such conceptions, has manifested his acquaintance with this principle in the following instance. The

To offer a last, and not the least striking argument in support of the doctrine of a poisonous origin for this disease, I may advert to the fact that erysipelas, strictly so-called, is incapable of elaborating *healthy pus** or *coagulable lymph*—the natural products of healthy inflammation. That the latter is never met with in erysipelas is a marked feature of the disease, its absence giving rise, as Mr. Travers observes, to the tendency of the inflammation to *spread*. The absence of that boundary which Nature throws up to limit the range of ordinary inflammations can scarcely fail to encourage the *creeping* tendency of this.

A host of authorities has testified to the inadequacy of erysipelas to the elaboration of *pure pus*. "Nunquam in veram suppurationem abit,"† says one; "Rarissimum" est (observes another) "erysipelas in suppurationem desinere, nisi phlegmonodes, ipsum extendit;"‡ "It est rare que l'erysipele suppure,"§ a third; "It" (erysipelas) "never runs into suppuration, but only, with weakly constitutions and other concurrent circumstances, into ulceration and gangrene; in consequence of which the destruction of the surface of the body extends to the parts beneath, and there ensues, not a bounded fluctuating abscess, but an open, wide-spreading, putrid, ulcerating surface."|| "Any effusion with which erysipelas is attended is commonly thin and acrid, and not convertible into pus."¶ "The true erysipelas" (according to Macbride) "never ends in an abscess:" and Hippocrates himself, more than two thousand years ago, insisted upon the same.

In erysipelas, uncomplicated with affection of the areolar and deeper tissues beneath, a wound occurring in such disease will not engender pus, but a serous weeping is set up in the part, which is wont to last a much longer time than healthy pus would do. My

valued friend, Dr. Weatherhead, in his essay on the "Diagnosis between Erysipelas, Phlegmon, and Erythema," has quoted a striking case from Bichat, "where 'un cautère établi au bras gauche cessa de suppuré' when attacked with erysipelas, and where the erysipelas was repelled by reproducing the suppuration by an ointment made of basilicum and powdered cantharides" (p. 17). "This is a fact" (observes Dr. Weatherhead) "which goes strongly to establish the distinct natures of phlegmon and erysipelas, and tends to show that they are not convertible, that suppuration is not natural to erysipelas, and that the phlegmon present is not an extension of the erysipelatous action," (loc. cit.) The same authority informs us that, encountering himself an "infectious erysipelas of a phlegmonous type," he made an opening into the most soft and tumid parts, out of which, instead of pus, flowed serum tinged with blood.

"*True suppuration*" (says Hunter) "I believe is a consequence only of inflammation. In treating on the cause of suppuration—viz. inflammation—I hinted that there were often swellings, or thickening of parts, without the visible or common symptoms of inflammation—viz., without pain, change of colour, &c.; and I also hinted, in treating of suppuration, that there were collections of matter, somewhat similar to suppuration, which did not arise in consequence of the common inflammation. I conceive all such collections of matter to be of a scrofulous nature: they are commonly called matter or pus, and therefore I choose to contrast true suppuration with them. Although I have termed this *suppuration*, yet it has none of its true characters, any more than the swellings which are the forerunners of it have the true characters of inflammation; and, as I did not call them inflammatory, strictly speaking, I should not call this suppuration, but I have no other term expressive of it."* Hunter is here strictly speaking of scrofulous affections—"of slow swellings in the joints," "tubercles in the lungs," "the suppuration of many joints, viz., those scrofulous suppurations in the joints of the foot and hand; in the knee, called white swellings; the

only, and that not always the most critical, of the series of changes composing it."—Holland's Medical Notes and Reflections, p. 90.

* *Erysipelas strictly so-called*—i. e., before it has involved the areolar tissue and subjacent textures. Callisen remarks:—*Erysipelas idiopathicum rarissimè vel potius nunquam veram suppurationem admittit nisi phlegmona simul junctam fuerit*.—Chirurgie Hodierne, p. 219.

† Lorrus, De morbis cutaneis.

‡ Burserius, De Erysipelate, vol. ii. cap. ii. p. 16.

§ Tissot.

|| Celsus, by South, vol. i. p. 98.

¶ Benjamin Bell, vol. i. p. 120.

* Hunter's Works, "Of the Suppurative Inflammation," vol. iii. p. 419.

joint of the thigh, commonly called hip cases; the loins, called lumbar abscesses," &c. Hunter was certainly incorrect in asserting that these collections of pus (or of "sanies," as he terms them in another place) were independent of inflammatory processes; but those processes are exceedingly *low* forms of inflammation, or, as I prefer calling them, "unhealthy" forms of inflammation. Hunter, therefore, in my opinion, would have been more correct in saying that "true suppuration is a consequence only of *healthy* inflammation." "These formations of matter" (he continues), "although they do approach the skin, yet do not do it in the same manner as collections of pus." They "are always larger than they would have been if they had been either a consequence of inflammation or attended by it: this is owing to their indolence, allowing of great distension beyond the extent of the first disease, and even of their moving into other parts; whereas an abscess, in consequence of inflammation, is confined to the extent of inflammation that takes on suppuration, and its rapid progress towards the skin prevents distension, and, of course, extension, of the disease" (p. 420). How applicable are not these observations to the process of softening in erysipelas,—bearing in mind that Hunter was certainly in error in disconnecting the sanious collections in the foregoing diseases from inflammation.* "All these formations of matter not preceded by inflammation, nor a consequence of it, are, I believe, similar to each other, having in this respect one common principle very different from inflammation;" which common principle is, he says, that none of the diseases giving rise to them produce pus until exposed, and that none of them terminate in a disposition to *heal*, which is always the case where true suppuration arises,—Mr. Hunter having considered inflammation always a salutary and never a diseased action. Without subscribing entirely to this position, I will add, from myself, that they certainly do

all possess "one common principle"—viz., *that of having their origin in the circulation of a poison in the blood*; and that it is to the presence of that poison that they are indebted for their inability to elaborate healthy pus. In this category I beg to be understood as including erysipelas in the most unconditional sense. Indeed, Hunter himself admits (p. 454), in his chapter on Pus, that there is something peculiar and unhealthy in the suppuration of erysipelas.*

Whether the same series of physical and vital changes occurs in the capillaries of the skin in erysipelas and in those inflammatory conditions of this structure allied to it, which *set in suddenly, and as suddenly take their departure*, as is known to characterise them in true inflammation, is too large a question to be discussed or decided in this place; but I acknowledge that I have my doubts upon the subject. We know that in true (healthy) inflammation it is not *until after a certain lapse of time* that the dilated capillaries become obstructed, and suffer the liquors sanguinis to exude through their walls. Nor, is it consistent with what we know of or see in this process, that such obstruction should suddenly be removed, and the enfeebled and dilated vessels as suddenly regain their natural calibre and tone; and in ordinary inflammation we are acquainted with no examples of such "*open and shut Sesame*" phenomena. But *here*, on the contrary, if the capillaries do undergo the same processes as in the case of true inflammation, it is necessary to admit this seeming paradox in Nature's operations. But, where these pseudo or unhealthy inflammations do go to the extent of

* It was not until I had written this sentence that I perused the lengthened foot-note (by the Editor of Mr. Hunter's works) at the end of the section on "Suppurative Inflammation" in the volume before me, when I was pleased to find that the able writer of it had anticipated me in the same line of strictures upon our great countryman's views of the non-inflammatory nature of the scrofulous affections alluded to.

* "To see how far the nature of sores might be ascertained from the nature of their discharge, matter from a cancerous sore has been analysed, and the result has been that such matter differs from the true pus: but this explains nothing more than what the naked eye can perceive—that it is not pus; but it will not show the specific difference between the matter from a cancer and matter from a venereal bubo where mercury has not been given, nor will it tell that one is cancer and the other is venereal! We might as well analyse the urine at different times in order to ascertain the nature of kidneys at those times!" (Hunter, vol. iii. p. 451-2.) Hunter's far-sighted sagacity has been remarkably at fault in these two instances. What would he say could he now re-appear among us, and satisfy himself that the microscope has realised both his seeming impossibilities,—that the peculiar cell of the former betrays to that instrument the existence of cancer, and the fat globule in the latter the presence of renal degeneration!

exudation, the latter consists either of serum alone, or, if of the liquor sanguinis, this is so altered in composition that it fails to coagulate into a healthy blastema, and to originate the cyto-blasts or cells of healthy inflammatory exudation. Hence, I conceive, the explanation of true pus corpuscles failing to be developed from the so-called *inflammatory* exudation of erysipelas,—or, if the cytoblasts be formed, of their being converted into abortive pus corpuscles. “Valentin, Gerber, and many others,” observes Mr. Paget,* “have not sufficiently, I think, if at all, dwelt on the probability that some pus cells are ill developed; others degenerate from a previously higher development. The many varieties of form, and the many differences of the conditions in which they occur, may be thus explained. I think, too, that the characters of *degeneracy* or imperfect development in the liquor puris have been too much overlooked.”

But I have said that the erysipelatous inflammation is incapable of elaborating *coagulable lymph* also, as well as pus. This primary product (lymph) of the healthy inflammatory process has no existence in the progress of the disease which we have now in hand: the inflammatory action here, as Mr. Travers has well observed, “is in itself wholly destitute of a healthy character, forming no stage of a process necessary or intermediate to recovery;” on which account it follows that the exudation cannot become solidified or organised so as to be developed into living tissue. “In regard to the influence of the state of the blood,” observes Mr. Paget, in the lecture from which I have just quoted, “in determining the characters of an inflammatory product, Rokitsansky has happily expressed it by saying that ‘the product of the inflammation exists, at least in part, in its germ preformed in the whole blood.’ “It is not difficult to show” (adds Mr. Paget) “that a certain character is commonly impressed by the *state of the blood* on the inflammatory product from it.” And he then lays it down as “a general truth that each morbid condition of the blood is prone thereto to produce an inflammation in a certain part or tissue, and to give to

that inflammation a certain form or character” (p. 1014-16).

That neither lymph nor true pus occurs as a product of erysipelatous inflammation is the consequence, then, in my opinion, of the circulation of a poison in the blood, the presence of which produces a septic or dissolved condition of that fluid, which renders it incapable of elaborating either.* We have here an instance of the “*præcipuus alter adjunctus morbus*,”† which Morgagni explains as the cause of the *malignancy* of some fevers as distinguished from benignant ones. That we cannot afford *palpable* evidence of its existence is no more a reproach to us, than that our senses are unable to take cognizance of the exact nature and properties of the poisons of malaria or of scarlet fever. We do not, on that account, refuse our assent to the reality of *their* existence and operation; and in demanding the same latitude for an *erysipelatous poison*, we are “accountant only for as great a sin” as we are in the cases of those diseases. Upon these principles, then, and upon this reasoning, I take my stand in this matter. “Principles,” it is said, “have no modesty. A principle never rests until it has gained the victory; and it cannot be otherwise. With it, to reign is to live. If it does not reign supreme, it dies.”‡ Let this principle, too, I now contend for, reign supreme, or let it die.

In connection with the present division of our subject, a question remains to be considered which in obscurity is inferior to none of the many that attend the history of erysipelas. That question has reference to the *origin* of the poison (or poisons) the existence of which in the causation of this disease we have hitherto been considering. *Whence* the source from which it is brought into operation? Does it exist in the atmosphere at such times? and if so, is that

* We know that certain *natural* poisons will destroy the coagulating properties of the blood almost instantaneously (vide Mead on Poisons), and render it permanently fluid. “It is well known that volatile alkali salts mixed with the blood when just drawn, or rather as it runs from the vein, keep it from coagulating, and hinder it from separating into crassamentum and serum, as usual: the experiment is easy, and every one will find it true on trial” (Huxham on Fevers, p. 51).

† De Febribus quædam, liber v., epistola 62, tomus 3tus, p. 378.

‡ D’Aubigné’s History of the Reformation, preface.

* “On the Processes of Repair and Reproduction after Injuries.” Lecture IV. MEDICAL GAZETTE, No. 1122, July 13, 1849.

element the invariable nidus of its creation? Or, on the other hand, is it engendered within the body itself, and wholly unconnected with influences external to the same? Or, lastly, is it of a compound nature, traceable to the united agency of both sources? and if so, are we able to point out the order and sequence of their respective operations? Than this, a more difficult problem in medicine can scarcely be conceived. It has baffled the researches of the acutest thinkers, and of the most mathematical reasoners; so that I may well be excused if I confine myself to little more than an enumeration of the several difficulties which surround the inquiry.

Its atmospheric origin?—"The remote cause of erysipelas," observes a great modern authority, "must exist at all times in the atmosphere, or else the human body must possess the power, when acted on by certain predisposing causes, of spontaneously generating this peculiar poison. The doctrine, however, of the spontaneous generation of any poison is ill supported by argument, and not generally received; and, consequently, the more probable hypothesis of the remote cause of this disease is, that it is at all times diffused through the atmosphere, varying greatly in quantity or intensity; for, although the disease is at all times sporadic, yet in some years it is epidemic.*"

Supposing the particular poison now under our notice to be at all times diffused through the atmosphere, and that one man shall escape while another will succumb under its influence, according as his system shall be predisposed to disease or the contrary, is a view which at least has the merit of disembarassing the question of almost all its difficulties; and, although it may not be free from some objections, it is the doctrine, of all others perhaps, which will best dispose of the many conflicting facts connected with the question of causation in this disease. That the atmosphere cannot be thrown out of the account altogether must be allowed at once, I think, if we admit the occasional occurrence of an epidemic form of the affection. And, if we admit a certain state of the atmosphere as capable at one time of engendering the disease, it is difficult to see

how we can refuse to recognise the possible influence of that element at another. The former admission plunges us, plainly enough, into this obligation.

Its origin by spontaneous generation?—The strongest argument which can be advanced in opposition to the atmospheric view of this question is to be found, perhaps, in the circumstance that erysipelas will spring up in a single individual, who, as far as we are able to judge, is in possession at the time of the rudest health, and surrounded by every circumstance conducive to the preservation of that state: in a word, that the disease is as common in a sporadic as in an epidemic form—far more so, I think, to speak correctly. Now, it is all very well in such cases to berid ourselves of the difficulty by presuming a *predisposition* on the part of the affected individual to take on the disease. The fact is, that such occurrence deals a heavy blow at the very foundations of the edifice of the atmospheric philosophers! I am supposing a case now, not in which a well-fed countryman shall break his leg, or incur a scratch on his body, and erysipelas break out in that part, but in which, while going about as usual, he shall be knocked down by the disease, while those around him, under less favourable circumstances, shall escape. Oh! but, then, exclaim the atmospheric logicians, his system must have been predisposed to take on the disease! To such mode of reasoning I know of no answer but that of my uncle Toby's under the like circumstances—videlicet, to put one's hands in one's breeches pockets, and indulge in a *hillibullero*! For, it is unanswerable.

But there is yet another difficulty surrounding that view of the proximate cause of this disease which refers it to the circulation of a poison in the blood, that demands a word or two at our hands ere I take my leave of the present division of this subject. The opponents of the *toxic* rationale of erysipelas have brought forward *traumatic* cases of the disease as, in their estimation, a decisive answer to those who contend for the necessity of a poison in its causation. And it must be acknowledged that herein lies their strongest argument, and a position not easy to subvert, either by direct facts or by unobjectionable reasoning. Systematic writers on erysipelas have been remarkably silent upon this point. Mr. Tra-

* Elements of Medicine, by Dr. Robert Williams, vol. I. p. 236.

vers has touched upon it in his work on "Constitutional Irritation;" but I can not satisfy myself that he has thrown any additional light upon this obscure question. "It may be asked," says he, "how is the appearance of erysipelas in a person previously healthy, after a lapse of forty-eight hours or sooner from the infliction of an injury, to be reconciled with the statement of its constitutional origin?—Simply thus: the elements of the disease have been the immediate workings and results of the injury, or, if they have pre-existed, they have been stirred into activity by its direct effects." "That a certain morbid condition is an essential preliminary to its appearance, as to that of gangrene and tetanus, is the strongest proof that can be had of its constitutional origin." Again: "Do we not see nausea, retching; obstructed or paralysed bowels, skin, and kidneys; head-ache, loins-ache; anxiety, stupor; delirium; full and bounding, contracted and wiry pulse, all following upon an injury within the same short period? And can it be questioned that here is combustible material for the change upon the part injured, whether erysipelas or gangrene? Would it not be more wonderful if, with such commotion of the citadel, the tranquillity of the outposts was undisturbed, even without the provocation of assault?"

But we are to remember that Mr. Travers explains all, in the pathology of this disease, by irritation through *reflex nervous* action, and not through the agency of blood. If "the elements of the disease have been the immediate workings and results of the injury," they produce their irritating effects on the great nervous centre, which in its turn (and after time sufficient has been allowed for the establishment of that commotion in the system, which he details) reflects that irritation back upon the originally injured part, the violence done to which lays it particularly open to a local manifestation of the same. Upon the nervous hypothesis, then, the appearance of the disease in the injured part might perhaps be allowed to be intelligible enough.

But, as I am not disposed to admit the phenomena of erysipelas to be explicable upon reflex nervous action, except in so far as that action is subordinate to the primary agency of blood, the above exposition offers no solution

of the question we have in hand. If we premise that erysipelas can only occur in such cases as are characterized by a previously unhealthy state of the blood (a position for which I must resolutely contend—Mr. Travers himself insists on the necessity of an "unsound condition of the system" for its appearance), the spontaneous generation of the poison in that blood might, it is conceivable enough, be developed in the injured parts the nervous actions of which had been done a violence to, or in some way been modified by the lesion. A little thing, under such commotion, might stir up the already existing poison to reflect or develop itself in the part. In no other way than through its spontaneous generation in the body can I conceive a poison to explain the occurrence of erysipelas in an injured part. And I confess that I think there are more arguments in favour of the possible generation of such in the humors than is assented to by Dr. Robert Williams.* We talk of the liability of persons with an *unhealthy state of body* to develop certain disorders. But in what does this unhealthy state of body consist, but in the decomposition of the elements of their blood, and in the consequent escape or generation of poisonous materials or gases? A medical student shall dissect and pore over his midnight lamp for twenty out of the twenty-four hours, and this through a period of six months consecutively. At the commencement of that period he might have wounded himself in the prosecution of the former occupation, again and again, with *impunity*; but, *at its close*, the case is quite altered; the prick of a needle will now engender diffuse inflammation of the integuments or of the areolar tissue, and such accident shall go hard with his life. And why?—Because long confinement, the absence of air and exercise to oxygenate and purify the blood, &c. &c. have disturbed the affinity between its several elements, and given rise to new compounds ungenial and unhealthy to the living system: in a word, to the generation of the so-called poisons.

That the blood is liable to undergo great alterations in its constitution, as well from an excess in some of its com-

* In connection with this question the reader is referred for many valuable remarks to the latter part of Mr. Simon's last lecture on "General Pathology," lately published in the pages of the *Lancet*.

ponent elements as from a deficiency in others, is a fact so incontrovertible, that I need not waste the time of the reader in enforcing it.* These alterations may be due to constitutional peculiarity. They are the result, again, at one time, of a vicious elaboration of the chyle and of the chyme, which in their turn elaborate a vicious blood. At another, they are due to a failure in the depurating functions of the body, and to a consequent retention in the vital fluid of materials intended to be thrown out of it. There is not the least doubt in my own mind that a *poison may thus be engendered*; or, if you will, that a state of blood may arise which, while it shall be unfit for the healthy nutrition of the body, may be capable of giving rise to erysipelas.† Such poison, in its transit through the capillary blood-vessels of the skin and mucous membranes, I suppose to be capable of occasioning an irritation that, under certain circumstances (which I do not attempt to explain), shall end in the disease under our notice. I see nothing at all far-fetched in this presumption; while it must be allowed the recommendation of sufficing to explain the much more frequent occurrence of erysipelas in the

debilitated constitutions of the naturally delicate, and in the broken-down ones of the dram-drinker, the Falstaffs, and of the "Slipperd Pantaloon," than in the robust and temperate.

In concluding this division of the subject, I wish to say a word or two upon what to me appears an error so universally fallen into by systematic writers, that it has come to be received as an acknowledged tenet in our articles of faith. I allude to the prevalence of parading forth certain *consequences* of the general pervasion of organs by a poison, as *salient points in the symptomatology and causation of the disease*. In the present instance, I will allude only to the derangements in the *hepatic* functions in the course of erysipelas, (to which our best writers are in the habit of inviting attention,) as an argument illustrative of the *influence of the liver upon the disease*. I would refer the reader to Dr. Stevens's work on the Blood for some very judicious observations upon this fallacy of physis and physicians.

[To be continued.]

ON THE USE OF CHLORINE FOR PREVENTING THE TRANSMISSION OF THE POISON OF PUERPERAL FEVER.

DR. SEMELWEIS has traced the great prevalence and mortality of puerperal fever in some of the obstetric wards of the Vienna Hospital, to the transmission of cadaveric poison from the hands of the male accoucheurs. The precaution of always washing the hands in solutions of chlorine, before making an examination, has been attended with the most marked success in arresting the occurrence of the disease. Thus the mortality, which was 9.50 per cent. in 1840, 8.44 in 1841, 16.98 in 1842, 9.57 in 1843, 8.91 in 1844, 7.40 in 1845, 13.68 in 1846, fell to 5.21 in 1847, and to 1.27 in 1848, the two years in which this precaution was first introduced.

The patients having been attended in two departments, in the one by medical men, and in the other by midwives, the mortality among the latter had been thirty-two, while it was six hundred out of the same number of cases under the care of the former. The same relative mortality attended a change of place, and was attributed to the circumstance of post-mortem examinations having been made by the male accoucheurs.—*Gazette Médicale*. x

* I may take this opportunity of explaining that it was in this sense that I intended to be understood when associating *chlorosis with a poison* in another part of this essay.

† "The causes of this malady," says Dr. Copland, "are frequently the same as those of low forms of fever, catarrh, and rheumatism; for, like them, it generally proceeds from peculiar states and vicissitudes of weather and of the atmosphere, especially cold, moist, miasmatic, and foul conditions of the air, acting upon a system already disposed to their influence by depression of vital power, or by the accumulation of morbid or effete matters in the circulation, owing to defective action of the excreting organs, to unwholesome diet and regimen, or to prolonged disorder of the prime viæ."—*Dict. of Medicine*, vol. i. p. 819. And, again, in another place, "Sir A. Carlisle says, that 'it is a humoral and constitutional inflammation occasioned by alimentary crudities,' and attended by an excess of acid in the fluids. That it is a humoral and constitutional inflammation is very generally admitted, and that alimentary crudities often precede and attend, it is also evident. I conceive, however, that not only a vitiated state of the chyme and chyle, proceeding from weak digestive power, or unwholesome and indigestible food, and vitiating the circulating fluids, but also the absorption of morbid effluvia, and the retention of effete matters in the blood owing to impeded excretion, either by the skin, the liver, the kidneys, the mucous surfaces, or the uterus: in short, that a morbid condition of the fluids, arising either from the passage into them of contaminating materials, or the retention in them of effete elements that are constantly being excreted by the various excretories, are the principal changes productive of this disease" (p. 287).

MEDICAL GAZETTE.

FRIDAY, OCTOBER 11, 1860.

THE dispute which has so long existed between Assurance Offices and Medical Practitioners, respecting the payment of a fee for filling up the usual certificate of the health of a person, has been recently turned into a question of law in the County Court of Colchester. The profession is, we consider, greatly indebted to Mr. S. A. Philbrick, a surgeon of that town, for procuring a decision on a subject which has created so much discussion; and although this decision is, in one sense, not so favourable as we might have anticipated, it fixes the law on a clear basis, and shows how, in future cases, a man may place himself in a position either to secure compensation, or entirely discharge himself of the trouble and responsibility of filling up these assurance-certificates. The case to which we refer is that of *PHILBRICK v. WHEATHAM*: it was tried in the County Court of Colchester on the 2d instant. As the particulars cannot fail to interest our readers, we subjoin a full abstract of the proceedings:—

"Defendant was sued as one of the Directors of the *National Provident Institution*, for the fee of one guinea, for furnishing particulars as to the health of a party proposing to assure his life. Mr. Hardwick (of the firm of Davidson and Hardwick, London) was retained for the Company; Mr. F. B. Philbrick appeared for the plaintiff, and observed that, although the action was brought for a small amount, it involved a question of some importance to the profession, as well as to the Society represented by the defendant. He then explained that, on the 6th of August, plaintiff received a circular letter from Mr. Marsh, secretary to the National Provident Institution, stating that, 'reference having been made to him' (Mr. Philbrick) as his medical attendant, in a proposal for a

life policy by Mr. Jas. Harwood, the Board requested that he would favour them with answers to the questions given in the document. It further stated that communications of this nature were considered as strictly confidential; and a request was added in writing that the information should be forwarded forthwith, so that the proposal might be considered and decided upon at the next Board-day. The certificate was sent up on the 8th, with a note charging the fee of a guinea; and, on the 10th, the plaintiff received a letter from Mr. Marsh, stating that it was not the practice of the office to pay fees to the medical attendants of assurers. Some correspondence on the subject not resulting in payment of the demand, the plaintiff had thought it right to avail himself of the assistance of the County Court for trying the question. The case occupied a considerable time, and the law applicable to it was the subject of a lengthened argument between His Honour, Mr. Philbrick, and Mr. Hardwick.

"The plaintiff, in his evidence, said that for some years he had furnished similar certificates to the office in question without charge: life assurance was not then so prevalent as to make the practice a burden to the profession; but since these applications had become so frequent, he had refused to supply the information without payment. He mentioned two instances in which he had actually received payment through Mr. Hayward, the Society's agent in Colchester—in December, 1848, as to the health of Mr. N. Cobb; and in the spring of this year, as to Mr. Linnett Bibby. In cross-examination he admitted that in the latter case Mr. Hayward had told him that the money came from the party effecting the assurance; but there was no such intimation in the case of Mr. Cobb: had heard Mr. Hayward say in conversation that offices did not pay these fees, but he had added the hope that he (Mr. Philbrick) would make them. Mr. Philbrick was about to call Mr. Waylen as to the reasonableness of the fee, but Mr. Hardwick said he should raise no question as to the amount: if the Society were bound to pay anything, they were willing to pay the full fee.

"The defence relied upon was in brief that the party being desirous of insuring his life—as it was one of the requisites

to enable him to do so, that he should refer to his medical practitioner—the *onus* of payment was upon the assurer, and not upon the Society. At the suggestion of the Judge, Mr. Hayward, the agent, was examined, and stated that when he applied to Mr. Philbrick about the first case he had mentioned, Mr. Philbrick said he had had so many similar applications as to Mr. Cobb, that he should decline furnishing a certificate without a guinea fee: he told Mr. Philbrick that the Society did not pay those fees, but that he would see the parties proposing to assure; and subsequently, by their authority, he paid Mr. Philbrick the guinea, explaining at the same time that it did not come from the office. In Bibby's case the transaction was similar. In answer to the Judge, Mr. Hayward said Mr. Philbrick had repeatedly told him he would not furnish any more certificates without a fee; but he had never said that *in case of applications to him he should hold the Society liable*: believed this was the first circular sent to Mr. Philbrick direct from the secretary; those which had been furnished gratuitously had all come from himself as agent.

“Mr. PHILBRICK submitted that the last answer was a very material one to the plaintiff's case, as showing a *new mode of application* to him after he had declined to furnish more certificates without payment. He also urged that upon the face of the letter itself, the agreement was between the office and the plaintiff; for he did not see how the letter could be construed otherwise than as a request from the office to perform certain services on their account.

“His Honour said, if the letter had stood alone, and this had been the *first transaction* of the kind between Mr. Philbrick and the office, he thought they would have been liable, and must have paid him; but now, as it appeared to him, the whole depended upon what had previously taken place. Of course Mr. Philbrick was not bound to continue furnishing certificates gratuitously, but he was of opinion that *he could not claim payment without previous notice*. Judgment for the defendant.—Mr. HARDWICK said he should make no application for costs, which, his Honour remarked, was a very liberal course. Many of the medical gentlemen of the

town were in Court during the trial, which appeared to excite considerable interest.”

The decision appears to have been fair and equitable. As no notice had been given to the Assurance office that certificates would in future be charged, and they had hitherto been given without charge, or, as alleged in evidence in two instances quoted, the payments had been made, not by the office, but by the insured,—it does not appear that any other judgment could have been delivered than that above recorded.

We therefore learn from the result of this case, that a medical practitioner who has already furnished certificates to an Office without payment, cannot legally claim payment for any new case unless he has given previous notice of his intention to make such a claim. The fact of the application for a certificate coming from another person, but still acting on the part of the Office, does not in any way affect the question. If, however, an Office should for the first time make an application to a practitioner, he can demand, and we apprehend recover, payment in a County Court for the important service thus rendered. The party making the application (*i. e.*, the Office) will be liable in law for a fair and reasonable remuneration. If, as they say, it is not their practice to pay such fees, and the service is really rendered to the insured person and not to the Company, this will be no defence, because, according to English law, as it is at present administered, a man who takes upon himself to give an order for an article, whether for an Insurance certificate or an arm-chair, must pay the party supplying it; and if, as it is cunningly alleged, the service is really rendered to another, then the party ordering the article has his separate remedy by action against that individual. Insurance Offices cannot benefit as principals, and evade

their responsibility under the pretence of being agents.

We do not doubt, after this decision that there will be another move on the chess-board on the part of the Offices; but if the members of the profession show the same spirit as Mr. Philbrick, it can only end in check-mate! On applying to a practitioner for the first time, or to one who has already supplied gratuitous certificates, but has since given notice that in any future case he will require a fee, it is most probable that the letter of application from the Office will contain a small printed line at the foot, to the following effect:—"N.B. It is expected that this certificate will be filled up and forwarded by Mr. — *gratuitously*. This Company does not pay fees for medical certificates." A practitioner will, however, be then placed on his guard. Unless a stamped envelope for returning it be enclosed, he should take no more notice of the application than he would of the well-known circulars of the Austrian Lottery agents, who promise the chance of a duchy with its title and appurtenances, on the purchase of a ticket at the cost of a few rix dollars. Both parties attempt to extort something for nothing, and the application should be treated accordingly. Let it be duly considered that a man who henceforth grants an insurance certificate, except in the case of a brother practitioner, without receiving a fee from the person who applies for the document, we care not whether it be the Office or the insured, is inflicting a gross injury on the profession, and retarding the settlement of a question which is seriously affecting its rights. Insurance offices, in disputing claims, have repeatedly forced into the witness-box surgeons who have given these *gratuitous* certificates, when, in speaking the truth on oath, they have been compelled, probably to the great injury of their pro-

fessional practice, to support the case of the Office on a document alleged by them to be of a *private* nature. The gratuitous certificate writer may rest assured that its privacy is only maintained by the Company so long as it suits their pecuniary interests, and no longer. Although they have not paid a fee for it, they will not hesitate, when an opportunity occurs, of turning it to the highly useful purpose of defeating a claim for three or four thousand pounds.

With facts like these it is scarcely necessary to treat as a serious argument the plea for non-payment invariably alleged by the non-paying Companies—namely, that the certificate is for the benefit of the proposed insured. He who applies a document to his own use must be considered to derive benefit from it. The proposed insured for himself neither requires nor makes use of such a document. He is ready to pay down a premium without a certificate: the Company, on the other hand, are not prepared to take his premium until they have ascertained the probable amount of risk from his usual medical attendant. Their pecuniary interest lies in ascertaining and fixing this risk as closely as possible. They cannot determine this without the medical certificate: it is therefore contrary to fact to allege that the *insured* and *not the insurer* derives the benefit from the certificate. There can be no doubt that the insurer is equitably liable for the fee: and to put this question to the best test, the following plan might be adopted by the Companies and the profession:—Let it be understood that the person proposing to insure his life pays the medical fee—the medical certificate is clearly his property, and should pass through his hands to the Company for their *inspection* only. He who has paid for the document has acquired property in it, and has a right to keep it. This plan might not accord with the views

of Insurance Companies: but if their argument for non-payment be well-founded, on what principle can they object to the adoption of such a plan?

In the meantime, however, we most strongly protest against medical men being made the victims of a mere quibble. Valuable service is rendered by the certificate: the party who requires and retains it refers for payment to him who does *not* require or retain it. This is mere shuffling. Let insurers take lives without inquiry, or let them pay for that of which they make a beneficial use.

ANALYSIS OF THE EVACUATIONS IN CHOLERA. BY M. A. BECQUEREL.

M. BECQUEREL considers from his analysis that the choleraic evacuations consist of the serum of the blood diluted by a varying quantity of water. In this fluid is suspended coagulated albumen, the particles of which are agglomerated by mucus.

M. Becquerel notices the following facts in several analyses of the evacuations:—

1. Albumen in solution.
2. Coagulated albumen, giving the rice-water character.
3. Great abundance of albuminous particles towards the end of the disease.
4. A large quantity of chloride of sodium, almost three times as much as found in the blood.
5. Acidity of the matters vomited.

The examination of the vomited matters shows considerable resemblance between these and the evacuations.

Albuminous fluid: a portion of the albumen being in solution, and a part coagulated and suspended by mucus.

This albuminous fluid is in general neutral or slightly alkaline, and containing a considerable proportion of chloride of sodium.

The analysis of the blood gives—

Increased density of the blood and serum, more especially of the latter;

Increase in the proportion of globules;

Considerable decrease of water;

Increase of the extractive matters, chloride of sodium, and other salts, and particularly of the fatty matters, to almost threefold their usual proportion;

Slight decrease in the albumen of the serum;

Increase in the proportion of fibrine towards the termination of the case.—

L'Union Médicale.

X

CLINICAL LECTURE

OR

CASES OF ASCITES

(Delivered at King's College Hospital),

By R. B. TODD, M.D. F.R.S.

Physician to the Hospital.

(Reported by H. H. SALTER, B.A., Dem. Anat. K.C.L.)

LECTURE XI.

BEFORE I enter upon the proper subject of this lecture, namely, that of the form of dropsy called ascites, I must call your attention to the issue of the case of cardiac dropsy, which we discussed in the last lecture. Our patient, Baylis, did not live long after we last met. The dropsy, despite of the various remedies employed, increased, the difficulty of breathing increased, fluid was effused into the pleurae as well as into the abdomen, and he died exhausted.

This is the fate of all patients suffering from mechanical dropsy when the cause is irremediable. So long as the cardiac difficulty remains, the mechanical cause of the dropsy remains, and you have no means of obviating the accumulation of water save by establishing drains at various points, to carry off this accumulation of fluid. Hence you begin by trying to excite the kidneys and the bowels, great vascular surfaces whence large quantities of water may be carried off. The continued application of the various stimuli which the materia medica afford, renders these surfaces at length insensible to their further influence; they refuse to obey the stimulus, and cease to yield a sufficient quantity of fluid. It is then that you must have recourse to more direct means of evacuating the fluid from the areolar tissue in which it has collected. You will remember that in this case, having tried the acupuncture, we proposed to adopt Lombard's* method of a single incision in each leg. This practice was so far successful that it evacuated a good deal of fluid, and gave relief for some days; but it was tried rather too late, and after a good deal of inflammation had already been excited in the limbs by acupuncture. The case was, therefore, an unfavourable one for trying the practice.

The post-mortem examination showed effusion into both pleural cavities, and into the peritoneum. The cavity of the peri-

* Or more properly, as Dr. Gall has shown, a method as old as the time of Mead, by Lombard.

cardium was entirely obliterated; in greatest part by means of *old* adhesions, the result of old pericarditis; but by *recent* adhesions over a portion of the anterior surface of the heart, towards its left side, which you will remember was the seat of the rubbing sound, which was audible for some time after his admission into the hospital. The former attacks of pericarditis had obliterated the greatest part of the cavity of the pericardium, leaving a small portion in front, which became the seat of the recent pericarditis.

Our diagnosis was quite correct as far as regards the condition of the walls and cavities of the heart. We found dilated and hypertrophied cavities on both sides. This diagnosis was indicated by both symptoms and physical signs, and therefore we were not likely to be mistaken. But there were no unequivocal signs of valvular disease, and therefore we could form no positive opinion as regards their condition.

The post-mortem inspection showed that the primary disturbing cause of the action of the heart was the almost total obliteration of the pericardium by adhesions due to an attack of pericarditis eleven years ago. During all that time the heart had been bound by these adhesions, instead of moving freely in its proper serous cavity. Its contractions were doubtless, therefore, laboured, and the contents of its cavities were probably not always completely expelled; and hence, in time, all those cavities became dilated, and their walls hypertrophied.

The condition of the kidneys was what we expected to find: they were contracted as the result of chronic disease. The cortical portion was much wasted. Many of the tubes were filled with altered epithelium and granular matter; others with fat; and others were empty and shrunk, and folded into a fibrous tissue.

You noticed the immense effusion of water which existed in the pleural cavities, and how this water compressed the lungs, so as to solidify a considerable portion of them. Now what I wish to remark respecting this fluid is, that the effusion must have taken place within a few hours of the fatal event; for we examined him only three days before his death, and found distinct audible breathing at every part of the chest. How remarkable the obstructing cause, which could give rise to the exomose of all this fluid in so short a time!

I must now call your attention to some examples of another form of dropsy—namely, ascites, or dropsy of the peritoneum, which have lately been under our observation.

CLASS I.—The first case is that of a man named John Murray, *æt.* 55, who had been for several years a butler in a gentleman's

family. This case is especially interesting as affording an example in which the dropsy was on one occasion completely removed by the aid of internal remedies; and on a second, almost so; and in which, in consequence of the subsequent death of the patient from another cause, we had an opportunity of examining the condition of organs upon which the dropsy depended.

This patient was a stout, portly man, just such a man as you would expect a gentleman's butler to be. He stated that he had always enjoyed good health, and professed himself to be strictly temperate. This, no doubt, was true; but probably he kept the key of his master's cellar, and possibly may have thought it his duty now and then to taste his wines. He drank beer, and took no great amount of exercise; and thus the seeds of disease were slowly, and gradually, and imperceptibly sown, so that he did not observe anything wrong with him, until dropsy had already developed itself in his abdomen, which began to swell by reason of the accumulating fluid. He first noticed the swollen abdomen three months before his admission.

On Murray's admission into the hospital on the 28th of August, 1848, his abdomen was quite tense with fluid, and fluctuation was most distinctly felt. His legs, too, were cedematous. No exact information could be obtained as to the state of the liver, owing to the tension of the abdomen. There was, however, an irregular and feeble action of the heart, which led us to fear that the liver was not alone at fault in the production of the dropsy.

During the three months before his admission our patient complained only of what he called a fluttering in the region of the stomach, and a gradually increasing dyspnoea. The fluttering was doubtless a sensation caused by the irregular action of the heart.

The action of the heart consisted in a series of short systolic contractions succeeding each other at irregular intervals, and evidently of very different degrees of strength; some being felt distinctly at the pulse, others exciting no pulsation in the artery, or a very feeble one. The sounds of the heart were feeble, but uncomplicated, save by a slight bellows murmur with the first sound, as heard at the apex.

There was not much miasa with the urine; it was rather high-coloured, but did not deposit any sediment, nor did it contain albumen.

Having tried for a few days the bitartrate of potash without advantage, I determined to bring the patient under the influence of mercury, and to combine with it digitalis in small quantities, watching carefully the state of the heart, in consequence

of the weakness of that organ. He was accordingly ordered one grain of powdered digitalis, one grain of squill, and two grains of blue pill; and a few days afterwards ten grains of the strong mercurial ointment, and the same quantity of the compound iodine ointment, were ordered to be rubbed in over the region of the liver twice a day.

In the course of three weeks there was a marked diminution in the quantity of fluid in the abdomen; the kidneys were excited to very free action, so that they secreted not less than from four to five pints of urine per diem. As the mouth had become sore in about a fortnight, the pill was ordered to be taken only once a day, and the frictions were continued. With these remedies was combined pressure on the abdomen by means of a bandage.

Under the influence of these remedies the patient rapidly lost all his dropsy, and went out quite cured *as regards it*, on the 19th of October, that is, in about seven weeks; the feeble and irregular action of the heart continuing much the same. The removal of the fluid enabled us to feel the liver enlarged and indurated, but without any irregularities upon its surface.

Unfortunately, soon after he left the hospital Murray was exposed to cold, and suffered an attack of bronchial catarrh; the dropsy returned, and he was re-admitted on the 11th of November, only three weeks after his discharge. Upon his re-admission we had recourse to the same treatment as that previously adopted, with the same beneficial effects. We again applied the iodine, mercury, and pressure: and in addition to this we gave him taraxacum, with manifest advantage. All his symptoms had improved, and the ascites had almost disappeared, when he was exposed to the contagion of erysipelas, at that time raging with great virulence in the ward. He vomited several times and shivered, and had sore-throat, which was followed by an extensive bronchitis affecting the small tubes; the respiratory mucous membrane having been, I have no doubt, the seat of an erysipelatous inflammation. Under the cough, difficulty of breathing, and excessive secretion produced by this affection, he rapidly fell into a very depressed state, and died suddenly in the night.

On examining the body, we found that the liver was considerably enlarged, and Glisson's capsule was much thickened, both around and in the substance of the liver. There was great dilatation of all the cavities of the heart, especially the right; and a good deal of deposition of fat upon and among the fibres of the heart, which seemed weak and ill-nourished, giving the heart a soft flabby appearance. It is very possible, also, that these may have been fatty dege-

neration of some of the fibres; but as the heart was not examined by the microscope, we cannot speak positively upon this point. The orifices of the ventricles were both dilated, and the mitral valve was shrank and thickened at its margin, so that there can be no doubt regurgitation took place at the mitral orifice during life, which explains the bellows-sound.

In this account of symptoms and morbid appearances you have the history of a large number of cases. Either the morbid state of heart, or the chronic disease of the liver, may take precedence; or the two diseases may come on simultaneously. It is probable in this case that the liver was the first organ to go wrong; and this is indicated by the nature of the dropsy, which was mainly confined to the belly, the oedema of the lower extremities being the only dropsy referable to the heart; but this may ensue upon an ascites of long standing, even when the heart is not diseased. The heart had not yet acquired that degree of dilatation necessary to the production of extensive cardiac dropsy. The condition of this man was as follows: he had a weak fatty heart, and consequently a feeble circulation; chronic disease of the liver is established, and the course of a large portion of the blood of the body, that, namely, of the intestinal canal and the spleen, is seriously impeded. This embarrasses the heart, which, in consequence of the obstructed circulation at one point, experiences a gradual dilatation. The obstruction to the circulation in the liver determines, so to speak, the dropsy to the peritoneal cavity, and the increasing dilatation and weakness of the heart would render it general. In such cases, if the disease of the liver has not yet gone to the production of contraction of Glisson's capsule, the diet, the antiphlogistic treatment, &c. may relieve the congestion of the liver, and diminish the obstruction to the circulation, and the rest may enable the heart to recover itself somewhat; the state of dropsy may be removed, and the patient may, as in Murray's case, temporarily recover; but the occurrence of any new cause of disturbance of the circulation, as, for example, the bronchial catarrh in his case, may renew the embarrassment of the heart, the dropsy may again be determined to the abdomen by the obstructed hepatic circulation, and all the old symptoms may return, to be again relieved by a similar plan of treatment to that at first adopted.

CASE II.—The second case to which I shall call your attention is that of Elizabeth Whiteman; this, like the other, terminated fatally, and therefore we can speak more decidedly as to the exact nature of

the diseased conditions. The patient was forty-five years of age, and had had eleven children. In this case there was much more evidence of the existence of intemperate habits than in the case of the butler, to which I have just referred. As is so often the case, however, she declared herself to be a very temperate person, but admitted that she was in the habit of taking a pint and a half of beer a day, and one or two glasses of gin. About twelve years ago, being then thirty-three years of age, she threw up a large quantity of florid blood. It is difficult to determine, at this distance of time, whether the blood was vomited or coughed up—whether it was a case of hæmatemesis or hæmoptysis. The florid colour favours the latter supposition; but although in hæmatemesis the blood is generally black, it need not be so always, for it may be thrown up at once, and then it may be as florid as if it came from the lungs. The cause of the usual dark colour of the blood in hæmatemesis is, that when hæmorrhage takes place into the stomach, the blood is apt to accumulate in that viscus for some time before it is cast up, and then it mixes and is blackened by the secretion of the stomach. In the lungs, on the contrary, it is expelled immediately it is effused. Six years afterwards, however, she appears to have expectorated a quantity of black blood; and the attack of vomiting was preceded by pain in the scrobiculus cordis, and followed by severe retching. It is evident that on this occasion the functions of the stomach were very much impaired, either primarily, or secondarily in consequence of disease of the liver. Five years ago she had a severe attack of rheumatic gout, affecting nearly all her joints; and two years afterwards she had another similar attack. About a month before her admission she began to suffer from severe attacks of retching, with excessive flatulence and spasms of the stomach,—symptoms evidently denoting great derangement of the stomach, due probably to irregularities of diet and intemperate habits. At this time she also began to pass urine of dark colour, diminished quantity, and depositing an abundant brick-dust sediment. Her abdomen began to enlarge at its lower part, and she suffered pain in the loins. Shortly afterwards her legs began to swell likewise.

On her admission we found very distinct evidence of ascites in the enlarged and fluctuating abdomen. Percussion elicited a clear tympanic sound over the region of the stomach, indicating that the organ was much distended by flatus. The patient was greatly emaciated, her skin was sallow, and there was a distinct yellow hue of the conjunctivæ. This patient also exhibited

another symptom (and it is one of great interest, and not uncommon, but which was not so clearly marked in the last case I narrated to you), in a great enlargement of the veins coursing over the abdomen—the superficial epigastric veins. You know there is a communication at one or two points between the veins contributing to form the portal vein and the systemic veins; and when the circulation through the liver becomes retarded, some of the blood of the portal system is thrown on the vena cava inferior, and through it on the superficial veins of the abdomen, which, naturally small and scarcely visible, become meandering blue lines, or even prominent vessels of considerable magnitude.

In this case, the patient being a woman, we had to diagnose between ascites and ovarian dropsy, a diagnosis by no means difficult, and for which the very simple process of percussion is generally sufficient. In ascites, the percussion sound is clear in the centre of the abdomen, and dull in the flanks; in ovarian dropsy the dullness exists over the whole extent of the tumor. But in some cases of ascites the length of the mesentery is not sufficient to allow the intestines to float to the top of the accumulated fluid; and then you will have a universal dullness similar to that in ovarian dropsy. In such a case you must resort to more indirect and collateral evidence for supplying the data of your diagnosis,—the history of the case—the mode in which the dropsy commenced; if it began on one side, and gradually extended—the presence or absence of liver disease, or of some other abdominal disease calculated to impede the portal circulation.

The symptoms did not appear in any degree to yield to the treatment which was pursued. She was mesenterialized, and various diuretic remedies administered; but the urine became less and less in quantity, she was frequently sick, and she became much weaker, and sunk gradually, apparently from exhaustion and insufficient nutrition, in a fortnight after her admission.

On the examination of the body we found the peritoneal cavity filled with a yellow serous fluid. The liver was contracted, and slightly tuberculated on its surface. The capsule of Glisson was much thickened; the thickening of the capsule causing a peculiar distinctness of the lobulation of the liver. When you consider how intimately connected the portal veins are with the capsule of Glisson, how that structure accompanies and envelopes them throughout their course to their smallest ramifications, you easily understand how its thickening and contraction must interfere with the free passage of blood through these vessels. Many of the cells of the liver were loaded

with fat, but others were quite devoid of it, as if starved. The heart was small, and the kidneys were healthy, but their blood-vessels much congested.

Comparing this second case with the first, we find ascites existing in both, but in the first connected with enlargement of the liver; in the second, with contraction of that organ. In both, Glisson's capsule was altered and thickened, and thus there existed in both the condition most necessary for the retardation of the portal circulation, on which depends the ascites. You may have enlarged liver without ascites, as in the simple fatty liver; but if Glisson's capsule be thickened and altered, then ascites is produced whether the liver be enlarged or contracted. That ascites, however, is much more untractable when the liver is contracted than when it is enlarged. Some, indeed, suppose that the contracted liver is an advanced stage of the same disease which creates enlargement of that organ; but the evidence in favour of this view appears to me to rest on very slender grounds. I know of no unequivocal evidence to prove that, in any given case, the liver has passed in succession through the stages of enlargement and contraction. A similar question arises as regards Bright's kidney,—is the enlarged mottled kidney an early stage of the contracted kidney? You are doubtless aware that there are the strongest reasons for answering this question in the negative, and that there can be little or no doubt that these two different states of kidney are essentially different diseases, both producing a common effect, namely, albuminous urine. So, also, it is highly probable that the enlarged liver, with thickened Glisson's capsule, is a different disease from the contracted liver, although both produce ascites.

CASE III.—The third case, that of Sarah Gadd, *et. 66*, differs from those I have just related, in the absence of that condition which in them was the most significant and important, and, doubtless, the cause of the dropsy—the thickening, namely, of the capsule of Glisson. On her admission there was no doubt as to the existence of ascites; but the previous symptoms were by no means urgent, and the dropsy had come on without any of its usual premonitory signs. There was no sallowness; the liver could not be felt, nor was there any symptom to give information as to its condition. There was, however, the negative evidence of the character of the urine—it contained no lithates; and this was so far favourable, as it denoted the probable absence of any severe organic hepatic disease.

This patient was under treatment for a long time. We gave her diuretics, especially bitartrate of potass; applied the iodine and mercurial ointment, and pressure. Under this treatment the urine increased in quantity, and the size of the belly diminished, but not so much so as to bring to light any enlargement of the liver, or the existence of any other tumor. In spite of this alleviation of the most prominent symptoms, the woman became gradually weaker and weaker (still passing a fair quantity of water), till the night of the 9th of February, when she died suddenly. The only particular symptom that could account for her sudden sinking was a cough that she had for a day or two before her death; but this seemed quite inadequate to such a result; at any rate, the treatment could not have pulled her down, as we were giving her citrate of iron and other tonics at the time.

You will remember that I had already expressed the opinion that no hepatic disease existed; and the suspicion that some deep-seated tumor, possibly of a cancerous nature, would be found external to the liver, compressing the portal vein or some of its principal tributaries, and thus impeding the circulation through the intestinal veins, and causing peritoneal dropsy. When I heard of her sudden death, the possibility of her having some internal cancerous disease was much more strongly impressed upon me; and when we came to examine the body, we found it to exist in much greater quantity than I had at all anticipated.

The diagnosis in this case was extremely obscure and difficult, for we had no definite symptom but the dropsy; that, by its situation, denoted an impeded abdominal circulation either within or external to the liver; but we had no sign of hepatic disease. There was sufficient evidence to show that the woman's habits were not intemperate; the kidneys performed their office sufficiently, and the urine was on the whole normal, although latterly it from time to time precipitated lithic acid in the form of grains. There was no evidence of disease of any other organ, whether abdominal or otherwise. Thus, then, we had a good deal of negative evidence; and all the positive evidence was the dropsy, and its persistence against all remedies denoting a persisting cause. The deposition of lithic acid somewhat favoured the suspicion of malignant disease, but gave no positive information.

On opening the abdomen a large quantity of a transparent somewhat viscid fluid escaped. The great omentum appeared shrivelled up, and was converted into a solid cancerous mass, which adhered closely

to the bowels and to the peritoneum, on the posterior wall of the abdomen, so that it must have pressed upon the mesenteric veins, and probably also upon the trunk of the portal vein itself, so as to cause engorgement of all that part of the venous circulation of the viscera which was on the distal side of the pressure, and thus mechanically to give rise to the effusion. There was a cyst, about the size of a small orange, attached to each ovary; and the interval between each of these cysts and the other pelvic viscera was filled by cancerous growths of the same kind, so that all these organs were capable of being removed in a mass. The cancer was doubtless peritoneal, and originated in this part of the peritoneum. The liver itself was not diseased; the capsule was a little thickened, but only superficially, and not that portion of it which penetrates into the substance of the organ.

Now here was a very interesting cause of the dropsy—mechanical impediment to the return of blood from the abdomen to the liver, while the circulation of the liver itself was not at all impeded. The difficulty is to diagnose between this particular cause and the other to which I have before referred, namely, *intrinsic* obstruction of the portal circulation by disease of the liver itself; that, in the present case I would call, for the sake of distinction, *extrinsic*. I know of no unequivocal sign of this condition but that of feeling the tumor. The cachectic aspect of the patient would doubtless excite suspicion; the deposition of lithic acid, too, in a diathesis not gouty, would also increase my fears of malignant disease;—but from these points we could merely guess, and, short of manual examination, I know of no sure foundation on which to build a diagnosis. Of course the evidence of the absence of disease in other organs is valuable; and we should be greatly aided if we could detect cancer in an abdominal or pelvic viscera; and our suspicions would be confirmed if the family history of the patient showed that cancer had existed in former members of the family.

The sudden death of the patient, as I said, excited my suspicions of cancer; and the reason it did so was, that I have known several cases of cancer end in a similar way; the patient suddenly sinks when perhaps you least expect it, and without any thing manifest to account for it. This woman, indeed, had been suffering for some days from difficult breathing, caused by the accumulation of fluid in the abdominal cavity; and this, no doubt, hastened her end. But we left her tolerably well the day of her death, and it was proposed to tap her; and I scarcely think that the distension was sufficient to explain the sudden change.

It strikes me that, in these cases, a certain quantity of cancerous matter passing suddenly into the circulation may cause a depression, and that the patient may die poisoned, just in the same manner as when the blood is contaminated by pus in purulent disease. In a particular class of puerperal cases death occurs very suddenly in this way, the patient passing from a state of almost health to death in the course of a very few hours; the source of the purulent infection being the surface of the uterus and the uterine veins.

Now here are three cases of ascites which I have thought worthy of your attention, as pointing out that that particular form of dropsy—peritoneal dropsy or ascites—has its origin in obstruction in the portal circulation, although not always in the same parts of that circulation. We see, also, how it may be associated with other forms of dropsy, either when, as in the butler's case, the heart is acting imperfectly; or when the kidney is diseased; or when, from the long continuance of the peritoneal effusion, pressure on the ascending cava impedes the return of the blood from the lower extremities.

Of the causes of ascites by far the most frequent is disease of *Gilson's* capsule, or of the liver itself. 2dly. Disease of the peritoneum is not an unfrequent cause, either chronic peritonitis, or tubercular or cancerous disease of the peritoneum; but these seldom cause extreme distension of the abdomen, unless the morbid mass presses very much on the portal vein or some of its principal tributaries. 3dly. Tubercular disease of the mesenteric glands, when the disease causes such enlargement of them as to form a tumor, which compresses the mesenteric veins. 4th. Enlargement of the spleen, too, is apt to produce ascites, but seldom, I think, to a great extent, as the spleen has an extraordinary power of adapting itself to changes in its circulation; and likewise because its position does not enable it when enlarged to exercise great pressure on the other tributary veins of the porta. 5th. I have seen, also, a case in which enlarged kidney gave rise to ascites. 6th. Acute peritonitis will produce ascites; this is of a different nature from those previously mentioned, and falls more under the category of acute dropsy. The peritonitis gives rise to an undue accumulation of blood in the capillaries beneath the peritoneum, and the dropsy takes place as a consequence of this congestion or sub-inflammatory condition.

I will now call your attention to one or two conditions which may possibly be mistaken, and, indeed, have been mistaken, for ascites, the necessity of guarding against

which fallacy renders the diagnosis rather more complicated.

Sometimes we find in ovarian dropsy that there is a clear tympanitic sound over the tumor; this may be caused by the development of gas in the cyst, either through a process of decomposition of its fluid contents, or by a kind of secretion, as when the bowels generate gas, but more commonly, I think, when a communication is formed between the tumor and the bowel, and the flatus from the latter gets into the ovarian cyst. A short time ago there was an interesting example of this in the hospital. A woman having all the signs of ovarian dropsy was tapped three or four times; there was dulness all over the tumor, and there could be no doubt about the case; when suddenly the tumor became perfectly tympanitic, and, on opening the body after death, a communication was found between the ovarian cyst and the intestine, through which the flatus had escaped into it, and given rise to conditions which made the case exactly resemble ascites. If the entrance of flatus had occurred earlier in the case, the diagnosis would have been extremely difficult.

Ascites, with very tympanitic bowels, would present exactly similar signs to those present in this case.

There is another source of fallacy which may be mentioned, as it was one into which John Hunter fell. Sir Edward Home mentions that that great surgeon and physiologist once tapped a *distended bladder*, under the idea that it was a distended peritoneum. We know that patients are very apt to allow their urine to accumulate, especially when they are unable either to feel acutely, or to communicate their wants, as is often the case in fever. The only symptom of the commencement of this distension may be that the patient passes a restless night: the medical attendant, perhaps, does not look for the real cause, and by and by the bladder becomes so distended as to fill the whole abdomen. Now you can easily understand how it would be very difficult to distinguish such a state from ascites. You should always ascertain how the urine is passed; and if this is such as to leave any doubt, you should pass the catheter.

There is only one other thing that occurs to me as possibly mistakable for ascites, and that is an enormously distended stomach. To what an extent this distension may take place we had an opportunity of seeing not long ago, in a woman who died of chronic gastric disease. The stomach was so much enlarged, that it filled the whole anterior part of the abdomen, all the other hollow viscera being compressed behind and below it. Such a stomach,

filled with fluid, may produce fluctuations very analogous to those of ascites. The way to put the matter beyond doubt is to produce vomiting, and cause the ejection of all the contents of the stomach: if, then, the undulating fluid was in the stomach, the fluctuation will immediately disappear. I had an interesting case of this a little time ago in my private practice; and I ascertained the nature of the fluctuation by visiting my patient the next morning before he had taken any thing, when the fluctuation had entirely vanished.

Treatment.—You may gather from the details already given in the history of the case much respecting the proper course to be pursued in the treatment. If you can clearly make out that the ascites depends on disease of the liver and of its capsule, and if the liver be not permanently contracted, then the treatment should be directed to the reduction of the chronic inflammation which affects the capsule. With this view the use of mercury should be resorted to, and in many instances it will prove highly useful, and a free discharge of water by the kidneys will often take place simultaneously with the production of pyralism.

It is also, of course, desirable to increase the action of the kidneys by other means besides. In the cases which I have detailed to you, we used as diuretics, with variable degrees of success, broom, tassa-cum, cantharides, nitre, bitartrate of potash, lemon-juice, digitalis, squills; and these are among the best of a very uncertain class of remedies.

I also recommend your attention to the part of the treatment I have made use of with great advantage, viz. pressure on the abdomen by means of bandages and strapping. Two of the cases derived great benefit from it, especially the man; he expressed himself (and the feelings of the patient are in such a case very valuable) as much relieved.

A question often arises as to the propriety of *tapping* in ascites. The opinion that I have come to is, that the operation of tapping should be postponed as long as possible; but that that should not be until the distension is extreme. When the distension is allowed to become excessive, the danger from tapping is greatly increased; therefore, if you have tried all reasonable measures and they have failed, do not wait for extreme distension, but tap at once. The danger to be apprehended from the operation is the supervention of peritonitis, and the sinking of the patient. Now the danger of peritonitis may be very much guarded against, by keeping your patient for some time previous to, and during the operation, under the influence of opium.

Still, tapping can be regarded generally only as a palliative measure, and affords very little hope of ultimate cure: nevertheless, it is not, however, perfectly hopeless, for I can tell you of two cases of recovery in my own experience. One of the cases you may have often seen; the woman frequently comes to the hospital for other complaints. She was tapped twice, and I followed the practice I have recommended to you; but with no success. I then tapped her a third time, after which she quickly recovered, and has remained well now for five years. The other case was a man who was tapped for ascites apparently dependent on hepatic disease; he got quite well, and several years afterwards came into the hospital for another disease.

Both of these cases exhibited, to a remarkable extent, a sign to which I have already alluded as not unfrequently present in peritoneal dropsy—namely, a dilated and tortuous condition of the epigastric veins. This venous enlargement is probably compensatory, and serves to open up a new and enlarged channel for the blood which cannot be returned through the hepatic circulation. Hence I am disposed to regard great enlargement of these veins as a favourable sign; and I should be more inclined to tap where that enlargement exists than where the veins remained small.

Tapping may be performed with more hope of success when the liver is enlarged than when it is contracted; and it affords least prospect of benefit when the obstructing cause to the venous circulation is extrinsic to the liver, as when it consists of a tumor, or of chronic peritoneal disease. Upon the whole I come to this conclusion—that tapping, though a remedy not lightly to be adopted, is not one lightly to be rejected; and though in the majority of cases it does not result in an ultimate cure, yet it is not perfectly hopeless.

ASYLUM FOR IDIOTS, COLCHESTER.

DR. E. WILLIAMS, Mr. R. S. Nunn, and Mr. Tracy, have been appointed to the honorary offices of Physician, Surgeon, and Dentist, to the inmates of the above charitable Institution. Previous to these appointments, the medical superintendence of the establishment has been performed by Mr. Churchill, to whom the Board of Directors have forwarded an acknowledgment of the satisfactory manner in which he had discharged the duties of the office. It is understood that a local committee is about to be formed for managing the Colchester branch of the Institution.

Reviews.

Observations on Chronic Hydrocephalus, Acquired, Sanguineous, and Congenital; with an Account of Three Cases in which the Head was Punctured, and an Examination of that Operation. By FRANCIS BATTERSBY, M.B., T.C.D., F.R.C.S.I., Surgeon to the Dublin Institution for Diseases of Children. Pamphlet. 8vo. pp. 30. Edinburgh: Inches. 1850.

THESE "Observations" constitute a very excellent monograph on chronic hydrocephalus, pointing out the real seat of the disease, its nature, and the small success that has attended the operation of tapping the head.

Acquired chronic hydrocephalus, the author observes, is most frequently developed from the age of two years to seven. With reference to its cause, Dr. Battersby is disposed to attribute it to pressure on the veins of Galen, or on the straight sinus.

"Many authors," observes Dr. Battersby, "have considered chronic hydrocephalus as a particular form of meningitis. Even were it so,—and we are far from thinking it,—the inflammatory character of the disease is too latent, or is effaced too soon, after a certain time, for any practical advantage to follow the adoption of this opinion. It may be true in some cases; but in the greatest number the disease consists certainly in a dropsy analogous to that observed in all the serous membranes, and in the areolar tissue" (p. 5).

The seat of the disease the author points out to be the brain itself, and not the sac of the arachnoid. The several forms of the disease are distinguished by Dr. Battersby.

A very full examination of all the published cases of the operation of tapping the head is given by the author, with the following conclusion:—

"An accurate examination of ancient and modern medical works" (we think it scarcely possible to have a more accurate examination than Dr. Battersby has here made) "would very probably discover other cases; but the above authorities, along with the forty cases in Dr. West's table, give about one hundred unsuccessful cases against seven, alleged successful cases,—or, in other words, one patient in fourteen was cured by puncturing the head" (p. 29).

To arrive at this conclusion, Dr. Battersby has very minutely criticised Drs. Conquest's and West's tables, and has compared these with the results of many other recorded cases. Within his own experience the operation has been performed ten times unsuccessfully in Dublin. It is clear from the author's examination of this question that the operation holds out but very slight promise of benefit; and as it is one attended with considerable danger of inflammation, we see little inducement for its adoption, more particularly, as Dr. Battersby observes, since many unsuccessful cases are never published!

We may pronounce this pamphlet to be a very useful essay: in too many systematic treatises published, statistics are adopted without sufficient examination. We trust that future compilers will not overlook Dr. Battersby's monograph, wherein the true value of therapeutic agents, and of operative proceedings, is duly estimated.

Proceedings of Societies.*

ACADEMY OF MEDICINE, PARIS.

Sept. 24, 1850.

Vicarious Hemorrhage from the Integuments of the Face.

DR. CHRESTIEN, of Montpellier, related the following case, as in some points resembling those mentioned by M. Bousquet.*

The patient, a young lady, had never menstruated, but a sanguineous discharge had occurred for many months from the skin of the malar regions. The drops of blood, as they oozed from the surface, becoming aggregated, ran down the cheeks. If wiped away with a sponge they were quickly replaced. The quantity of blood which exuded daily was from 120 to 160 grammes (= about 4 or 5 oz. Eng). This hemorrhage had occurred at every menstrual period.

Extirpation of a Bronchocele.

M. VELPEAU related, on the part of Dr. CABARET, the following case:—

H. H., aged sixty-seven years, of a strong constitution, had always enjoyed good health until September, 1849, when he first noticed the existence of the goitre,

which increased so rapidly, that in April, 1850, it considerably impeded respiration. M. Cabaret reported that it was then of the size and shape of an ostrich egg. It was moveable above and below, but behind it was firmly attached to the deep parts. The exact limits of the tumor were traced with difficulty on account of the thickening of the surrounding areolar tissues. Pressure did not cause pain, but produced a sense of suffocation. Difficulty of breathing ensued on the slightest exertion. The voice was impaired, and deglutition was also to some extent impeded.

Various applications having been tried without any beneficial result, the removal of the tumor was undertaken at the urgent desire of the patient. Two incisions exposed the tumor, one vertical, the other transverse. The dissection of the tumor from the adjacent parts was effected by the handle of the scalpel. The carotid arteries, jugular veins, and nervous trunks, were detached by the same means. The greatest caution was required at every step. Many vessels demanded the ligature or torsion, by proceeding from below upwards every ligature placed on a considerable branch secured all its branches. The base of the tumor was found to be adherent to the thyroid and cricoid cartilages, and to the three first rings of the trachea. After its detachment only a slight oozing of blood occurred, and lasted for about two hours. During the whole time of the operation, which occupied forty-five minutes, M. Cabaret estimated that about 500 grammes (= about 15 oz. Eng.) of blood were lost. The tumor weighed 250 grammes (= nearly 8 oz. Eng). The result has been entirely successful, cicatrization having been complete by the thirty-eighth day.

M. SEDILLOT observed, with reference to this case, that he did not participate in the opinion expressed by M. Velpeau, that the publicity given to these cases would tend to encourage rash attempts at the removal of goitres. It was necessary, M. Sedillot observed, to distinguish between true bronchocele or hypertrophy of the thyroid body, and false bronchocele or cysts, partial hypertrophy, and other formations developed in its substance. The latter constitute the tumors which may be removed. M. Sedillot regarded the instance under consideration, as also that of M. Roux,* with three or four that he had himself removed, to be of this nature.

MM. ROUX and VELPEAU replied, that the introduction of these new distinctions and terms would be attended with difficulties, and inconveniences, without aiding diagnosis.

* See p. 506 of present vol.

Excision of the Astragalus.

M. GREY presented a man from whom he had, twelve years previously, removed the astragalus in consequence of its dislocation, and the impossibility of its reduction. The only trace of the operation was a hollowness in the sole of the foot, and a slight limp.

ACADEMY OF SCIENCES, PARIS.

September 23, 1850.

Staphyloraphy.

M. SÉDILLOT submitted the instruments with which he had performed staphyloraphy, and related the particulars of a case in which he had remedied, by this operation, an extensive loss of the velum palati caused by venereal ulceration.

Phænomena of Phosphene.

M. MARTINET transmitted a note in which he stated his concurrence in the view of M. Serres with reference to the diagnostic value of pressure on the globe of the eye in cases of amaurosis, and at the same time claimed priority of the introduction of this means of diagnosis, having pointed it out so far back as 1846.

Ligature of the Common Iliac Artery.

M. CHASSAIGNAC stated that he had recently performed the operation of tying the common iliac artery in a man forty-nine years of age, from the inner aspect of whose thigh he had to remove a cancerous tumor, which penetrated the pelvis by the obturator foramen. The object of tying the common iliac artery was to prevent the fatal hæmorrhage which would inevitably have attended the complete removal of the tumor. This was effected with very slight loss of blood, notwithstanding that several branches of the internal iliac and profunda arteries were divided. The patient, however, died on the following day. M. Chassaignac regarded the operation as perfectly successful, so far as the ligature of the common iliac artery was concerned, and added, that according to the best surgical works this operation has not before been practised in France.

MAGNESIA AN ANTIDOTE FOR ARSENIC.

M. LUCAS, of Beauvais, states, that in as many as nine cases of poisoning with arsenic, he has found calcined magnesia arrest the symptoms of poisoning, and remove its effects.—*Journal de Chimie Médicale*. x

Correspondence.

TESTIMONIALS TO MEDICAL MEN.

SIR,—In your last number an anonymous correspondent, dating from Bath, writing on the subject of testimonials, makes the following observations—"It has appeared to me that on several occasions, the committees authorised to decide upon the form or kind of testimonial have erred, and instead of conferring an honour and benefit upon the individual or his family, have rather taken advantage of a good name to promote some local improvement, or to serve some body of men." "In the instance of Sir Benjamin Brodie, the subscriptions of his friends and admirers were diverted to the founding of a medal for the students of St. George's Hospital; the effect being to benefit the medical staff of the hospital, rather than to gratify and honour Sir Benjamin, by presenting to him a testimonial to be handed with his title as an heirloom to his family."

As the statement with regard to "the Brodie Medal" is *entirely* untrue, I feel called upon, in justice to "the Brodie Testimonial Committee," to which I had the honour to be Secretary, to inform you that the subscriptions have not in *any way* been diverted from the original intentions of the subscribers. A gold medal, and the dye from which it was struck, were presented to Sir Benjamin Brodie, and it was left to him to do with them what he might think right; and they still remain his property; neither have the medical staff, or the students of St. George's Hospital, been benefited in *any way* by "the Brodie Testimonial," as your correspondent has asserted.—I am, sir,

Your obedient servant,

CHARLES HAWKINS.

Savile Row, Oct. 1, 1850.

* * It is evident, from Mr. Hawkins's statement, that our correspondent M.D. has been misinformed respecting the Brodie testimonial. We cannot admit that his letter displays any mischievous intention. He entertains an opinion, no doubt shared by many, that portraits and medals are not in all cases the most substantial or the most appropriate testimonials to be presented to professional men. He has been unfortunate in one of the illustrations of his argument; but we consider it far better that a statement, if unfounded, should be authoritatively contradicted, than that it should be allowed to obtain credence by private circulation.

We shall only remark, in conclusion, that the letter of M. D. was published in the Provincial Medical Journal of Oct. 2d.

Medical Intelligence.

PURIFICATION OF WATER BY ANIMALS AND VEGETABLES—BALANCE OF ORGANIC LIFE.

MR. WARRINGTON has for a year past kept twelve gallons of water in a state of admirably balanced purity by the action of two gold fish, six water-snails, and two or three specimens of the elegant aquatic plant known as *Valisneria spiralis*. Before the water-snails were introduced, the decayed leaves of the *Valisneria* caused a growth of slimy mucus, which made the water turbid, and threatened to destroy both plants and fish. But, under the improved arrangement, the slime, as fast as it is engendered, is consumed by the water-snails, which re-produce it (i. e. the slime) in the shape of young snails, whose tender bodies again furnish a succulent food to the fish; while the *Valisneria* plants absorb the carbonic acid excreted by the respiration of their companions, fixing the carbon in their growing stems and luxuriant blossoms, and returning the oxygen (during sunshine, in visible bubble streams) for the respiration of the snails and the fish. The spectacle of perfect equilibrium thus simply maintained between animal and vegetable, and inorganic activity, is certainly striking and beautiful; and experiment alone can determine how far such means may be made available hereafter on a larger scale for keeping tanked water clear and sweet.—*Quarterly Review*, September.

. This is a most ingenious application of the "happy family" principle to an important purpose. The suggestion here thrown out by the *Quarterly Reviewer* really well worthy of the attention of the Board of Health. With a sufficient number of gold-fish and water-snails, their scheme of supplying the great metropolis with rain-water from Bagshot Heath, at the rate of forty millions of gallons per day, would be perfect. According to the above experiment, one gold-fish and three water-snails have an organic purifying power sufficient to keep in a proper condition for dietetic, domestic, and manufacturing purposes, six gallons of water. Those who object to aquatic scavengers when seen through a microscope on a small scale, will probably have no objection to the general employment, by the Board of Health, of visible gold-fish and water-snails! We regret to break into this organic circle, and to suggest a doubt respecting the existence of the equilibrium alleged to be maintained by these living filtering machines: but we are compelled to ask—Do the gold-fish and water-snails impart nothing to the water

but carbonic acid? Do the excreta of these animals consist of this gas, and of nothing else? If so, it is a very curious fact in physiology: if not, something in addition to the *Valisneria* would be required for the purification of the water.

FEMALE OBSTETRICIANS.

At the Stippling Court of Justiciary, on the 3rd inst., a female medical practitioner was sentenced to ten years' transportation for inflicting injuries upon a pregnant woman, which terminated in her death.

LICENTIATES OF THE ROYAL COLLEGE OF PHYSICIANS.

At the usual quarterly meeting of the Comitia Majora, held on Monday, September 30, Dr. Peacock, Finsbury Circus, was admitted a fellow. The following gentlemen were admitted licentiates of the College:—Dr. Steavenson, Heighington, Darlington; Dr. Ogle, Hertford Street, Mayfair; Dr. Tanner, Charlotte Street, Bedford Square; Dr. Kirkes, St. Bartholomew's Hospital; Dr. Frassanges, the Mauritius. Also, Dr. Bullock, Congleton, Cheshire; and Dr. Hoggan, R.N., were admitted extra licentiates.

ROYAL COLLEGE OF SURGEONS.

GENTLEMEN admitted members on the 4th inst.:—Messrs. G. Simpson—J. Hogg—E. Simpson—T. B. H. Wood—H. G. King—E. Whitchurch—J. Wilson—J. H. Hammond—H. Thompson.

APOTHECARIES' HALL.

NAMES of gentlemen who passed their examination in the science and practice of medicine, and received certificates to practise, on Thursday, 3rd October, 1850:—Peter Pennell, Canterbury—William Gillibrand, Chorley, Lancashire—Robert Settle, Lancashire—John Darwen, Birmingham—Frederic Foster Andrews, Lynn Regis—Jonathan Hutchinson, Jun., Selby, Yorkshire—Edgar Sheppard, Enfield.

OBITUARY.

On the 30th ult., Mr. Robert Grubb, Surgeon, of Dalston, aged 55.

On the 5th inst., at Worcester, James Swift, Esq., surgeon, R.N., aged 76, last surviving son of the Rev. James Swift, M.A., of Worcester.

CASE OF POISONING WITH ARSENIC, FOLLOWED BY SPONTANEOUS GANGRENE OF THE LOWER EXTREMITIES. BY PROFESSOR FORGET, STRASBURG.

A MAN, aged 63 years, constitution, swallowed, in about 40 minutes, a quantity of arsenic, and destroyed his health.

and constitution, swallowed, in about 40 minutes, a quantity of arsenic, and destroyed his health.

at night. About an hour afterwards frequent vomiting and purging took place. Not finding death arrive so rapidly as he had expected, the patient attempted to drown himself, but was prevented. When admitted into the hospital, nine hours after having taken the arsenic, his face was pale, he was extremely feeble, his extremities cold, the pulse small and frequent. He experienced violent pain in the abdomen, and presented other symptoms of poisoning by arsenic.

The hydrated peroxide of iron was administered, and at the same time small doses of ether, with sinapiams to the extremities. Reaction followed, and was met by leeching, &c. The symptoms of poisoning had all disappeared by the fourth day. The patient, however, complained of acute pain in his left leg, which was not altered in size or appearance: it was rather cooler than the other, and tender to pressure. Two days later the limb was in a state of gangrene below the knee. The gangrene continued to spread, and the limb was amputated above the knee ten days from the first taking the arsenic.

On the following day the patient was sensibly weaker, and gangrene had shown itself on the stump. Despite the use of every means to arrest the course of the disease, the patient sank on the tenth day after the operation. Dissection of the limb exhibited—1, mortification of all the soft parts; 2, red patches on the arteries, which were obstructed by fibrinous clots; 3, a healthy state of the veins, which contained a few coagula.

M. Forget suggests that the sphacelus in this case was, most probably, in a great degree attributable to the violent reaction which followed, and which was augmented by the brandy in which the poison had been taken.—*Gazette Médicale.* X

BOOKS & PERIODICALS RECEIVED DURING THE WEEK.

Holden's Manual of Dissection. Part 3d. Contributions to the History, Diagnosis, and Treatment of Croup. By John Ware, M.D.

Pestilence, its Source and Suppression. Reports on Pestilential Cholera. By Dr. Wright.

On the Use of Alcoholic Drinks. By Henry Kennedy, M.B. T.C.D.

Epidemics examined and explained, or Living Germs proved by analogy to be a Source of Disease. By John Grove, M.R.C.S. &c.

Parliamentary Report on Prison Discipline. Comptes Rendus. Nos. 10 to 12, 2d to 16th September.

The Journal of Psychological Medicine. No. 12, October 1860.

BIRTHS & DEATHS IN THE METROPOLIS During the Week ending Saturday, Oct. 5.

BIRTHS.		DEATHS.	
Males....	680	Males....	465
Females..	728	Females..	428
	1408		893

CAUSES OF DEATH.

ALL CAUSES	893
SPECIFIED CAUSES	890
1. Zymotic (or Epidemic, Endemic, Contagious) Diseases....	195
Sporadic Diseases, viz.—	
1. Dropsy, Cancer, &c.	37
2. Brain, Spinal Marrow, Nerves, and Senses	122
4. Heart and Bloodvessels	37
5. Lungs and organs of Respiration	103
6. Stomach, Liver, &c.	67
7. Diseases of the Kidneys, &c.	9
8. Childbirth, Diseases of Uterus, &c.	8
9. Rheumatism, Diseases of Bones, Joints, &c.	8
10. Skin	1
11. Premature Birth	14
12. Old Age	23
13. Sudden Deaths	17
14. Violence, Privation, Cold, &c.	48

The following is a selection of the numbers of Deaths from the most important special causes:

Small-pox.....	10	Convulsions.....	37
Measles.....	7	Bronchitis.....	37
Scarlatina.....	31	Pneumonia.....	47
Whooping-cough.....	27	Phthisis.....	111
Diarrhoea.....	8	Lungs.....	7
Cholera.....	7	Teething.....	11
Typhus.....	36	Stomach.....	5
Dropsy.....	11	Liver.....	20
Hydrocephalus.....	20	Childbirth.....	35
Apoplexy.....	23	Uterus.....	2
Paralysis.....	20		

REMARKS.—The total number of deaths was 77 below the average mortality of the 40th week of ten previous years.

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer	30.63
" " " Thermometer	53°
Self-registering do.	Max. 60° Min. 26°
☉ From 13 observations daily. ☽ Sun.	

RAIN, in inches, .25.—Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was 3° above the mean of the month.

NOTICES TO CORRESPONDENTS.

We have particularly to request that those gentlemen who favour us with Newspapers, will either by a note or private mark indicate the article to which they wish to direct our attention. One of these rural "channels of information," sent to us last week, contains sixteen square feet of type, but there is no mark to point out the object of the writer in sending it. We regret that we have been compelled to postpone until next week the contributions of Mr. Barlow, Mr. Bate, Dr. E. J. Tilt, and Mr. Richardson, as well as the King's College Hospital Report.

Mr. Sands Cox's communication has been received.

A Lecturer on *Materia Medica*.—We shall act upon the consent of our correspondent, and forward the letter, which is written in a gentlemanly spirit, to the proper quarter. Our correspondent appears to have forgotten that the *Examiner* in question is no longer a Lecturer.

Lectures.

LECTURES

ON THE

MEDICAL JURISPRUDENCE OF
INSANITY.

Delivered in the Medical School of King's
College, Aberdeen.

BY ROBERT JAMINSON, M.D.

Lecturer on Medical Jurisprudence in the
University.

LECTURE VI.

General description of the insane state (concluded)—(c) Insane impulses (concluded)—(3) The impulse to steal—a symptom in insanity—sometimes a form of insanity—Kleptomania—distinguished from criminal theft—(4) The impulse to burn—sometimes a form of diseased mind—Pyromania—its characteristics—(5) The impulse to intoxication—medical consideration of drunkenness—legal relations of drunkenness—modifications in practice—the propensity to intoxication sometimes a disease—Dipsomania—legislation on the subject of drunkenness—(6) Morbid sexual impulses—Cythromania—(7) Unnatural impulses—(d) Physical condition of lunatics—physiognomy of the insane—the insane expression—the eye—the mouth—the ear—the complexion—the bodily physiognomy—automatic movements, &c.—odour—muscular power and endurance—insensibility to discomfort—pulse—Post-mortem appearances—Feigned insanity.

ANOTHER impulse originating from disease, which is of frequent occurrence among the insane, is the impulse to steal. I have frequently known it to be one of the earliest symptoms of insanity. A medical student, a quiet orderly fellow, suddenly took to drinking porter and stealing books, making no particular attempt to conceal either the one propensity or the other, but just as he was becoming an object of interest to the fiscal, a paroxysm of mania broke forth in full force, and the lunatic asylum was made his prison. Unmeaning theft is a common enough symptom in mania. A poor country labourer, much deformed in his person by rheumatism and hard labour in peat mooses, and of a reserved and dispirited cast of disposition, became afflicted with mania, in which was manifested neither delusion nor incoherence; indeed, there could be said to be no indications of intellectual disorder unless a more rapid flow of ideas than was

natural to him; the sole symptoms, in addition to physical disturbance, were unnatural cheerfulness, excessive talkativeness, Munchausen-like lies, and a most remarkable propensity to thieve. It did not matter what he came across; if it was small, it was pocketed; if it was ostensible, it was swallowed. When he washed himself in the morning he generally managed to finish the operation by pocketing the soap and hiding the towel. If a patient fell asleep in the airing yard or at the fireside, he was on the watch for an opportunity to plunder him. If a pair of stockings was missing he was sure to be found with two pairs on his legs.

The propensity to steal sometimes occurs as a disease uncombined with intellectual derangement, or with unnatural excitement, as in the above case; it then forms a variety of moral insanity, and has been written of under the name of *kleptomania*. Some have ascribed at this disease, but the fact of its being at times a premonitory indication and a phenomenon in maniacal delirium, forms an argument for its probable occurrence as a morbid impulse. The propensity to acquire being a natural one, constantly stimulated by the temptation of opportunity, controllable and requiring to be controlled, and the punishment of theft not being like that of homicide, extreme and irremediable, the proof of disease ought to be very clear before such a plea is allowed to have weight. Kleptomania may be presumed when the theft is the act of an individual not of a vicious character, not impelled apparently by the ordinary motives of the crime, who has hereditary predisposition to mental disease, who has formerly been insane, whose natural disposition has suddenly or gradually become changed by fever, nervous disease, injuries of the head, and by such conditions as pregnancy, or disordered menstruation, or who manifests other traces of disordered mind immediately subsequent to the act. The circumstances of the theft must also be taken into account along with the character and history of the offender. The impulse should be motiveless and irresistible. The plea will be strengthened if the articles are heterogeneous, of less trifling value than there was opportunity to seize, appropriated without guile or attempted concealment, unsuited to the wants, the condition, the tastes, the sex, of the individual, not sought to be turned to any account, unheeded after they are possessed, and not missed when returned to their owners. Lavater tells of a physician with this affection, who daily robbed his patients of such articles as spoons, scissors, keys, and watchable gear; he never made any use of these things, nor seemed to miss

them, though they were daily returned by his wife. A crate may be apparent in the nature of the articles collected: for example, an agent of the government at Vienna, we are informed by Dr. Gall, had a mania for kitchen utensils: he stole nothing else but only pots and pans, which he stowed away in a couple of rooms hired to hold the collection, and there they remained unused and unsold. I have heard of kleptomaniacs, but have never met with any in whom the mental faculties were not clearly deranged.

The next diseased impulse to which it is necessary to allude, is the *incendiary impulse*. The propensity to burn is not amongst lunatics a mischievous bent remarkably more common than other forms of destructiveness, and would not require special attention, were it not that there are grounds for believing that such an impulse may be a result of mental disease, when there is no evidence of disturbed understanding. I have occasionally known incendiary acts to be performed by the insane; the nearest approach to simple morbid impulse was in the case of a supposed criminal, in whom the fire-raising, which led to the apprehension of the individual, proved to be a forewarning of insanity. The incendiary impulse, as a variety of moral insanity, is usually termed *pyromania*, and is a disease recognized by all the best authorities in medical psychology. According to Esquirol, there are facts sufficient to prove, that certain incendiaries are moved by an instinctive impulse independent of their will, the burning being the result not of delirium, delusion, nor want of reason, but yet of a diseased motive which may be uncontrollable. A German authority, of the name of Hencke, was struck with the frequency of incendiarism by young individuals, and propounded the view, that fire-raising is in many cases to be attributed to the condition of the system at puberty, and is a disease related to the mental disorders and convulsive affections which sometimes make their appearance with the sexual development, more particularly in females. The propensity has been observed in connection with amenorrhoea, imperfect sexual character, natural imbecility of mind, epilepsy, and other nervous disorders allied to insanity. The plea of pyromania has been admitted by juries in this and other countries. For examples, I refer you to the later writers on insanity and forensic medicine. In such cases the act is motiveless, probably repeated without caution or self-protection, on various dissimilar occasions, by an individual about the age of puberty, who has a known tendency to mental disease, who is subject to some convulsive disorder, or who

in most cases is of the female sex, and labours under some disease or disturbance of function in the sexual system.

The *impulse to intoxication* is often a very prominent feature in the moral disease of lunatics. Drunkenness may be a cause, a forewarning, a complication, or a consequence, of diseased mind. Insanity is a disorder in which there is an unusual craving for stimulants of all kinds.

Medico-legal consideration of drunkenness.—The state of intoxication is not an exculpatory plea in law, nor does it constitute such a state of unsoundness of mind as to invalidate civil acts. Though it must be admitted to signify a condition in which emotion is perverted, and reason and judgment not sanely exercised, still, as it is a form of unsoundness voluntarily induced, the individual is held to be responsible for actions committed. A habit of drunkenness does not afford legal grounds for placing a person under restraint or confinement, or for interdicting him from the management of his own affairs. In Scotland, one who from habitual intoxication is liable to be imposed upon and taken advantage of, may, if he choose, inhibit himself, as it is called,—that is to say, he may put himself under trustees or curators, without whose sanction no act of his shall be valid. Any confession which an accused person may unguardedly make when in a state of intoxication may be used as evidence against himself or others, having no less weight than the voluntary disclosures which he may proffer in sober moments. No one is called upon to criminate himself; but voluntary statements, whether sober or drunken, though they may be afterwards denied, cannot be nullified as evidence by any retraction. Such is the general statement of the law; but there appear to be occasional modifications in practice. A distinction is made between partial and complete drunkenness; neither is understood to afford an exculpatory plea for criminal actions, though the latter might be with propriety urged in mitigation of punishment. Extreme intoxication, such as interferes with personal consciousness, has been held to vitiate civil acts; and, no doubt, lesser degrees would be similarly protected if the unexpected result of some accidental and unknown physical condition rendering the individual unusually sensitive to the influence of stimulants: so, also, if the state were apparent to all, and brought about by the contrivance and collusion of evil-intended parties for the special purpose of obtaining an advantage. As the law estimates the degree of culpability more from the intent than the effect of an action, it may often be of importance to plead the

state of drunkenness as lessening the presumption of malice.

The propensity to drunkenness as a morbid impulse may manifest itself independent of intellectual disorder; in such a case it constitutes the variety of moral insanity usually referred to under the name of *dipsomania*. The possibility of drunkenness being at times itself a disease, seems indicated by several circumstances. There are pathological relations between intoxication and insanity; the former sometimes passes into the latter, and more frequently still forms a predisposing or exciting cause of mental disease; the latter often unexpectedly develops the propensity to the former; a state of dipsomania suddenly appearing is not unfrequently a leading symptom in the premonitory stage of insanity. Drunkenness is often a consequence of physical organization,—an original sin of the constitution, hereditary, and partaking of the ordinary characters of hereditary disease. It is also sometimes a periodical disorder, and in all respects incompatible with the habits of the unfortunate sufferer during the intervals. Habitual drunkenness has been known to have been induced by alterations of disposition following diseases and accidental injuries of the head. Cerebral irritation from continued or intense mental emotions also occasionally compels to it. The following, with other similar cases, is narrated by Dr. Ray:—A young man, a sailor, was, for his sobriety and good conduct, elevated to the command of a ship; “but,” says the writer, “no sooner did he reach this reward of his merits, than he began to drink with all the recklessness of an old toper. As soon as he was degraded to an inferior station, no man could be more temperate; and this appearance of reform each time encouraging his friends with the hope that he had abandoned his bad habits altogether, they would restore him to the station he had lost, to be again and again forfeited by his mad propensity.”

The propensity to intoxication, when dipsomaniacal, does not follow temptation or opportunity, is contrary to habit, hereditary, conjoined with predisposition to insanity, follows some special, mental, or physical cause, is sudden, or connected with antecedent change of disposition and character, irresistible, periodical, and solitarily indulged during a lengthened unremittent paroxysm.

Enactments on this subject have varied much in different countries and times. Sometimes drunkenness has been entirely overlooked by legislators; in some cases it has been admitted as a plea in extenuation of crime: it has been punished by fines, imprisonments, and loss of civil status, and

even, under special circumstances, by death. At present, in this country, it does not appear to be sufficiently recognized either as a crime or as a disease. Legislation on the matter would be, no doubt, difficult, but not more difficult than important and useful. It would be well for society that the habitual drunkard should, in many cases, be interdicted from the control of his affairs; and it would very much lessen the labours of judicatory lords, were dipsomaniacs allowed to be put under control and restraint, subject to every inspection desirable for the prevention of abuse. Such individuals should be placed beyond the power of falling into criminal acts, or be made liable only to a mitigated punishment for crimes which legislation is careless to prevent.

Disease of the sexual impulse is not uncommon amongst the insane. It may occur as a form of moral insanity, and give rise to medico-legal difficulties. Under this head would be included the nymphomania and satyriasis of nosologists: these and such like might be comprehended, for uniformity's sake, under some such term as *cytheromania*; reserving the term *erotomania*, which has sometimes been made use of, to represent that species of monomania or of moral insanity in which the sentiment of love is insane.

Lastly might be specified unnatural impulses, which are occasionally combined with insanity of intellect. It is probable that even where this is not the case they may be attributed to disease as often as to brutality. In the words of Dr. Prichard, “they have given rise to a series of phenomena in human actions which have been considered to belong to the province of the moralist, or the enactor of penal chastisement, rather than to that of the medical philosopher. That this opinion has been founded in error,” says he, “we are fully convinced; and we doubt not that the time will come when the very names of many offences against decorum now considered as punishable crimes will be erased from the statute-book, and when persons now liable to be sentenced to the pillory or the gallows will be treated as lunatics.”

The physical condition of lunatics.—In continuation of the picture of the insane state, and in conclusion of it, I shall now advert to the physical condition of lunatics, directing your attention first to the peculiarities of the lunatic countenance.

The physiognomy of the insane.—Well established facial characteristics of disease are of great diagnostic value, affording pathognomonic indications which no endeavour of the patient can conceal, and which enable those who have profitably studied the expression of disease to some to

a fixed and correct conclusion upon a slight examination in instances where one less skilled might, after more laborious proceedings, continue in a state of uncertainty. Physiognomy as a science of character is a most vague and fanciful affair. It assumes, however, a more practical value in states of sickness, in which we do not look on the countenance as an index of disposition, but as an exponent of accidental and morbid psycho-physical sympathies. Expressions of disease, though less instinctively understood, are, when once known, more to be trusted in than expressions of thought or emotion; and when these last are not natural, but morbid, they are, being more involuntary, less calculated to mislead. The knowledge to be acquired from the appearance of the countenance is of greatest value in those temperaments in which there is most reaction between the mind and the body, and in those diseases which are most likely to induce mental anxiety and distress, or to occasion actual sympathetic disorder of the mind. "The study of the physiognomy is more essential in mental derangement," according to Dr. Burrows, "than in almost any disease. It not only guides us in doubtful cases, and instructs us to ascertain a hereditary predisposition to insanity, but also warns us of its approach." It may enable the medical jurist to come to a ready determination in cases of simulated insanity, to distinguish the approach of the disease, the presence of a lucid interval, and the validity of recovery.

What is the peculiarity in the expression of insanity? Painters and others have represented it either as simple exaggeration of emotion, or a jumble of incompatible characters. Sir Charles Bell conceives that it is a brutalized type of expression. Sir Alexander Morison, who has given much attention to the moveable physiognomy of the insane, seems to think that it is a combination of wildness, abstraction, or vacancy, with the ordinary expression of the predominating emotion. I believe that Sir Charles Bell's opinion is a correct statement; the expression of insanity is always degraded and repulsive. I refer you to the drawings and casts before you. How inferior is this excited aspect to any natural expression of mirth, self-confidence, or excited expectation! How different this suspicious look of ferocity from the coarsest natural expression of anger! Here is a monomania of pride;—distortion, not dignity, not even a ludicrous, but only an ugly caricature;—here the monomania of fear—the aspect and action of a terrified cur.

The eye is in mania an unnaturally brilliant, quick, unsettled, and projecting eye, with usually a contracted pupil. Here

is what writers term the "*oculus bovinus*"—a wide, dilated, and prominent orb, with an exposure of the whole circle of the iris. In melancholy the organ is dull, slow, downcast, and evasive. In dementia it has lost much of its natural brilliancy of expression; the pupil is dilated, the eyes droop, and the supercilia are drawn up on a low wrinkled brow, giving an appearance of stupid amazement. In cases where intemperance has been at work producing the disease, the eyes wear all along the intoxicated expression.

Many characteristic traits are to be observed about the expression of the *mouth*, especially in dementia and imbecility. It would be difficult to convey an accurate idea of them in verbal description; you will remark them in the examples before you. There is a peculiar relaxed expression very characteristic of primary or acute dementia, which contrasts very remarkably with the natural expression of the countenance as represented after recovery.

It is a curious fact, that there is a certain conformation of the external ear which is occasionally to be observed amongst the insane. To this my attention was first directed in the Hanwell Asylum by Dr. Conolly; but I believe the observation was originally made by M. Foville, who has also in addition remarked, I understand, some relations between the configuration of certain portions of the organ and the condition of the brain. These may be fanciful; but true it is that changes do take place in the shape of the ear, especially in cases of old standing: the lower portions of the pinna become enlarged, misshapen, and more vascular, while the helix and antehelix seem flattened and defective. These changes would point out some as yet but indifferently understood relationship between the condition of the cerebrum and external ear, which there are some facts to confirm: for instance, we are told by Kirby, in his "*Habits and Instincts of Animals*," that in the horse the condition of the ear in a certain particular varies with the wildness or tameness of the animal,—with its state of cerebral education in short. In the wild horse the ears lie back; in the domestic or cultivated one they are erect. Every representation of the trained animal, however ancient, as on Grecian and Egyptian sculptures, exhibits it with the ears erect; whereas in the wild horses of Tartary, and those which have run wild on the mountain plains of South America, they are uniformly recumbent.

Finally, as regards the facial appearance of the insane, you will find that in very few instances indeed is the *complexion* of a lunatic unexceptionable. In mania, when the disease is recent, the countenance will

sometimes bear the appearance of health ; but in all other forms of non-congenital mental derangement, and wherever the disorder is of any standing, the skin has either a pale and leuco-phlegmatic hue, or it is sallow, dirty, and badly coloured. A clear condition of the skin you will very seldom observe amongst lunatics. Beauty is what you will fail to discover in a lunatic hospital: the physiognomies are lined by the expression of diseased feelings, which expression is seldom such as to inspire sympathy, and never admiration; the trace of every benevolent emotion is too frequently banished from the countenance, giving place to cunning, suspicion, timidity, outrageous passions, and unworthy propensities.

The *bodily physiognomy* of lunatics, or their carriage, attitude, and gait, is also characteristic, as you will see from some of the graphic representations before you. On this point I shall no farther dilate than to say that the maniac may show during paroxysms of excitement more than his natural degree of grace in his frantic movements; the melancholic has an external appearance of slowness, shrinking, and oppression; the monomaniac with a high delusion expresses his ruling passion in a caricature of its natural language; and the positions of the imbecile and demented are usually degraded and debased.

The incurable class of the insane often exhibit a variety of *automatic movements*,—that is to say, certain habitual involuntary or semi-involuntary actions of an unvaried monotonous kind, such as an automaton might be contrived to perform. Such a symptom does not, however, of necessity indicate an incurable form of disease. These convulsive and habitual movements have received the name of *tic* from the French writers on insanity. It would be futile to attempt enumerating them, they are so various. I may mention here a curious action confined to paralytic lunatics—namely, a grinding movement of the teeth against each other, productive of a sound much louder than any you would conceive could be produced in this way. The sound is very loud, and in a room where two or three such patients are congregated together I have heard it absolutely amount to a distressing noise. It is a curious symptom, but speedily put a stop to by death. There is another phenomenon still more singular occasionally manifested by incurable lunatics, and that is rumination—a symptom which has sometimes, however, been known in dyspeptics not insane.

Insanity is frequently conjoined with a peculiar odour of the perspiration and the

breath. This fact looks odd, but many other conditions have a connected and peculiar effluvia. There is a peculiar smell which often precedes death, and which is an absolute indication of its approach—a sort of earthy odour prophetic of the church-yard. Gout and rheumatism have also a peculiar fetor; and the disease called the *pellagra*—a combination of a mental and a skin disease which occurs in Italy—is said to be distinguished by a smell like mouldy bread. The smell of mania is described as being quite *œt generis*: in some books you will find it compared to the scent of hyoseyamus in a state of fermentation. It is not confined to any of the particular forms of mental derangement. “The maniacal odour,” says Dr. Burrows, “is not always present; but I consider it a pathognomonic symptom so unerring, that if I detected it in any person I should not hesitate to pronounce him insane, even though I had no other proof of it.” Such an opinion from an authority so respectable makes this occasional symptom of mental disorder deserving of notice. A person sensitive in this way will almost always be offended by the breath of lunatic patients. All are agreed as to the insane being liable to be the subjects of a very powerful and disagreeable odour, whether special or accidental. It is remarked by all attendants, by all who have to deal with the repairing and washing of their bedding and clothes. In an old, ill-constructed, and badly ventilated asylum, I was once witness to two female attendants having their stomachs fairly upset by it in the morning, as they were engaged in unlocking the doors and opening the shutters of a gallery of sleeping apartments.

Mental alienation is a disorder related to physical debility, yet, during paroxysms of excitement, lunatics often manifest wonderful *muscular power and endurance*. Young and delicate individuals will perform almost incredible feats of strength, and continue for days in a state of unceasing motion and exertion of voice and limb, with perhaps little or no sustenance, continued want of sleep, and in spite of all the means which may be employed to depress their strength and calm the violence of their excitement. It is only, however, during a period of maniacal exaltation that the insane manifest an increase of muscular power: they are ordinarily of less than average vigour. Such an appearance of strength is in a considerable degree fallacious, being in part attributable to diminished or perverted corporeal sensibility. Indeed, when the bodily organs themselves make it clear that they have been exerted

far beyond the legitimate extent, the maddened brain will still impel them to obey its destructive mandates. You will see maniacal patients, after they have from unceasing vociferation shouted themselves into a state of inarticulate aphonia, persevering in their vocal attempts till at length they are prevented by fatal exhaustion. Epileptic lunatics have generally an augmented degree of strength during their periods of excitement. Demented patients, unless when epileptic, are usually feeble in body. When melancholics and monomaniacs exhibit much strength and endurance, the exhibition of it is attributable to blunted sensibility. In such a case they may be capable of undergoing great fatigue in obedience to their delusions; but when the will is exerted in a healthy way, in furtherance of any other object, they exhibit not merely a small capacity of attention, but a slight power of exertion and endurance.

The insane frequently manifest much insensibility to external impression, and much endurance of want of sleep, cold, hunger, and discomfort. This is an evidence of anæsthesia, not of strength or robustness. They may actually suffer physically, though they do not express themselves as sensible of suffering; the condition of insane abstraction from surrounding circumstances, and the absorption of attention by delusion, serving to deaden all natural impressions. A knowledge of such particulars is of importance for the discrimination of feigned insanity.

Dr. Rush, we are told, decided upon the insanity of a criminal from the single fact of an excited pulse. Now, as regards the pulse, it is not much to be trusted in determining mental disease. In the insane it may be found varying from 60 to 120. The most common pulse in lunatics is one which is not at all indicative of any state like acute inflammatory action: it is a soft and indistinct pulse, between 80 and 90, and easily quickened.

There are certain peculiarities in the constitution of the blood, urine, and secretions, which possess no little interest, but which we must pass over, as they would lead to a discussion upon the pathology of the disease beyond our province. I shall just briefly enumerate certain of the *post-mortem appearances* that are frequently found in the bodies of lunatics. These are, altered thickness of the scalp and calvarium; opacity, thickening, and adhesion of the meninges; effusion of serum into the subarachnoid tissue, the sac of the arachnoid, and the cavities of the ventricles; atrophy of the cerebral convolutions; redness or atrophy, and increased density, of

the vesicular nervous matter; diseases of the heart, particularly hypertrophy of the left ventricle; diseases of the lungs, particularly phthisis; diseases of the abdominal viscera, and, as a peculiarity, displacement of the colon. No definite relation has been established betwixt these or other morbid changes and special forms of mental disease. They do not constitute the disease, but are allied with it, sometimes as causes, sometimes as consequences, and sometimes as mere accidental complications. The last appearance alluded to is most common in cases of melancholia, and was first taken notice of by Esquirol. He observed that the colon, instead of pursuing its transverse course from the right to the left side of the abdominal cavity, was sometimes suddenly precipitated and tied down behind the pubis, or deflected from its course at the transverse portion in the form of an inverted arch of greater or less extent, at one time being a slight curve, at others a loop so long as to fall into the pelvis. Most of these post-mortem appearances are best marked in old cases. It often happens in recent cases, and occasionally, too, in old cases, that no pathological alterations whatever in the brain can be discovered either by the knife or the microscope. There is no mode of investigating insanity so discouraging as that by post-mortem examinations, and few have persevered very long in the attempt to elucidate its mysteries in this way.

You have now had laid before you a description of all the features which are characteristic of the insane condition—at least, all with which it appears necessary that the medical jurist need be acquainted in order to expose feigned insanity, and to give sound opinions in cases of judicial difficulty.

On the subject of *feigned insanity* I think I may venture to be very brief. I need not lay before you the objects which may lead a person to feign madness: but bear in mind that a person may be suddenly attacked with insanity under some of the ordinary circumstances which tempt persons to feign the disease. For example, the excitement accompanying the commission of a criminal deed, the remorse which may follow its execution, or the shame of discovery, might occasion a real attack of madness liable to be canvassed and dealt with as a feigned lunacy, owing to the suspicious circumstances in which it had been developed. All forms of insanity are difficult to be feigned, and easy to be detected when attempted to be simulated. The general rules for the detection of feigned disease are applicable to the detection of insanity. I need not take up your

time describing how the imposition of imbecility, maniacal excitement, fixed delusion, or morbid impulse, is to be exposed. The history of the individual, his character, the history of his crime, the aspect of his symptoms, are all available. Every circumstance mentioned in describing the lunatic condition has a practical application to the discrimination of feigned insanity. The true lunatic has peculiarities of physiognomy, attitude, action, and appearance: he has certain distinguishable and broadly marked emotional and intellectual symptoms, which would require to be long and perseveringly studied to be successfully imitated by even the most accomplished actor, and there are pathological features of madness which cannot be assumed. I do not think that there are many diseases whose subjective symptoms it would be more difficult to feign. In most instances, a feigned lunacy would be at once suspected, even by the inexperienced, and in every instance distinguished by those well acquainted with the characteristics of the disorder. Those who do not know insanity from having personally observed it seem to conceive it badly. Dramatists and writers of fiction, excellent depicitors of natural character, have not in general been very successful in their delineations of insanity.

SCHENBEIN'S RESEARCHES ON OZONE.

M. BECQUEREL has laid before the Academy of Sciences, Paris, the following experiments and observations on ozone which have been communicated to him by Dr. Schenbein:—

A supply of ozonized air may be procured by placing in a globular glass vessel of from ten to fifteen pints capacity, and accurately closed, a small quantity of water which about half covers some pieces of phosphorus about a centimetre (≈ 393 Eng. inches) in diameter. After some time, the glass vessel is to be inverted in a tub of water, so as to withdraw the pieces of phosphorus, and the ozonized air is to be agitated with water, in order to wash it thoroughly: the neck of the globe is then to be stopped with a cork which shall admit of the passage of two tubes, one to permit the admission of water, the other to allow of the exit of the ozonized air.

Under the preceding conditions, ozone is formed by the contact of the vapour of phosphorus with oxygen and aqueous vapour. It is not formed at the same temperature in dry oxygen, nor in moist pure oxygen at the ordinary pressure of the atmosphere; but its formation takes place under diminished pressure, or when the temperature is raised to 70° Fahr.

The presence of certain other gases, as hydrogen, nitrogen, or carbonic acid, have the same effect as the elevation of temperature, or the diminution of pressure.

Ozone is not formed in moist air at the freezing point, but takes place rapidly at an elevation of six or eight degrees.

Increased density of the air is opposed to the production of ozone, as also are certain gases, as olefiant gas and nitrous vapour.

When ether vapour is slowly burnt in air or in oxygen, among other products a compound of ozone and olefiant gas is formed.

By passing ozone through a tube heated to about 600° Fahr., it is completely destroyed.

Ozone is insoluble in water: it is destructive to the life of small animals when these are placed in an atmosphere strongly charged therewith: it destroys colours, also ligneous and albuminous matters: it combines chemically with chlorine, bromine, and iodine, in the presence of water, forming acids. Atmospheric air charged with ozone and exposed over lime water forms nitrate of lime.

Ozone in the nascent state, in contact with water, nitrogen, and a strong base, generally produces a nitrate of the base. A small portion of nitric acid is produced by the slow combustion of phosphorus in atmospheric air.

Ozone acts powerfully on most metals, causing them to pass into the highest degrees of oxidation: it combines directly with olefiant gas without decomposition: it destroys sulphuretted hydrogen, seleniuretted hydrogen, &c., and changes nitrous and sulphurous acids into nitric and sulphuric.

Ozone precipitates oxide of lead from alkaline solutions, or from acetate of lead: it rapidly decomposes salts of protoxide of manganese, whether solid or in solution, producing peroxides: whence it results that a strip of paper impregnated with sulphate or chloride of manganese serves as a reagent for the detection of ozone, by its becoming rapidly changed to a brown colour. The same occurs to starched paper moistened with iodide of potassium.

A solution of yellow ferrocyanuret of potassium is changed by ozone into the red cyanuret.

Ozone, according to Schenbein, is the most powerful oxydizing agent in nature: it is produced in the air by electrical changes, most abundantly in winter, especially during a fall of snow.—*Comptes Rendus*, 1850. X

Original Communications.

A SUPPLEMENT TO
SOME OBSERVATIONS ON THE
MUSCULAR CONTRACTIONS
WHICH OCCASIONALLY HAPPEN AFTER
DEATH FROM CHOLERA.

Containing a note of a case of Apoplexy, after which similar Movements were remarked.

By WM. FREDERICK BARLOW, M.R.C.S.
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Hospital.

THE more attentively the subject of post-mortem contractions is considered the more worthy of study will it appear; and it is really a fit matter for astonishment that it should so slowly have attracted the thoughts of inquirers. Like every other topic actually submitted to contemplation, it amplifies as one views it; what once seemed peculiarities take another shape, and the air of strangeness which it wore at first sight fades gradually away. Phenomena gather more thickly, are more illustrative than might be imagined. If observers would but take the trouble of fairly and intelligibly recording all the facts which touch upon this interesting question, we should soon obtain a history which would do much more than repay perusal; but some have thrown away the opportunities which have occurred to them of illustrating the inquiry, or have so dimly, so superficially observed, that their remarks are of but trifling value.

In the present state of the question facts are yet acceptable. I have been indebted to Mr. Kesteven for the following particulars:—"In 1832 I attended a gentleman, aged 42 years, who died of cholera in fourteen hours. After death the muscles of the lower jaw continued to act for two hours: the jaw was not moved as in speaking, but the mouth simply opened and closed at regular intervals, which gradually lengthened. In the same body the muscles of the abdomen were observed to have a tremulous movement. The muscles of the thighs also, more particularly the sartorius, were in action; they contracted in spasmodic twitches, but not sufficiently strongly to raise the limbs, though these were slightly moved."

This is clearly to be placed amongst the most striking class of cases. The movements of the jaw remind one of what is sometimes witnessed in the heads of decapitated animals. The time of their continuance deserves particular notice. I believe that contractions of this kind endure seldom so long as two hours; but one ought to speak with deference on this point, because future inquiry may find them to be renewed after a longer period of cessation than we are now acquainted with. The form of the motions was peculiar; and they appear to have been rhythmic, or almost so, and unlike any voluntary action. I have noticed rhythmic movements in insects, from which I had removed the heads, that I think should be classed with such as these. The intervals between the movements lengthened: this circumstance probably indicated, and was caused by, diminished irritability of the muscles. The tremor of the abdominal muscles observed here has not been, so far as I know, nearly so frequently seen as that of the muscles of the legs, arms, chest, and face; for what reason I cannot tell.

Dr. Risdon Bennett some time ago informed me of a case in which the respiratory muscles seem to have been spasmodically affected after indubitable dissolution; and I refer to it on the ground that contractions of those muscles would be especially likely to delude some persons as to the fact of dissolution being absolutely real.

Amongst my own too many omissions I regret to have made no special observations as to the time of occurrence, degree, and duration of rigor mortis, in those cases wherein post-mortem movements or quiverings were seen: these points require investigation.

Inquiry as to the event of post-mortem contractions should obviously not be confined to cholera; so to restrict it would be at once to narrow and mystify the question. We need a knowledge of all the circumstances under which they occur. Some writers would imply, by the way wherein they mention them, that they have only been noticed after Asiatic cholera; but this is a mistake, and needs well pointing out, inasmuch as it might lead a person to suppose that the poison of cholera of this type, whatever it may be, or the peculiar state of blood occasioned by it, is necessary to their production.

It may reasonably enough be made a question whether similar movements do not ever happen after English or common cholera.

A fatal case of this form of disease occurred this autumn in the Westminster Hospital. It was extremely severe; the temperature was strikingly diminished; and, but for the *bitious* dejections, it might have been called Asiatic, if we may still apply that epithet to a malady, now, most unhappily, European also. The patient, a man *et. 40*, died, was laid out in the usual way, the arms being placed straight by the side. In about an hour the arms were found bent at right angles, one hand being placed upon the chest, and there was complete rigor mortis.*

There are two points of interest here: first, the early setting in of rigor mortis; secondly, the remarkable change in the position of the arms. What was the latter owing to? The most probable explanation that can be offered is, that first they were moved by muscular contraction, such as depends upon irritability, and then were fixed in their changed position by the quick supervening of rigor mortis. If this explanation be rejected, it must be attempted to account for the altered posture by referring it to the rigidity of death. Now there is nothing in the present state of our knowledge, so far as my information goes, which would justify us in so referring them. Nysten went the length of stating that "the position of the limbs always remained unchanged" by rigor mortis; and who has treated of this phenomenon more forcibly, more delicately, more beautifully than he? His authority may be cited against such a notion. Nor is there anything in the opinions of M. Sommer, as stated by Professor Müller, which would warrant the conclusion that so marked a flexion as that noticed in the case before us could be satisfactorily explained in the manner mentioned. Let me, availing myself of Dr. Baly's translation, lay the passage alluded to before the reader. M. Sommer states "that a *slight* motion takes place when the muscles become rigid. He affirms that Nysten was incorrect in stating that the position of the limbs always remains unchanged.

The lower jaw, he says, though it be separated from the upper jaw at the time of death, becomes afterwards firmly drawn up towards it. The extremities also become more strongly flexed: thus the thumb is drawn towards the palm, and even the fore-arm is a *little* bent on the arm."†

I have ventured to put two words in italics; and I think that, admitting M. Sommer to be right, and Nysten to be in error as to the point disputed, we cannot properly conclude that the flexion in the example stated was, looking well to the *degree* of it, produced by the rigidity of death. If it really were so, it is quite clear that so extreme an effect of the phenomenon is unusual.† I should have dwelt thus far upon the change of posture were it only for its physiological import; but it has also a most practical relation to medical and medico-legal inquiry; for it is very plain that such alteration of position might lead to the conclusion that a person had been pronounced dead before he really was so, to the prejudice perhaps of his medical attendant, and the discomfort of his friends, who might even imagine that some chance of saving life had, owing to ignorance or inattention, been thrown away.

It is to be regretted that the body in the case narrated was left unwatched between the time of death and that of the coming on of rigor, for it would be important to be able *certainly* to state that muscular contractions had occurred. As it is, I can merely affirm my belief that they most probably did happen, and that it will very likely be one day established that such contractions may occur after rapidly fatal English, as well as after rapidly fatal Asiatic or epidemic cholera, and that no distinction between these affections or forms of affection can be based rightly upon them.

It is to be supposed, since cases differ greatly, and also the subjects of them, that post-mortem muscular contractions may be more common in some epidemics than in others: and I have been informed that they were not so frequent in Paris during the late visitation as in a former one.

In relation to post-mortem muscular

* This case happened during my absence from town. I owe the account of it to Mr. Kidd, and notes taken by Mr. Morris.

† See Elements of Physiology, p. 890.

† I have made a few remarks on the alteration of the position of the dead in a former contribution on the Condition of the Body after Death by Cholera. See MEDICAL GAZETTE.

contractions in general there is nothing of higher importance to determine than the various circumstances under which they may happen. When are they most likely to be observed? When most likely to be observed most strikingly? What does pathology, what does experimental physiology, say respecting them? How is it that they were only first observed since cholera in a malignant form was known to us?

Dr. Dowler, whom I have referred to in previous remarks, on account of the importance of the facts he has narrated, has put on record a well-marked case wherein these contractions happened after yellow fever—a disease, it will be remembered, in which he has shown repeatedly how readily muscles may be excited to contraction after life is over by means of percussion.

What, more especially, are the forms of sudden or rapid death, for I suppose they are quite limited to such dissolutions, in which these contractions, if seen at all, would probably be found most violent and most protracted?

I have referred already to two instances of muscular quivering which were remarked on post-mortem examination: one of them was a case of tetanus. In this affection the disposition to spasm is, despite exhaustion, extreme unto the end, and there are manifestations of irritability when the general powers are too obviously declining.

I find the following passage in a case of disease of the spinal cord described by the distinguished Bellingeri:—"The spasms disappeared for a short time, and returned with greater severity, so that death appeared to have been produced by asphyxia, owing to a spasmodic action of the glottis. Immediately after death the left arm was drawn spontaneously backward."

I do not remember to have read any case of post-mortem contractions in the writings of Sir Chas. Bell, Dr. Marshall Hall, Dr. Abercrombie, and others; nor in those of some celebrated French authorities on the nervous system. Dr. Marshall Hall, as I can state from conversation with him, has never yet seen any after disease. The most distinguished surgical authorities on injuries of the brain and spinal cord make no allusion whatever to the matter. And yet some diseases and injuries of the nervous system occasionally destroy with an exceeding swiftness; the muscular system

is well, and sometimes even extraordinarily nourished at the time they surprise; the muscles must be not rarely extremely irritable at the moment of death; and there is, all things considered, a condition of the body wherein the occurrence of such movements might surely not unreasonably be expected; neither might they, one would think, be absurdly looked for after some abrupt terminations of heart affection.

It was not, however, until a short time ago that, with the exception of what happened in consequence of cholera, I observed any movements whatever after death, though I am no stranger to the aspect of the body on sudden dissolution taking place. I allude to a fatal case of Apoplexy, and will now endeavour to describe briefly some phenomena which succeeded to the last expiration.

John Reid, a well-developed man, *stat.* 41, was admitted, one day last summer, at 1 o'clock A.M., into the Westminster Hospital, under the care of Dr. Basham. He had been apparently struck by apoplexy, was completely insensible, breathed very stertorously, the circulation was languid, and he was pale and cold. The pupils, unlike what generally happens in the apoplectic, were as narrowly contracted as I have ever seen them, either in sleep or poisoning by opium. He could be made to swallow with great difficulty. Some wine, was, however, administered, and in a few hours he rallied considerably, at least so far as organic life was concerned; his breathing was less embarrassed, although still alarmingly impeded; his pulse became more vigorous and rapid, and there was a most palpable increase of animal heat, which I now regret was not measured by the thermometer. Towards evening he became obviously worse, and died at a quarter past 7 o'clock, no convulsion or spasm of the face or extremities having been noted previous to his decease.

Scarcely had he died, when the nurse who had attended on him hurried to me, and said that he was moving much as some of the patients did after death by cholera. I of course went directly to see the corpse, and found it was true enough. My attention was first attracted by motions in the feet. The toes were flexed; sometimes altogether, sometimes two, or one only, were inflexed, and the contractions varied greatly

in force. The whole foot was distinctly moved several times, but not with any great degree of violence. The tibialis anticus was in a frequent flickering action. The whole of the muscles was not affected, at least not visibly. A narrow portion only, running its whole length, was seen to act; but so fleeting was the motion, that unless the eye was fixed upon the muscles at the very time of contraction it would have eluded notice. The motions of the toes could be much more easily surveyed. They now affected one foot, now both feet simultaneously. The gastrocnemii muscles were once or twice slightly influenced. The hands were slightly moved twice or thrice; and the fingers flexed several times. The motions would pause for a while, and then be renewed, coming and going just as I have seen them after fatal cholera. I could not *exactly* discernible motions, either by flexing the limbs, or striking the muscles, or picking them. There were no motions of the face, thorax, or abdomen. The contractions were timed, and found to last *three-quarters of an hour*. They could not have been distinguished in any way from those which have been noted subsequently to cholera; and if I have seen some evidently more marked and striking after that disease than these were, I have likewise noticed others which were infinitely less so.

The surface felt extremely warm. I placed the bulb of a thermometer in the axilla *half an hour* after death, and ascertained a temperature of 104° . The body had been, judging by the touch, of an unusual heat during lifetime. I do not, therefore, mean to imply that there was any rise of heat. I do not think that there was, though it is impossible to affirm this positively.

Bodies should be watched closely and patiently after all kinds of sudden and rapid dissolution, by any one desirous of witnessing such motory phenomena as those described: experience only can determine how often search will be successful. But it must be remembered that the *most rapid* deaths, some through violence, and more in the course of nature, occur month after month, day after day; the subjects of them must often be observed more or less closely, and for a longer or shorter time after dissolution, by friends, or others, with them when they die, and yet no motions are seen in such cases at all comparable

to those which happened in this apoplexy, though there may be occasionally a faint muscular quivering or two observed, enough to attract a passing notice. Only let us imagine the vast numbers who annually perish in hospitals, infirmaries, union houses, and private dwellings, without any such remarkable movements being seen. Still, in most new inquiries, one is supplied with more abundant instances than were hoped for. This is a consequence of research. Look at the many cases of reflex action in paralytic limbs which have now been seen at one time or other. Most of them would certainly have been passed over, but for special questioning.

We have long heard of post-mortem movements happening in India. Some formerly have either treated them as fables, or not troubled themselves about them because of their occurring so far away. So with many the cholera excites no interest now. They say it is in Malta, and elsewhere, but not here; that the topic is not popular, and they will think no more of it till the disease returns. Should it again invade, there will once more appear a myriad of contributions, and the columns of journals, medical and general, will be forthwith crowded to inconvenient excess.

It can, with difficulty be supposed that the case of apoplexy which I have just sketched is the *only* instance of that affection in which these contractions have taken place, though it may be the only one in which they have been seen to any remarkable extent; and it may be conjectured that other cases of sudden or rapid destruction caused by injuries or diseases of the nervous system, or other injuries or maladies, or, perchance, by the speedy course of poisons, will, sooner or later, be found to exemplify them. It remains for future inquirers to treat the whole subject of them in its length and breadth; to note the species of death in which their supervention will be most probable; to mark accurately their periods of beginning, declining, ending; their course, their form, and what may best provoke them; to trace minutely every circumstance of interest, and to show with all clearness, fulness, and judgment, their every physiological and pathological bearing. What I have written is but a fragment.

Let me now subjoin a short state-

ment of the principal pathological changes observed after death in the preceding instance.

There was no peculiarity in the external appearance of the body, which was examined *eighteen* hours after death.

The limbs were by no means unusually rigid.

The vessels of the brain were more congested than usual, and a flattening of the convolutions was observed.

On opening the left lateral ventricle it was found completely filled with coagulated blood; the septum was broken down, and some bloody fluid, with scarcely any coagula, was observed in the fellow cavity. Some blood, too, had made its way into the fourth ventricle. The hæmorrhage seemed to have proceeded from the corpus striatum, thalamus opticus, and almost the entire portion of brain which formed the floor of the left lateral ventricle. This portion was soft, irregular, shreddy, and the blood which covered it was mixed here and there with diffuent cerebral matter. Numerous bloody points remained diffused everywhere, after water had been gently but freely poured over it. There was no marked degree of softening in any place, if we except that part of the brain which lay just immediately beneath what appeared the direct seat of the apoplexy.

The arteries at the base of the organ presented unmistakable and pretty general signs of fatty degeneration. The heart, which was, I think, to some degree enlarged, showed a marked rigor mortis, such as formerly led to the use of the erroneous term "concentric hypertrophy." The aorta was thickly studded with atheromatous or fatty deposit, and there was a considerable layer of fat upon the surface of the right ventricle. The kidneys, which lay imbedded in most abundant fat, were atrophied, and far advanced in granular degeneration. Some urine was in the bladder, which was highly albuminous.

Thinking it most likely that the muscular fibres of the heart would be to some extent in a state of fatty degeneration, though, looking to the rigid state of the organ, not far gone therein, I requested Dr. Quain to examine it microscopically; but, through accident, the heart did not reach him in a state sufficiently fresh to admit of that satisfactory scrutiny which was desirable.

Dr. Quain observes, in a letter with

which he favoured me,—"There is abundant evidence of granular fatty matter in the muscular fibres. This appearance exists throughout several parts of the heart that I have examined; but the granules are very small. The degeneration, therefore, though extensive, is not greatly advanced. It is possible that the presence of some of this matter is due to decomposition; still I feel quite satisfied, from the general appearance of the heart, that fat would have been found if the heart had been examined when fresh."*

Examinations of cases of this kind cannot, of course, be satisfactory without the microscope be used. The pathology of fatty degeneration is only to a slight extent explorable by the unaided eye. I can hardly doubt that, had the small blood-vessels of the brain been examined, we should have discovered in such degeneration of them the precursor and cause of the apoplexy. I rest this observation on a communication of Mr. Paget to the Abernethian Society, "On Fatty Degeneration of the Small Blood-vessels of the Brain, and its relation to Apoplexy,"†—a contribution to pathology of the highest value.

It would have been necessary to have apologised for making such special reference to the fatty degeneration of the heart which was observed, had not the subject of that common change the widest possible relation to the whole question of muscular irritability, whenever, however manifested.

The relation of such degeneration to the irritability of the heart is every now and then emphatically exemplified by the action of the organ suddenly failing, and as suddenly producing death. The proper stimulus is not withdrawn, but the damaged fibre fails to respond: the stimulus cannot act on it, as galva-

* The just published volume of the Royal Medico-Chirurgical Society contains a most admirable contribution, by Dr. Quain, on Fatty Diseases of the Heart.

† Since published in the *MEDICAL GAZETTE*. Fatty degeneration will, it may be fairly anticipated, be the means of explaining some forms of paralysis, both cerebral and spinal, some diseases of the special senses, and some affections of the mind, especially in aged people, whereof the immediate cause is at present hidden. In a case of nearly complete blindness (the subject of which was a patient of Mr. Hall's) the optic nerves were found after death to have most extensively undergone this form of degeneration. They were first examined by Mr. Holthouse, and afterwards by Mr. Dalrymple, who were both quite satisfied on this point. I had the opportunity of examining them.

nism cannot influence an inirritable muscle. It must be observed that a heart, wherein fat has taken to a serious degree, the place of the proper irritable tissue, may be equal to ordinary, though not to extraordinary, occasions. Shock of mind or body, intense emotions, whether of joy or sorrow, syncope, and all accidents and diseases which kill quickly by asphyxia, must obviously be far more perilous to those whose hearts are thus affected than to others; means of recovery would be lost in them which might succeed in others; the suspended animation might be death in them which would not in others. And every cardiac fibre is impaired in use just in proportion as it is in texture, whilst the entire destruction of the irritability of a single one is a type of what may eventually happen on a large, and perhaps fatal, scale.

In some cases of the change, the voluntary muscles, from having extensively sufficed in like manner, would probably be found less susceptible of galvanism than they commonly are. It is not pretended that this would have been discovered, had a trial been instituted, in the instance I have mentioned, for the degeneration of the heart was not in any sense nearly so marked as it is oftentimes; and the contractions of the muscles, seen after dissolution, lead to the conclusion that their fibres were entirely or almost sound.

Nothing can be plainer than that this degeneration may greatly modify, and, at times, prevent every kind of muscular action. A cause of cramp, it may be assumed, would not affect an excessively fatty muscle as it would another; nor would galvanism, nor strychnia, nor heat, nor cold, nor emotion, nor the will, nor any other sort of known excitant; neither, *anteris paribus*, could we so reasonably look for post-mortem contractions in such a muscle as in one unimpaired. And we may safely believe that, amongst the causes which may prevent, or aid in preventing, the occurrence of post-mortem contractions in some cases of sudden death by apoplexy, the change in question merits distinct notice.

Certainly such contractions could not occur in the altered parts, no matter what the circumstances of death, in such a case as that described by Vieq d'Azyl, the subject of which had almost all the muscles of the leg converted into

fat or cellular tissue, and "was obliged in the latter part of his life to walk with crutches."* We must not, of course, ever confound the fat deposited upon, with that which takes the place of, a structure—for there is a wide difference between addition and destruction—nor suppose a part to be necessarily degenerated because largely and perfectly surrounded by it. "Fat," says Mr. Paget, "accumulated in tissue round a part is a very different—probably an essentially different—thing from fat within it: the one is compatible with perfect strength; the other is always a sign of loss of power. In the muscles of some fish—the eel especially—it is hard to get a clear sight of the fibres, the oily matter round them is so abundant; but the fibres are peculiarly strong, and, in their own texture, make a striking contrast with the fibres of a degenerate muscle in which the fat is in great part within."†

It might have been laid down from ordinary reasoning that the structural state of muscles has much to do with the post-mortem contractions which happen in them, and that it could not be otherwise; nor need we wonder that males should manifest them more frequently than females, and the strongest males most of all. It may be, occasionally, that the reason of some muscles of the same subject contracting to the exclusion of others is, that they are more irritable in consequence of being better nourished.

We need not be too surprised at the after death contractions of cholera being so partial and disuniform: for only let us fancy the very varying states of the muscular fibre existing in different instances at the time of dissolution; the unlike degrees in which this fibre has been exhausted in the course of the disease; the diversified states of its nutrition; and the dissimilar condition of the blood in its vessels; and we shall, to rest here, see no just cause for extraordinary amazement. The physical state of man—to say nothing of his mental—is never, perhaps, exactly the same throughout any two given minutes of our existence singularly impossible.

* See Corvisart's oblique version of the Muscular Tissue as a Fatty Substance." *Dis-1862.*

† See Lectures on Nutri Atrophy, Man. Chap. 11.

for ever fluctuating. Never, as Bichat insisted long ago, shall we understand life's mechanism in the sense that we comprehend the workings of instruments of our own invention; but we shall be perplexed always as to many matters, because we have not *all* the data which their solution requires, and, from the very nature of the problems, cannot, nor even reasonably hope to, have.

These contractions come and go much as cramps do during life; a renewal of irritability appears indispensable to the continuance of both sets of actions, and its temporary exhaustion accounts, apparently, for their so common intermittence. The varying periods at which they begin after dissolution is, it may be imagined, in some way connected with unlike conditions of the fibræ; at the moment of dying, as to the exhaustion or renovation of their power; and in the interval between the last expiration and their commencement it is not improbable that their force may be renewed: for there are facts to show that irritability may be restored or augmented, of course within limits, even after the circulation of the blood has ceased. The cause of these movements in the dead might in some instances, had it acted in the living, have provoked cramps. But one thing seems certain: they generally suffer most from cholera who have the most irritable and least easily exhaustible fibræ at the time of its attack, the muscular energy heretofore so valuable to, and, perchance, by some men envied, them, becoming subservient to their exquisite agony, and rendering them liable, beyond all others, to those contractions which happen, sometimes, after the end of life.

Westminster Hospital, Sept. 25, 1850.

[To be continued.]

EMPLOYMENT OF IODIDE OF POTASSIUM IN CIGARS FOR CONSUMPTIVE PATIENTS.

M. CHARTBOULE has suggested the idea of the administration of iodide of potassium in cigars, as a method by which, in cases of phthisis, the salt will be more rapidly absorbed and more surely effect the cauterization of the tubercles (!) It is further stated by MM. Piory and Cl patients who have made indicated cigars have constant marked improvement in them.

An apparatus has been devised to prevent irritation of the larynx.
—*Journal de Chimie Méd.*

ON THE ORIGIN, CAUSE, AND NATURE OF CHRONIC OVARIAN TUMORS.

By EDWARD JOHN TILT, M.D.

Senior Physician to the Paddington Free Dispensary for the Diseases of Women and Children, &c. &c.

[Continued from p. 449.]

Predisposing Causes.

If we conformed to current opinions, our chapter on the determining causes of chronic ovarian growths would indeed be short; for they are generally pronounced obscure and unintelligible: and so, indeed, they are, to those who are ill acquainted with the physiology of the ovaries, and who expect to find in the dim memories of their patients some *one* cause for a tumor, they are only asked to cure when it has attained a considerable size. If, however, we interrogate an intelligent, self-observing patient, notwithstanding the time that may have elapsed since the commencement of the disease, she will generally tell us that previous to the development of the tumor menstruation was accompanied by more or less intense dysmenorrhœa, or that some fall, or other accident, had occurred during menstruation, or at the time of its cessation, which accident suspended its flow, and brought on prolonged pain in the ovarian region. She may then add, that these pains seemed to have nothing to do with the tumor; that they left her, and she was cured; but still on further inquiry she will often own that the pain returned in the same way for several times before she became aware of the existence of a small tumor. The length of time which often elapses between the origin of a tumor and its attaining a considerable size, no doubt renders an increased acuteness necessary in the medical adviser; but the causes of chronic ovarian growths are as easily detected as those of any other complaint. We have already indicated the conditions which predispose to the growth of these tumors, and we have added them to be predisposing causes of inflammation of the ovaries. The same causes are, also, those of acute ovarianitis. We shall first assign those which have been assigned as causes of chronic ovarianitis.

nic ovarian cysts, and then inquire into their modus operandi. My friend, Dr. Chereau, who has done so much for ovarian pathology, has given (*Union Médicale*) a specification of the causes of ovarian dropsy in 179 cases wherein they could be detected. Mr. S. Lee has given 36 cases wherein the specific causes could be assigned; and on adding to these 34 cases which have come under our own observation, we obtain a total of 248 cases, the various causes of which stand in the following proportion:

Marriage	30
Parturition	47
Suppressed menstruation	38
Cessation of menstruation	10
Dysmenorrhœa	28
Abortion	15
Exposure to severe damp and cold	31
Blows and falls on the seat, &c.	15
Disappointed love (Mr. S. Lee)	1
Violent fit of anger	1
Appearance of an eruption (Mr. S. Lee)	1
Other causes (Chereau)	82
Total	248

This table forcibly shows that in the majority of cases the rise of ovarian growths not only often coincides with the morbid excitement of the ovaries, and with the perturbation of their functions, but even with their natural performance. In certain predisposed subjects matrimonial influences excite the development of the morbid as well as of the physiological cell; and the effect of conception in the morbid cell is to stimulate it to increased action, acquiring a fresh impulse: it increases with the advance of pregnancy. This influence of the phenomena of parturition on the development of ovarian tumors, which has been too much overlooked of late, was formerly considered as proved thus:—David Manchart (quoted by Morgagni), speaking of ovarian dropsy, says, "that it may be particularly suspected when the tumor has had its origin in a laborious confinement, in puerperal fever, in the gestation of a false conception, or in abortion." The causes ascribed to ovarian dropsy are those which we have shown to be the causes of ovaritis: we therefore conclude that the causes of ovaritis, under certain predisposing circumstances, also produce ovarian dropsy. We shall not again discuss the mode of action of these causes, as we have already done

so with more than usual care in our chapter on the causes of subacute ovaritis.* An approximation of the comparative degree of frequency of chronic ovarian diseases in married and in single women may be derived from the following table, wherein we have embodied the cases which were collected by Dr. Chereau, Mr. S. Lee, and various others, as well as those observed by Bluff and ourselves, in which cases it has been distinctly stated whether the patients were or were not married:

	Married.	Single.
Chereau (<i>Union Médicale</i>)	160	110
S. Lee (<i>on Tumors of the Uterus</i>)	99	87
Bluff (<i>Experiences</i>)	15	6
The Author	26	14
Total number of cases.	290	197

Thus, contrary to the opinions of Drs. Ashwell and Nauman, and others, we are led to admit that those who have been married, and who have been most subject to the manifold vicissitudes of sexual life in our highly civilized societies, are most liable to this disease. J. Jefferson is of opinion that women married late in life are the most prone to it; and in this Professor Recamier agrees, and adds, particularly so if the marriage be not followed by childbearing. Glancing at other supposed causes of ovarian disease, we find that prostitution has been considered by some as favouring its development: but while we have good evidence to show that this cause produces subacute ovaritis and inflammation of that portion of the peritoneum which enfolds the ovaries, we have no facts to prove its influence in producing ovarian tumors. Much has been ascribed to onanism; but vague assertions only have reached us, not positive facts: it stands to reason, however, that pernicious habits, by reacting on the ovaries, must increase any morbid action which may pervade them; a similar result may be produced by any hæmorrhoidal tendency, and the pressure of impacted feces; and the obliteration of the ovarian vein has also been given as a cause (Douglas, *Philosoph. Trans.* vol. i. p. 122.) Before attempting to show how the

* On Diseases of Menstruation, and on Ovarian Inflammation. By the Author.

causes of ovaritis produce chronic ovarian disease, it is necessary to observe that most morbid anatomists admit the inflammatory origin of adventitious cysts. Without burdening ourselves with numerous citations, we shall quote the opinion of one of our most eminent authorities on such matters: "Although the origin of adventitious serous cysts is in a great many cases involved in much obscurity, there are good reasons for believing that it is similar in kind to that of adventitious serous membranes; that is to say, that these cysts originate under the influence of causes which excite inflammation of the cellular tissue in circumscribed portions of organs" (Carswell's Pathological Anatomy). We only affirm, then, that cysts are formed in the ovary by a process similar to that which produces them in other parts of the body; and we also add that, as under healthy circumstances, the ovary devotes its whole energies to bring the menstrual cysts to perfection, so under the influence of inflammatory causes it devotes the same energies to produce encysted growths of a morbid nature. In no system is inflammation more susceptible of being limited to a very small space than in the sexual system, for the neck of the womb may be severely inflamed without the body in the least participating in it. Huguier affirms that the follicles of the uterine mucous membrane (the ovula of Naboth?) are sometimes found dilated and inflamed, while the other portions of the mucous membrane do not participate in the inflammation. Cruveilhier and other observers have sometimes seen one uterine vessel full of pus, offering distensions about the size and shape of small olives: the fallopian tubes may also be independently inflamed, one or both ovaries may be so; a single ovary may be inflamed in its stroma, in its peritoneal coat, in certain portions of its tissue, or in one menstrual cell which may alone be found containing pus or well organized false membranes. When we talk of partial ovaritis producing the different forms of chronic ovarian disease, we merely affirm that inflammation develops, in a morbid degree, the structural tendencies of our organs. Inflammation of the bony structure will cause a greater deposition of saline particles in their cellular texture, that of the serous membrane an increase of the usual fluids and other products; in

like manner partial ovaritis develops the peculiar cystic structure of the ovaria by exaggerating the prolific tendency of its elementary tissues. When once the cyst is originated, and the balance between secretion and absorption on its internal surface is interrupted to the advantage of the secreting powers, there is no reason why exhalation should not continue predominant in artificial encysted cavities, as well as in those cavities which are natural. We believe inflammation to be as much the key-stone of ovarian disease as it has been proved to be of uterine pathology, and of the structural lesions of other organs. We believe that it lends a hand in the formation of almost every ovarian cyst, even when that inflammation is too circumscribed to be detected by any means we have at our command; and we thus admit that it is alone a cause sufficient for the very numerous cases of fibro-serous variety, and of ovarian growths, even when they are more or less solidified into cartilage, fibrous substance, or bone. If, however, the blood be loaded with constitutional impurities, with the occult seedlings of cancer, or the original elements of that protean diathesis called scrofula, the existence of which is clearly established, however difficult it may be to define it characteristically, then, if the ovaria are under the influence of any of the causes of ovaritis, they will withdraw from the blood which flows continually towards them the elements of disease, and will so assimilate them as to build up cancerous (colloid) encysted ovaria. The warp and woof of the cancerous tissue is afforded by the primitive structure of the organ. The cancerous diathesis finds the rough materials which are to be employed, and inflammation is the all-powerful agent which works up this rough material into the tissue of the organ. Thus we account for the production of areolar cancerous ovarian cysts.

When the follicular multicystic forms are found to exist in patients presenting signs of a scrofulous tendency, we can then in a similar way account for the strange and different productions we have described; but in many cases we cannot explain them by any constitutional affection. We must, however, bear in mind that it is but fair that the organ which has within it sufficient energy to secrete the living man should

also have the power of giving spontaneous growth to productions of an inferior nature; and that in the whole history of the ovary there is an uniform striving for production, which is equally shown by its physiology and its pathology; and thus, if the organs of increase are useless for fruitful progeny, they will often team with spurious growths. The streams of living productivity having been once made to rise in those hidden walls of our existence, they cannot be dried up. They will often go on producing imperfect growths or forms which, being perfect, can reproduce themselves; and so it is throughout the whole creation.

The inflammatory origin of ovarian cysts is not a new hypothesis; at least, as early as 1782 the German writer inculcated that doctrine in the following words:—"Interioris generationis partes, præsertim ovaria fere nunquam liberantur ab assidua et sæpius repetita irritatione et humorum congestione; quæ vitiosæ tantum irritata ovula alterantur, et quandoque humoribus inundata tumescunt ut hydropici status initia præbeant" (Kruger, Dissert. Gœttingue, 1782).

A more modern writer in the same country (Meissner) also says, "Chronic ovaritis is the mother of all ovarian degenerations." Cruveilhier, Ricord, Caseau, Chereau, and numerous other writers, have entertained an opinion in same degree similar; but we have attempted to make it our own by the development we have given it; and we think these views are not only true in pathology, but eminently useful in practice; and that those ovarian cystic growths, which are the opprobrium of our art, would be much less frequently met with if the subacute ovaritis from which they spring, and by which they are fostered, was more carefully sought for and more energetically treated. Nature is, after all, the best court of appeal in pathological as well as in all other questions: so, without referring to olden times, we shall mention a few contemporary cases, wherein ovarian cysts have had an inflammatory origin.

A woman, 21 years of age, was, during the menstrual period, exposed to the influence of damp and cold. The catamenial flow was stopped, inflammatory symptoms appeared, and a small tumor was soon detected, which in three

years reached the abdomen, and was extracted successfully by our friend Dr. F. Bird, who says he has seen several similar cases (*Lancet*, June 15, 1845).

Catherine Staub, 25 years of age, and in perfect health, received a kick in the abdomen during the menstrual epoch. The flow ceased, and soon after an ovarian tumor was discovered, which in a few years caused the death of the patient (*Arch. Gén. de Méd.* tom. iv. p. 582).

Mrs. Palmer, 39 years of age, who always suffered from dysmenorrhœa, felt acute pain in the right ovarian region; pain increased by pressure, and soon after tumefaction was detected, and gradually increased (*Lancet*, May 18, 1839).

An unmarried woman, 30 years of age, under the combined influence of cold and violent fit of anger during the menstrual period, was seized with acute pain in the hypogastric region, accompanied by shivering, fever, and hysterical symptoms. The menstrual discharge stopped, and a considerable ovarian tumor was speedily developed. The patient was tapped by the vagina, and eventually cured (Pr. Hirtz, Thèse de Strasbourg, No. 67, 1841).

In the *Lancet* of September 9, 1846, we have recorded a similar case, wherein a large ovarian growth evidently arose from acute ovaritis.

Madame Dubois, 24 years of age, first menstruated at 16, and was always subject to considerable disturbance of the function. She married, and was confined the following 21st February, 1822. Twenty days afterwards she travelled nine leagues on horseback. The weather was bitterly cold, and she felt acute pains in the ovarian regions. Flooding came on; but by appropriate remedies this subsided, and nothing remained but a dull and deep-seated pain in the right ovarian region. For this, however, she did not consult us until three years after, when we found a large ovoid tumor, about a foot in diameter, springing from the right side, and occupying nearly the whole abdomen.

On March 20th, 1827, the sufferings of the patient from abdominal distension were so acute that, although fluctuation was not manifest, tapping was resorted to. A first puncture only brought out 8 ounces of serum; 1 on

tapping more to the right, 35 lbs. of an unctuous liquid were withdrawn. The patient recovered sufficiently to resume her daily avocations; but the tumor re-filled, and, after several tapplings, her constitution gave way, and she died. On a post-mortem examination a multilocular cyst, weighing fifty pounds, was found in the abdomen (T. Butand, *Thèse de Montpellier*, Nov. 27, 1832).

We shall conclude with another case, wherein the origin and form of the disease was inflammatory, and where a successful termination, was brought about by the judicious treatment of Mr. Phillips. Mary Addison, was admitted into the Westminster Hospital, under Mr. B. Phillips, in January 1849. She is of a thin, spare habit, and 86 years of age. She menstruated for the first time at 16; and since then, up to the time when she became affected with ovarian dropsy, menstruation was regular and painless. In June 1846, during her menstrual epoch, she was seized with a sharp pain in the right iliac region, which lasted eight days: there was also swelling in the same spot. The catamenial discharge appeared, and went on as usual. In August 1847, she was seized with a similar sharp pain in the right side, accompanied by swelling. The symptoms were more severe than on a previous occasion, and lasted three weeks. She was bled in the arm, and had fomentations to the side. As in the previous attack, the menstrual flow followed its usual course. The patient does not remember whether it preceded or followed the appearance of acute pain in the right iliac region. In February 1848, while grinding coffee, she felt a return of acute pain in the side. This increased, and was accompanied by swelling in the iliac region, which was distinctly felt by her master, Mr. Bayntin, of Lower Eaton Street, who ordered leeches, gave purgatives, and employed other judicious treatment. In the meantime the menstrual flow appeared, went on as usual, and naturally made Mr. Bayntin give a favourable prognosis; but as the patient did not mend, he sent her to St. George's Hospital. Notwithstanding the energetic treatment prescribed by Dr. Barnes Jones, and the numerous applications of leeches (78 at different intervals), to the right side, the tumor, in less than a month, increased

to the size of a child's head. There seemed to be partial peritonitis in the epigastric region, as for several days there was manifest eripitation to be felt on pressing to and fro the teguments in that region. The pain was also so intense, and the general symptoms so considerable, that some of those who watched the case thought suppuration was going on in the tumor. It was not so, however; for Dr. B. Jones, having thought it advisable to have the tumor punctured, Mr. Tatum made an exploratory incision, and withdrew a tea-spoonful of glutinous liquid tinged with blood. The patient was relieved by the puncture. The tumor, however, increased; but, as it was no longer accompanied by acute local symptoms or fever, she left the hospital. Some time after this she entered St. Bartholomew's Hospital, where her abdomen was covered with a large plaster. When cured of the violent erysipelas occasioned by this plaster, she left that establishment. In the hope of finally obtaining relief, she some months afterwards entered the Westminster Hospital; and here began Mr. Phillips' treatment. He plunged a small trochar into the cyst; and, withdrawing a little sero-purulent matter, he intended leaving the tube in the cyst, to promote the inflammation of its internal cavity, but the tube fell out after remaining there seven hours. Its introduction produced considerable inflammation of the tumor, and when this had subsided Mr. Phillips again punctured the cyst, and introduced a small India-rubber catheter, which was left in for several days, during which time a considerable quantity of sero-purulent fluid trickled out. This was followed by considerable inflammation of the cyst, which greatly increased, and there was evidently a pointing of the tumor in the spot where the cyst was punctured. We thought the tumor was going to burst, and we anticipated the possibility of the patient being radically cured by adhesion of the walls of the sac. We were, however, deceived. The inflammatory symptoms abated, the cyst diminished in size, and Mary Addison, instead of appearing as though seven or eight months advanced in pregnancy, presented no preternatural enlargement of the abdomen, although a hard globular tumor was easily detected by manual examination. She

was for several weeks confined to her bed, by an acute attack of rheumatic gout; otherwise she would be moving about, pursuing her usual avocations.

The patient called on us on the 6th of September last; she had recovered her health and strength. By a careful abdominal examination, a small lump about the size of an egg could be detected on the right side, but it was not painful on pressure. Menstruation, which had ceased during the patient's sojourn at St. George's and St. Bartholomew's, had returned once previous to the tapping of the cyst by Mr. Phillips; it then ceased, to reappear in July last, and has continued regular and painless as before the development of the tumor. There is, however, during menstruation, a greater amount of swelling; and when she gets up suddenly, or walks fast, she feels "a little sharp pain," which lasts about a minute or two.

Remarks.—We have given this case entire, as, in more than one point of view, it is worthy of our attention. The cure by reabsorption of the purulent contents of the cyst is in itself interesting, as in this disease we have as few means of effecting a radical cure. The acute nature of the tumor, its attaining the size of a child's head, and being attended by the symptoms of suppuration in the short space of a month, is also well worthy of remark. The origin of the tumor in sub-acute ovaritis is the reason of our recording the observation in this place. Three successive attacks probably took place, each attack being more severe than the preceding; each was accompanied by the menstrual flow, which in the first attack effectually relieved the patient's sufferings, did so imperfectly in the next, but in the third it had no effect. The fact of the disappearance of the tumor by the absorption of its contents, and the subsequent appearance of acute rheumatism, is also interesting. Mr. Phillips has seen similar cases, and looks on the facts as related to each other as cause is to effect; but Mary Addison was subject to rheumatism, having already had an acute attack at St. George's Hospital, and previous attacks in her youth.

8, York Street, Portman Square.
October 1850.

REMARKS ON THE VITAL STATISTICS OF THE BOROUGH OF PLYMOUTH.

FOR THE YEAR ENDED ON THE 31ST OF
MARCH 1850.

By Dr. W. H. HAMMOND.

Barbarians in Calcutta, cholera spread with destructive rapidity to every town and village within an area of many thousand square miles, from Silhet in the east to Cuttack in the west, from the Delta of the Ganges to its confluence with the Jumna at Allahabad. Proceeding from this extensive district in three directions, it totalled a south-west direction along the Circar coast to Madras, which it reached in twelve months; and it also arrived at Ceylon, whence, towards the close of 1817, it was carried to the Mauritius in the Topaz frigate.

Proceeding along the eastern shores of the Bay of Bengal, it took twelve months to reach Arracan, whence it traversed the Malay peninsula to the Iang, where it swept away three-fourths of the population. It next spread over the islands of the Eastern Archipelago; and, on the west coast of Borneo, it carried off the whole of a Dutch garrison but one who witnessed its fury. Arriving at Canton in 1820, it attended in the following year to Peking, and passed the six succeeding years in decimating the dense population of that extensive empire. At length, in 1824, it crossed the celebrated wall, and, carrying destruction among the semi-barbarous population of Tartary, it invaded a large portion of Mongolia.

Again, branching from Calcutta as a centre, taking its route along the valley of the Ganges to Bundelkund, it swept off no fewer than 8000 of the army and camp followers there under the orders of the Marquis of Hastings. Pursuing the course of the Jumna, it arrived at Delhi, Ratah, and Surampore, in August 1818, and spread from thence to Nepal, on the southern slope of the Himalayan range, and to Bombay on the western coast; hence, following the sea line, it travelled on to Cape Comorin, the southern extremity of Hindostan.

After a sojourn of twelve months

these districts it penetrated further into Asia, and made an abrupt lodgment in Ooleypore, the capital of Chittore, and, during the space of two years committed its usual ravages in this town and its adjoining territories. Here it lingered for two years, at the end of which it broke into Persia, where it took up its head quarters in the three principal sea-ports,—Muscat, Bushire, and Bus-sarah,—carrying off at the former 10,000 victims; and at the latter, situated at the mouth of the Tigris, 18,000, or nearly one-third of the population, in the brief space of 11 days, or at the daily rate of above 1,633. Ascending the Tigris, and its tributary the Euphrates, of Scriptural celebrity, it arrived at Bagdad; the city of the Caliphs, which was at the time besieged by the Persians: here it swept away, with indiscriminating fury, alike the besiegers and the besieged.

Continuing its course along the banks of the Tigris it reached Aleppo, the capital of Syria, one of the cleanest and best built towns in the Turkish dominions; and, pursuing simultaneously the course of the Euphrates, it effected a lodgment in Erzeroum, the capital of Armenia or Turcomania, a considerable town, distant five days' journey from the Black Sea, and ten days' from the frontiers of Persia, reaching both these places in the summer of 1822. From Erzeroum, the Turkish army, in its retreat, carried it to Bakou, near the 40th parallel of latitude, on the western shore of the Caspian. Hence it spread to Astracan, at the mouth of the Volga, where, after immolating 144 victims, it appeared for a time to subside; while from Aleppo it extended to Antioch, and other parts of the Levant.

Halting then for about six years on the eastern frontier of Europe, it reappeared with renewed virulence, in the summer of 1829, at Orenburg, a town N.E. of Astracan, 400 miles from the mouth of the Oural, on which it stands. In the July of 1830 it revisited Persia, extending its ravages from Astracan, at the mouth of the Volga, to Saliany, at the mouth of the Cur, along the whole western shore of the Caspian.

Ascending the Cur it reached Tiflis, and thence advanced along the Terek and the Kuma into Caucasus; while, following the Volga from Astracan, it advanced to Sarataff, and from thence reached Moscow on the 14th of Septem-

ber, 1830. Thus established within the limits of Europe, it set the vigilance of quarantine at defiance. It spread rapidly to Petersburg and the shores of the Baltic. Descending the Don to Azof, and the Black Sea, it arrived at Odessa; whence, ascending the Danube, it reached Vienna in August, 1831, during which year it infected Austria, Germany, Hungary, Egypt, and Turkey. On the 26th of August, 1831, it was conveyed from Hamburg to Sunderland, and broke out, in the January following, at Edinburgh. On the 14th of February it appeared in London; and, on the 22d of March, showed itself in Dublin, whence it spread rapidly over the whole country. On the 11th of June it made its first onslaught at Coxside, on the eastern side of Plymouth, a low situation bordering on Sutton Pool, whence, as from a focus, it spread rapidly, 77 cases and 31 deaths taking place during the 19 days which remained of the month,—averaging 4 cases daily, and about 8 deaths in 5 days. Among the deaths were those of a woman named Eoderich and her daughter, aged, the former, 42, and the latter 19, years, who expired at No. 11, Higher Street, within half an hour of each other. In both these cases the transfusion of albumen and salt into the veins was without effect.

About the 2d of July it broke out in Devonport. It reached Exeter on the 19th of July, and attacked, in the 12 first days of its existence, 45 persons, of whom 19, or above 42 per cent., died, the daily average of attacks being 3.2. Its duration here was 100 days, in which it proved fatal to 345 out of 1,145 who were attacked, or above 30.39 per cent, the mean daily mortality having been 8.45 per cent.: in Plymouth it reached to nearly 6 per diem.

In the 17 years which elapsed from its outbreak among ourselves in 1832 to its return in 1849, we can hardly believe that it had wholly ceased to exist, although no accounts of its effects have reached us: its hidden fires may have slumbered without ceasing to exist.

Dating from its appearance in 1817 at Jessore, to its establishment at Plymouth in June, 1832, we find a total interval of 17 years occupied in carrying destruction over at least one half of the known portions of the globe. But this is far from being the true limit of its actual duration; for we find it continu-

ing its ravages for a farther term of 5 years: in Spain and Portugal in 1833 and 1834; in Italy in 1836 and 1838, and at length at Malta, where it is supposed to have expired, in 1837. During its existence in this country, from its

first appearance at Sunderland, it attacked, as the official reports inform us, 187,060 persons, of whom 84,533, or about 45 per cent., recovered, and 52,547 swelled the black catalogue of mortality. These were distributed as follows:

	Cases.		Recoveries.		Deaths.	
	Number.	Centesimal Proportion.	Number.	Centesimal Proportion.	Number.	Centesimal Proportion.
England and Wales.	62,326	45.47	41,600	49.21	20,726	39.46
Ireland	54,552	39.80	38,381	39.49	21,171	40.29
Scotland	26,202	14.73	9,552	11.30	10,650	20.25
Aggregate . .	187,060	100	84,533	100	52,547	100

In London, out of 11,020 cases, 5,745, or 52 per cent., recovered, and 5,275, or 48 per cent., died.

Let us now endeavour to trace its progress in its last approach to our shores.

Its first authentic reappearance was at the beginning of the hot season in 1845 at Cabool, only four years before it reached us. By the 10th of July it had reached Umballa, one of the filthiest towns in Lahore, and the best fitted for its reception and propagation. We cannot, therefore, be surprised to learn that in the brief space of 57 days, or up to the 5th of September, it attacked, exclusive of natives not included in the returns, 322 men of the 31st Regiment, or above 5 per diem, of whom 142, or 44 per cent., died. By the 12th of the following June it had reached Teheran, where it swept off 300 victims daily. In February, 1847, it was at Kouba. Previous to this, on the 16th of November, 1846, a few cases had occurred at Saliany and Leucaran, places which it had visited 16 years before, and in which it is probable it had never been wholly subdued, although the mortality it occasioned may not have been of sufficient magnitude to attract particular notice.

In March 1847, after a fallacious interval of repose, from which it appears to have acquired a greater concentration of virulence, we find it raging with augmented fatality in the regions lying beyond the Caucasus; whence it diverged in the following month in three different directions: extending first to the north along the shores of the Caspian, as in 1822; next to the north west, in the direction of the mountains and the Black Sea; and thirdly, to the

west, arriving at Tiflis, the capital of Georgia proper, on the 17th of May.

Of these three divisions, the first, coasting the Caspian, reached Astracan in July, and, out of a population of 31,000, it attacked 5,195, about 16 per cent., or nearly one-sixth, of whom it carried off 3,131, about 60 per cent., or nearly two-thirds. Next, penetrating the regions watered by the Don, and occupied chiefly by the Cossacks, it attacked 12,651 of them, and proved fatal to 7,617, or above 55 per cent. At Woronesch, which it reached on the 4th of September, it raged with such fury as to attack 420 daily, 150 of whom, or nearly one-third, sank under the assault. The date of its arrival at Moscow is not known with precision, but few parts of the empire, Asiatic or European, were free from its ravages during the summer and autumn of 1847, in which time it attacked, as we are officially informed, no fewer than 300,000 individuals, to one-third of whom it proved fatal.

On the 24th of October it reached Constantinople, where it found an appropriate soil for its reception and propagation in the peculiar habits of the Turks.

September 1848 witnessed its simultaneous arrival at Vienna, Paris, and Hamburg, from which latter place it crossed over to Britain about the beginning of October.

In 1831, cholera made its first appearance at Hamburg early in October, and reached Sunderland, where, if I recollect rightly, it broke out in a ship from Hamburg, on the 26th of that month.

In 1848 it reached *Hamburgh* as early as the 1st of September, but does not appear to have gained a footing among ourselves before the end of that month, or beginning of October, when it showed itself simultaneously among the shipping both at London and Hull, — parts with which *Hamburgh* maintains a considerable commercial intercourse.

Sporadic cases of cholera, indeed, possessing all the unmistakable characteristics of the Asiatic or malignant form, had previously appeared, *rari nantes in gurgite vasto*, in both these places, as early as the preceding June, but these were not of that epidemic character which marked the disease afterwards, and can only be compared with the three or four similar cases which appeared among ourselves in the month of June immediately preceding the fearful outbreak in July 1849.

During the cold months of winter the malady slumbered, and appeared to be extinct; but, as the warm weather advanced, it awoke with renovated energy, and spread rapidly over almost every part of the country, carrying dismay and terror in every direction, and baffling the utmost efforts of science and vigilance to stay its progress or mitigate its destructiveness.

Unwarned, however, by the steady and unmistakable course of its advance—unwarned by the lessons of the past—heedless of the confident predictions of those whose memory faithfully retained the recollections of 1832—cholera in 1849 found us, with a very few honourable exceptions—such as that of Exeter—not only wholly unprepared to resist its invasion, but almost courting its approach by our utter disregard of sanitary improvements,—the window-tax still excluding the light and air of heaven—and the supply of pure water, and the drainage of our streets, utterly inadequate to the growing wants of the community, and the wholesome aphorism of *venienti occurrere morbo* apparently obliterated from our thoughts; while, with the listless apathy of Turks, we awaited the arrival of the destroyer, nor looked to our ramparts till the enemy had possession of the citadel.

We flatter ourselves, even now, that the danger has passed, and we may safely rest on our arms: we relax in our vigilance, and relapse without thought into our accustomed habits of criminal

negligence; while occasional sparks are bursting from the smouldering embers around, to remind us that the fire is suppressed, but not extinguished, and may at any moment burst out anew with fresh and resistless fury.

Notwithstanding the imperfect character of the meteorological observations made of the two periods of cholera, they expose some of the popular fallacies with respect to the origin of this most mysterious scourge; by some of which it has been attributed to some supposed deficiency of atmospheric electricity—to excessive heat, or moisture, or the variations of atmospheric pressure.

It will, however, be seen, on a careful comparison of the meteorological tables that there was the same alternation of weather and atmospheric pressure which occurs in ordinary seasons, and that there was little, if any, want of the usual amount of positive electricity. Hence the latent cause must be sought in some other direction than that to which we have hitherto been accustomed to look.

The following table of the mean monthly pressure and temperature, as recorded during the last nine years at Greenwich, may be serviceable as a farther standard of comparison, the months selected being those in which cholera was present. In the last year of the series the pressure and temperature for October and November are wanting.

The writer of an excellent article on Cholera, in the Companion to the Almanack for the present year, to which I have been indebted for the history of the progress of cholera, draws the following parallel between the two attacks of 1832 and 1849:—

“The main points in which the recent visitation of cholera appears to differ from that of 1831-2 are the longer continuance of the disease in the places visited, the greater tendency to subside and reappear, and the higher mortality” (p. 107).

To which he might have added, the greater rapidity of its advance into Europe.

The first of these points of difference has been strikingly exemplified in our own town, where its duration exceeded by four weeks that of 1832; but as to the increased mortality, when due allowance has been made for the advance

Mean Pressure of the Atmosphere.

	1841.	1842.	1843.	1844.	1845.	1846.	1847.	1848.	1849.
June . . .	29.801	29.801	29.700	29.814	29.775	29.866	29.805	29.642	29.689
July . . .	29.716	29.820	29.828	29.758	29.769	29.765	29.824	29.836	29.789
August . .	29.788	29.899	29.816	29.677	29.729	29.777	29.876	29.732	29.841
September .	29.824	29.715	29.617	29.881	29.801	29.824	29.825	29.833	29.767
October . .	29.423	29.846	29.604	29.562	29.847	29.516	29.809	29.646	
November .	29.672	29.599	29.718	29.690	29.575	29.821	29.905	29.785	

Mean Temperature.

	1841.	1842.	1843.	1844.	1845.	1846.	1847.	1848.	1849.
June . . .	56.4	62.9	56.3	60.7	60.7	65.3	58.0	58.5	57.9
July . . .	57.8	60.2	60.9	61.4	59.8	64.5	65.4	61.5	62.1
August . .	60.6	65.4	62.1	57.7	57.8	63.2	62.1	58.6	62.9
September .	58.1	56.4	59.5	56.9	58.6	60.1	54.8	55.8	58.8
October . .	48.8	45.4	48.0	49.5	50.2	50.5	52.9	51.6	
November .	42.7	42.8	46.8	44.0	45.8	46.0	46.9	46.8	

of population, it will be found that among ourselves it was lighter. In 1832, out of a population of 31,527, we lost by cholera 779, or 1 out of about 41; in 1849, out of a population of 39,571, or 8,044 more, it carried off 819, or 1 out of 48.

In 1832 the greatest intensity was in August, when 500 deaths occurred; while in 1849 the period of greatest intensity was divided between August and September, when 602 deaths took place, of which 290 belong to the former and 312 to the latter month. Of the 290 deaths in August, 139 took place in nine days, between the 10th and 19th—namely, 127 in St. Andrew's district, and 12 in that of Charles; while of 160 deaths in the same space of time in September, between the 15th and 24th, 39 only occurred in the former, and 121 in the latter; illustrating, in a striking manner, the observation made by the writer in the Companion, as to the subsidence and revival of the disease; for, at the time of the fresh and unlooked-for outbreak between the 14th and 15th of September, in Higher, Gaskin, and Moon Streets, in the district of Charles, the mortality in the larger population of St. Andrews was rapidly on the decline.

On a centesimal comparison between the population and mortality of the two periods, we still find the favourable balance on the side of 1849; the mortality of 1832 amounting to 2.46 per cent, while in 1849 it was only 2.06 per cent, being a difference of nearly one-half per cent.

But the two attacks seem to have

differed in other respects: in 1849 they were neither so sudden nor so violent; instances did not occur of individuals dropping down in the street, and expiring on the spot: nor do the fears of the inhabitants appear to have been so much awakened. This may perhaps have arisen from greater familiarity with the complaint, and from having been prepared to expect it, as well as from its almost exclusive restriction to the poorer classes.

We have now examined the more prominent points of distinction which present themselves, and shall conclude with directing attention to the singular predilection it manifested for water, following for the most part the direction of rivers, and the sinuosities of coasts, and rarely crossing any extensive tracts of unwatered land.

THE PUBLICATION OF SCIENTIFIC DISCOVERIES AND IMPROVEMENTS.

If you do make discoveries—if nature and education have made you great enough for that—be not mean enough to run away into the dark with it; skulk not out of good company to fatten on your good fortune, for recollect that you have borrowed as much from the liberality of others as your own genius will ever repay to the great world of science. If there be anything revealed to you, give it to the light, that others may interpret, and test, and prove it. Recollect that the apostles of science have all things in common; and if you meanly secrete a part of the wealth which belongs of right to the general stock, you deserve to be carried out of the profession, like Ananias, feet foremost.—*American Journal of the Medical Sciences.*

MEDICAL GAZETTE.

FREDAY, OCTOBER 10, 1850.

THE American Medical Association has rendered good service to the profession by the publication of its recent Report on the Comparative State of Medical Education in Europe and America. It is more than four years since a Committee was first appointed at New York to inquire and report on the adoption of a uniform and elevated standard of medical education throughout the United States. A preliminary report on this subject made its appearance in 1847; and this has been followed by the more elaborate document published in the last volume of the Transactions of the American Association. The first Report points out the defects of the American system; the second compares this with medical education in the various countries of Europe, and contains resolutions on the best method of raising the character and acquirements of medical practitioners in the States.

There can be no doubt that the most durable reform of medicine will be that which improves medical education; and perhaps this has been hitherto too much lost sight of in the attempts at legislation in this country. The desire has been rather to define and limit the powers of Universities and Colleges already established by Act or Charter, than to go to the fountain head, and remove those anomalies which are known to exist in medical education. There are three kinds of curricula in England, and a still greater number in Scotland and Ireland. Such differences ought not to exist; there should be some uniformity in the system of instruction, in its duration, and in the examination to which the candidate for medical prac-

tice is compelled to submit. Our object, however, at present is to ascertain how far the British system is superior or inferior to that adopted in other countries.

The first American Report (1847) tells us very plainly that the European system, defective as it is considered to be in each country, has many advantages over that adopted in England and the United States. In the first place, in addition to the branches taught in American schools, medical education in Continental institutions comprises the several subjects of Zoology, Botany, Comparative Anatomy, the History of Medicine, Hygiene, Medical Physics, and Clinical Medicine and Surgery.

"All these branches, it is true, are not crowded upon the mind during a single session, but are distributed throughout successive years; while clinical instruction, in both medicine and surgery, is reserved for the last year or two, after it is presumed the student has made such advances in his primary studies as will enable him perfectly to understand the lessons which are taught him at the bedside of the patient."

In reference to the time allotted for education, and the mode in which the examination of the candidate is conducted, we find the following remarks:—

"Instruction so full and so complete must of necessity require a lengthened period for its accomplishment: hence we find that in France and Austria the term of lectures is extended to a fraction more than four years; in Germany to three years and four months; in Great Britain to two years; while in the schools of our own country two sessions of four months each are alone sufficient to entitle the candidate to an examination. And if we extend our inquiries a step further, we shall find as great a contrast in regard to the examinations by which the qualifications of candidates are tested. Nowhere, we believe, except in England and America, is the qualification made to rest upon a single viva-voce examination; but throughout the period of the student's probation, repeated examinations are held at stated

intervals. By proceeding in this manner not only is the qualification more accurately ascertained, but certain branches are completely mastered before others have been commenced. Thus we learn from Mr. Surgeon Wilde's work on Austria and her Institutions, that at Vienna the student is compelled to undergo an examination at the end of every six months by each professor whose lectures he may have attended. During the fifth year, which completes his term of study, he is subjected to two other examinations, besides giving a detailed history of two or more clinical cases, and defending in public a thesis written in the Latin language. In Prussia four examinations are required. When the pupil has satisfactorily completed the third, the honours of the Doctorate are conferred upon him; but he is not yet permitted to enter upon the practice of his profession. To enjoy this privilege a more rigid ordeal must be passed: he is now called upon to demonstrate his *practical* acquaintance with the several subjects in which he has been instructed; to take charge of patients under the immediate inspection of his teachers; and, finally, to be questioned by eight of the most distinguished professors of the country in every department of medical science. By a recent decree of the University of France, it is now ordered that examinations shall be held at the termination of each year upon the branches which have been taught during the year: at the conclusion of the whole term of study a final examination is instituted, embracing the subjects taught throughout the whole course. A knowledge of clinical medicine and surgery is also required, and a thesis must be publicly defended."

The period required for education in England is here considerably understated. The English College of Physicians requires five years, the College of Surgeons four years, and the Apothecaries' Society three years of professional study before the candidate can be admitted to examination; and either of these periods contrasts favourably with the two sessions of four months (*i. e.*, a *studentship of eight months*) which qualifies the American graduate to present himself to his examiners.

To a certain extent the objection to the British system is well founded. The University of London has, however, broken through the ancient rule regarding a single *visd voce* examination. With very rare exceptions its examinations are as searching and as *practical* as need be desired.

In the United States there are no fewer than *thirty-eight* distinct institutions engaged in *teaching* medicine, and in *licensing* (!) candidates to practice. These institutions are quite independent of each other, and they appear to be practically independent of the legislature. Thus, observe the reporters,—

"The colleges themselves, resting secure in their chartered rights, may contend against any interference with their several systems of education; although we would fain indulge the hope that some among them stand prepared to listen to that voice which rarely speaks in vain—the voice of public opinion. This it is which now agitates the professional mind from one end of this Union to the other; and this it is which, if not now heeded, will not cease to reiterate its cry. Let but a very few even of our conspicuous institutions demand of their pupils a more extended course of study, and so far from their interests perishing in the attempt, we cannot doubt that an accession to their numbers will ere long reward their efforts. The possession of the diploma now no longer tests the qualification of the man; and it cannot be doubted that the large number of medical colleges throughout the country, and the facility with which the degree is obtained, have exerted a most pernicious influence. Numbers of young men have entered the profession who were not prepared for its onerous and responsible duties, who, after a longer or shorter period of disappointment, have served to swell the list of empirics, or to seek employment by means unbecoming the character of the physician or the gentleman. Nor can it well be otherwise, when each returning spring lets loose upon the community some twelve or thirteen hundred graduates, whose professional existence must depend upon the encouragement they receive."

The suggestion for the correction of these evils is that some of the more prominent institutions should of themselves at once proceed to raise the standard of preliminary and medical education. We quite agree with the reporters that great responsibility rests upon Professors.

"They stand as the guardians, more than any others, of the public weal; for every student who receives the diploma is furnished by them with *prima facie* evidence that he is prepared for that important occupation in which he seeks to engage. And if we admit the supposition that, in any case, he is not thus prepared, it will require the prediction of no seer to forecast the lamentable result. We believe the opinion defensible, that in no profession is it more difficult to estimate just conclusions, as to real merit, than in that of medicine. And hence there devolves upon those who are the most competent to judge, and who have the best opportunity for judging, a vast responsibility to see well to it that none but the truly meritorious should be admitted to the profession under their sanction. From the fact that society cannot in this respect adequately protect itself, the teachers in our medical schools should exercise a guardianship, the more watchful, a jealousy the more keen, and a firmness the more unyielding. With a determination thus expressed, to furnish the community with individuals who have gained the honors of the doctorate solely on account of their merit, it can not be saying too much when we assert that a reciprocal duty rests upon it—a duty which should ever lead to the protection and encouragement of those who have devoted their time and talents to its dearest interests."

"Unfortunately the principle of pecuniary competition in teaching and licensing has too great a sway in America and England to allow this appeal to have that just and universal influence which it should carry with it. There as here, the question too often is, not where the largest amount of instruction in a special branch of medicine can be obtained, but where a *certificate* of attendance, granted, as a matter of course, on the payment of the fee, can be procured at

the smallest cost. The following summary presents a curious picture of the mode in which doctors of medicine (not physicians) are manufactured in the United States. Out of thirty-eight colleges, the Committee obtained answers to their questions on medical education from nineteen.

"In summing up the information obtained from the nineteen colleges, in the order in which the questions were proposed, it was found that the number of students belonging to these institutions during the session of 1845-6 amounted to 2544, including 81 non-paying pupils; and that the number of graduates was 730; the number of professors varies from three to eight. It may be proper to remark that the lowest number here named is attached to the University of Virginia, where the usual branches of medical education are taught, and where the term of study extends throughout the period of nine months. If we exclude this institution from our table, the minimum is five and the maximum is eight. The time employed in lecturing also varies, eleven weeks being the shortest, and eighteen the longest period. Sixteen weeks, however, are devoted to the lectures in a large majority of the schools. The requirements for the degree of M.D. appear to be very general: the candidate must be twenty-one years of age, of a moral character good, his qualifications satisfactory, his thesis passable, and he must have attended two full courses of lectures, to be included within the period of three years' study. In some of the schools a practice of four years, and attendance on one course of lectures, is sufficient to entitle the individual to an examination. Branches are taught in some institutions which are omitted in others, and the manner in which these are distributed is by no means uniform. It appears without exception that the inquiry is made, previous to examination, whether or not all the requirements have been fulfilled, and in some cases unquestionable proof of the fact must be adduced. Evidence of having attended a course of clinical instruction is required in twelve of the colleges, while in seven it is not; and as to dissections, five render it obligatory, and the remaining fourteen are content to urge its recommendation."

From this it follows that the great object in most of the American Colleges is to create M.D.s, and not physicians! If the title be legally conferred, a knowledge of medicine is of no importance as a qualification for practice.

With this for the present we close our extracts and remarks. We shall take an early opportunity of returning to the subject.

Books.

The Cyclopædia of Anatomy and Physiology. Edited by ROBERT B. TOWN, M.D., F.R.S., &c. Parts XXXVIII, XXXIX, and XL. Longman and Co. 1850.

We are glad to perceive, from the rapid succession of the various parts, that this excellent work is approaching completion. We have now before us three parts, comprising numerous important articles under the letter T from *Testis* to *Urethra*. In Part XXXVIII, the elaborate paper on the *Testis*, by Professor Owen, is brought to completion: this portion of the paper is chiefly devoted to the comparative anatomy of these organs. It may be profitably consulted by all students of this branch of science. Two short papers follow; the one on *Temperament*, by the editor, and the other on the *Temporo-maxillary articulation*, by Mr. S. R. Pittard. The two next articles—on *Teratology*, by Professor Vrolik, and on the *Testicle*, by Mr. Curling—are the most important in the Part now before us.

Professor Vrolik has given a complete anatomical description of all the varieties of malformation and monstrosity hitherto observed. The paper is more of an anatomical than of a physiological character, and is profusely illustrated with wood engravings, which render the author's descriptions perfectly clear and intelligible. We observe that Professor Vrolik, who is a high authority in such matters, wholly disbelieves that deformations of the originally well-formed germ can take place as the result of a mental impression produced on the mother during pregnancy. He freely admits that the origin of monstrous births still remains very mysterious. The opinions

on the subject may be reduced to two—i.e., that monstrosity is due—1, to original malformation of the germ; 2, to the subsequent deformation of the embryo, by causes operating on its development. It is in reference to the latter point that the author asserts his disbelief that the mental condition of a pregnant female can have any influence on the embryo, except indirectly. We subjoin an extract containing the arguments of the writer against the view, which has been suggested, by some eminent physiologists:

"a. That malformations seldom, or perhaps never, agree with apprehensions or fears *a priori* of pregnant women (G. Vrolik, T. Zimmer, J. J. Plenck, and Burdach). On the contrary, it often happens that a woman who has once procured a malformation, and is continually troubled by the fear of another similar and occurrence, may become the happy mother of a second well-formed child.

"b. That the *fœtus*, even when a germ, is quite independent; transferred from the ovary into the uterus, it needs for its development a material intercourse with the maternal body, but no *organic* connection; for which reason it can be formed as well without as within the uterus, as in extra-uterine pregnancy; that it stands in no connection, either vascular or nervous, with the body of the mother, and that therefore it is improbable that her mental condition can have any influence whatever upon the form of the *fœtus*.

"c. That malformations occur likewise among the inferior animals—insects, testaceous animals, echinoderms—in which the development of physical life is very imperfect, and the oviparous generation of which must preserve the young from the influence of disordered maternal imagination.

"d. That in the case of twins, as the acephali specially show, one child may be malformed and the other in perfect condition, notwithstanding they were both exposed to the same influence.

"e. That more deeply situated organs, the very existence of which may be unknown to the pregnant woman, may be malformed; as, for instance, the heart, the intestinal tube, &c.

"If now, on all these grounds, I exclude the mental impressions of pregnant women from the ætiology of malformations, I do not mean to deny the influence which, by her somatic condition, the mother may exercise upon the *fœtus*. Thus, if in consequence of mental agitation her body were to suffer a violent shock, this might have a

prejudicial influence on the maternal transmission which takes place between her and the fetus, and the latter might thereby become morbidly affected. There are instances of its being the subject of intermittent fever (P. Russell); of sudden death, occasioned by frightful agitation of the mother (Wienholdt); of jaundice communicated by the mother (Kerckring); of small-pox (Jenner, Montgomery, Friedlander); of syphilis and scarlet fever (R. Lee); all derived from the mother. But all this is entirely different from the effect of mental impressions. It is a material result easily conceived, and of which physiologists need no further explanation" (p. 945).

We may remark of this article, that it displays deep research on monstrosities, and a thorough acquaintance with the subject on the part of the author. We think that its interest would have been heightened had Professor Vrolik, besides types and species, given us a short description of some of the remarkable monsters of modern times, such as the *Siamese Twins*, *Christina Ritta*, and the *Chinese Heteradelph A-ke*. The physiological condition of a monster which survives for months or years is quite as instructive as the determination of its abnormal peculiarities when dead. The physiologist desires to know how the disjointed machinery acted, if it acted at all; but on this point Professor Vrolik refers the reader to the general records of medicine. So far as the classification and anatomical description of monsters are concerned, there is probably no essay or treatise in the English language which can be more profitably consulted than this.

Mr. Curling's paper on the *Testicle* is divided into two sections; the one comprising the *normal*, and the other the *abnormal anatomy*. In the first section, which fills nearly forty pages of the number, the author gives a clear and minute description of the anatomical structure of the testicle and its appendages. As a specimen of Mr. Curling's mode of treating the subject, we quote his remarks on the passage of the testicle into the scrotum during fetal life. This is commonly called the *descent* of the testis; but, as the writer observes, if we consider the ordinary position of the fetus in utero, this term is obviously incorrect:—

"Between the fifth and sixth month of fetal existence—sometimes later—the tes-

ticle begins to move from its situation near the kidney towards the ring, which it usually reaches about the seventh month. During the eighth month it generally traverses the inguinal canal, and by the end of the ninth arrives at the bottom of the scrotum, in which situation it is commonly found at birth. The testicle, both during its passage to the ring and through the inguinal canal, carries along with it its original peritoneal coat, adhering by the reflexion of this membrane, during the whole of its course to the parts behind, in the same manner as whilst situated below the kidney. The testicle, therefore, does not pass directly and abruptly into a pouch prepared to receive it, but carries the peritoneum with it, continuing to be connected to the parts behind by the reflexion of the membrane, between the folds of which the vessels and nerves join the gland. In the passage of the testicle from the abdomen to the bottom of the scrotum, the gubernaculum, including its peritoneal investment and muscular fibres, undergoes the same change as that which takes place in certain of the *rodentia* at the access of the season of sexual excitement; the muscle of the testicle is gradually everted, until, when transition is completed, it forms a muscular envelope external to the process of peritoneum, which surrounds the gland and front of the cord. As the testicle approaches the bottom of the scrotum, the gubernaculum diminishes in size, owing to a change in the disposition of its areolar elements: the muscular fibres, however, undergo little or no diminution, and are very distinct around the tunica vaginalis in the recently descended testicle. The mass composing the central part of the gubernaculum, which is so soft, lax, and yielding, as in every way to facilitate these changes, becomes gradually diffused, and after the arrival of the testicle in the scrotum, contributes to form the loose areolar tissue which afterwards exists so abundantly in this part; the middle attachment of the gubernaculum, which may be traced to the dartos at the bottom of the scrotum, gradually wastes away, and soon becomes indistinct, though slight traces of this process often remain to the latest period of life. Thus, after death, in dragging the testicle of an adult out of the scrotum, by pulling the cord, the lower part of the gland, which is uncovered by serous membrane, is often found connected to the bottom of the scrotum by a band of firm and dense areolar tissue, which requires division with the scalpel. This band is the remains of the middle attachment of the gubernaculum" (p. 963).

In the section on the Abnormal ana-

atomy of the Testicle we have a very carefully digested essay, and one which will well repay the pathological student by its perusal. We know of no English surgeon better qualified to deal with this subject than Mr. Curling, and accordingly we find in the various paragraphs the condensed experience of an acute observer. Under the head of a normal anatomy the writer treats of congenital imperfections and malformations,—deficiencies of the vas deferens,—imperfect transition,—inversion,—atrophy, and arrest of development,—wasting,—hydrocele in its various forms and complications as it affects the testicle and spermatic cord,—hematocele, orchitis,—carcinoma,—cystic disease,—calcific deposits,—and varicocele. Among these subjects we find one of peculiar interest to the surgeon, because it is but little known, and we do not remember to have seen it described by any other writer: we allude to "*Inversion of the Testicle.*" Mr. Curling says—

"It sometimes happens that the position of the testicle in the scrotum is reversed, so that the free surface presents posteriorly, and the epididymis is attached to the anterior part of the gland, instead of to the posterior. The first case that I met with was that of a man who had a swelling of the right testicle, which puzzled his medical attendant. On examination I found this to be the epididymis thickened from chronic inflammation. I was able easily to trace the vas deferens proceeding to it along the front of the scrotum. The body of the testicle was unaffected, and its posterior edge was quite smooth and regular. The disposition of the left testicle was normal. On visiting the Hôpital de Midi in Paris, in April, 1849, M. Ricord showed me a case of epididymitis on the left side, in which the gland was thus inverted. He informed me that he had often met with this arrangement. I have since had two patients under my care, one of whose testicles was thus inverted. One was a lad in the London Hospital affected with epididymitis: the other was a gentleman who consulted me for chronic orchitis confined to the body of the testicle. The epididymis being unaffected, the inversion was less perceptible than in the three preceding cases. M. Maisonneuve, in a thesis published in Paris in 1835, I believe first called attention to this irregular disposition, which he states that he had met with many times upon the dead body, and upon the living; and he mentions what I remarked myself in the four cases just noticed, that the inversion was confined to one side.

Surgeons should bear in mind the liability to this disposition of the gland in making their diagnosis of the diseases affecting it" (p. 991).

The remarks, at page 998, on the frequent existence of spermatozoa in the fluid contents of the cysts of hydrocele are well worthy of perusal.

This article is brought to a conclusion by an account of the morbid anatomy of the scrotum, in which the subjects chiefly discussed are elephantiasis and cancer scroti. It will be perceived from this description that no part of surgical pathology which can throw light upon the diseases of the testicle and its appendages has escaped the author's notice. Of *cancer scroti* Mr. Curling observes that it

"occurs more commonly at the middle period than at any other time of life. In the majority of cases which I have met with, the disease occurred between the ages of thirty and forty. Those exposed, however, to the action of soot may become affected at a much earlier period. Mr. Wedd has figured a diseased prepucce and soot-wart on the scrotum from a boy aged fifteen; and Sir J. Earle witnessed a case of the disease as early as at eight years of age. It appears that the seeds of this malady are sown in early life, but in general do not germinate until they have remained for some time dormant in the system. What is the permanent effect on the scrotum produced by soot which thus renders it in certain individuals so peculiarly susceptible of a cancerous action at some distant period, we cannot explain; but that the soot, though the exciting cause of the disease, may in some instances be a remote one, is shown by several striking facts. It is known that persons who have been sweeps when young, but have abandoned the occupation, have afterwards been attacked with chimney-sweeper's cancer, although they have long been removed from all contact with soot. A sailor, between forty and fifty years of age, was admitted into the London Hospital with an ulcerated sore on the scrotum, presenting all the characters of genuine chimney-sweeper's cancer. The inguinal glands were indurated and enlarged, and subsequently ulcerated. He had been brought up as a sweep; but for the last twenty-two years, during which period he had served at sea, he had not been employed amongst soot in any way whatever. The disease first appeared in the scrotum about three years before. In this case, therefore, the injurious influence of soot must have been exerted nineteen years before the appear-

ance of disease, during which long period he was entirely recovered from the effects of its exciting cause. It has admitted happened after the morbid parts had been completely extirpated, and the wound healed, the patient having avoided further contact with soot, that the disease has reappeared as it were afresh, a second, and even a third time; not, however, in the matrix of the wound, but on a detached part of the scrotum. These, and similar facts, lead to the conclusion, that though abandonment of his occupation may render the adult chimney-sweep less liable to cancer, it by no means forms a satisfactory security against its occurrence." (p. 1016).

The *Anatomy of the Thorax*, by Dr. R. Hutchinson, is an article of considerable extent: it covers upwards of seventy closely-printed pages; and, while it commences in Part XXXVIII, it occupies nearly one-half of Part XXXIX. In this elaborate essay Dr. Hutchinson gives us, in the first place, an account of the different varieties of respiration in animals, and the structure of the thorax, as well as the circumstances which affect the conformation of the cavity. A curious series of engravings is here introduced, showing the exact space of the thoracic cavity, encompassed by each pair of ribs respectively. The reader will also find many tables and calculations, showing the relation of the external chest to the height or stature of individuals. After a very full description of the bony cavity, we come to the muscular parietes, and the circumstances which govern the motion of the ribs. The author goes into a minute description of the changes produced by respiration in the thoracic cavity, and illustrates his views by numerous wood engravings on the lever-action of the ribs. The various conditions which modify the act of respiration, its influence on position, the form of the thorax, the pulse, and other physiological states, are also fully described.

The two following articles, on the *Thymus* and *Thyroid Glands*, are from the pen of Dr. Handfield Jones, an indefatigable pathologist, who has contributed many valuable papers to the pages of this journal. The human and comparative anatomy of these ductless glands is given with sufficient minuteness and care to put the reader on a level with all that has been hitherto ascertained with respect to them. After remarking that, owing to the conditions

which chiefly affect the size and repletion of the *thymus gland*, it must be regarded as a very exact exponent of the state of the intricate processes generally, or a delicate barometer of nutrition, Dr. Jones gives us the following speculation respecting its probable function in the economy:

"Now if one of the organs which belong to the class of ductless glands have for its function to act as a living attractive recipient or reservoir for the blood as mass, may not another fulfil its destined purpose by leaving as a medium for that part of the blood which ministers to nutrition, perhaps for the plastic element of the liquor sanguinis, in particular? When such plastic material is in superabundance in the circulating current, a quantity of it passes off, and solidifying in the thymic cavities, assumes that most universal of all organized forms, the form of nuclei. When there is again a demand for such material, the solidified particles would again liquefy, and re-enter the impoverished blood. It is not difficult to understand that such a function may be most necessary during the period when growth is most active, the supplies of nourishment most frequent, and the waste of the tissues most rapid; but that as the several nutrient processes, both of the assimilative and destructive kind, attain to more steadiness and equilibrium, diminishing somewhat in their intensity and rapidity, but increasing in real strength, firmness, stability, and perfection (one is obliged to use somewhat metaphorical language); it may no longer be requisite, and the organ will therefore undergo a gradual atrophy." (p. 1101.)

Of the use of the secretion of the *thymus gland* physiologists are at present unable to give any satisfactory explanation. Dr. Jones quotes Mr. Simon's view, that this organ serves as a diverticulum to the cerebral circulation; but, as he justly observes, further investigations are required before this can be received as a true solution of the physiological difficulty.

In the section on the *Thyroid*, Dr. Jones devotes a short space to an account of its morbid anatomy, and the alterations of structure which the gland undergoes.

Passing over a short article on the *Tibio-fibular Articulation*, by Dr. M'Dowal, we come to an essay on the *Tongue*, by Mr. H. H. Salter. The human anatomy of this organ, including the microscopic characters of its muscular fibres, are well described, and illus-

trated by the aid of wood-engraving. The 39th Part closes with the account of the papillary structure of the organ; and, in continuing the subject in the 40th Part, the author gives us a description of the comparative and morbid anatomy of the organ.

We next come to an admirably-written article, on the physiology of *Tæch*, from the pen of Dr. W. B. Carpenter. The author has shown great care in the preparation of this paper, and has duly noticed the observations and discoveries of Weber, and other physiologists. It is not commonly known to what degree the sense of temperature is influenced by the extent of surface on which the impression is made, or that the *left* hand is in most persons more sensible to variations of temperature than the right.

Every one is familiar with the fact that hot water in which a single finger may be held without inconvenience, will be felt intolerably scalding when the whole hand is immersed in it. And it has been shown by Professor Weber, that if one vessel of water be heated to 98° and another to 104°, and the whole of the hand be immersed in the former, while the finger alone is immersed in the latter, a wrong judgment of their relative temperatures will be probably given, that which is really the cooler being pronounced the hotter, on account of the larger extent of surface on which it acts. This mistake was made in some of his experiments, when the difference was as small as eight degrees, the cooler water being at 98°, and the hotter at 106°, and yet the former being esteemed the hotter. So, again, the immersion of the entire hand enables minute differences of temperature to be detected, which could not be recognised by the immersion of a single finger.

By the former method, a difference of only one-third of a degree may be distinguished; the entire hand being immersed, repeatedly and successively, in two vessels of water, differing only that much in their relative warmth. But it is remarked by Professor Weber, that these minute differences are best detected when the medium examined does not fall short of, or exceed very considerably, the usual temperature of the body; just as the ear can best perceive a difference of tone in sounds which are neither very acute or very grave.

It is a remarkable fact, discovered by Professor Weber, that the left hand is in most persons more sensible to variations of temperature than the right. Thus, when the hands of a person lying in bed, and

having exactly the same temperature, are plunged simultaneously in separate vessels of hot water, the left hand is believed to be in the hotter medium, although the water in which it is immersed is really one or two degrees colder than the other. This difference is the more remarkable, as the power of tactile discrimination is usually greater in the right hand; and it is attributed by Professor Weber to a difference in the thickness of the epidermis, the left hand usually having a thinner epidermis than the right, especially in the palm, because it is less used. But this will only apply to the hand; and since (as will be presently shown) we possess a greater power of discriminating warmth through the entire surface of the left side than through that of the right, it would seem much more probable that there is an original difference in the tactile endowment of the two sides respectively. (p. 117.)

The section on the *Improbability of the Sense of Touch* will be found especially interesting in reference to the education of the blind.

The two remaining articles are entitled *Tæchæ* and *Ostrææ*. Of the former, which is by Mr. A. R. Jones, we shall only remark that it is a very learned essay on the general and minute anatomy of molluscous animals; while the latter is the commencement of an article important to the practical anatomist and surgeon, from the pen of Mr. John Adams. The present part contains only the account of the anatomy of the urethra in the male—its length, direction, and structure. We shall reserve our opinion of it until the appearance of the next part, which is promised for November.

Whatever objections may be made to certain serial publications on the ground of want of punctuality, we do not think that they can be fairly applied to the *Cyclopædia of Anatomy and Physiology*. Any one part shows that it has required immense labour and industry for its preparation; and it is highly creditable to the editor and contributors that, without impairing the value of the articles, the parts have appeared at such comparatively short intervals. In our next notice we hope to be able to announce the completion of one of the most valuable works on anatomy and physiology in the English language.

Physician and Patient; or a Practical View of the Mutual Duties, Relations, and Interests of the Medical Profession and the Community: From the text of WILLIAM HOOKER, M.D. Edited by EDWARD BENTLEY, M.D. &c. Small 8vo. pp. 303. London: Bentley. 1890.

THE topics discussed in this little work are:—

1. The uncertainty of medicine; its causes and its extent.

2. Skill in medicine; in which are shown the advantages of the possession of comprehensive principles, and the careful investigation of every circumstance connected with each case.

3. *Popular errors.* In this section the over-estimating the value of remedies is exposed, and the important truth expounded "that it takes as much knowledge to know what not to do, as it does to know what to do." It is also here shown how "those who drug their patients freely are more apt to satisfy the mass of the community than those who place reliance upon positive medication."

4. *Quackery*; the consideration of which embraces homœopathy, popular medical literature, hydropathy, &c.

5. Good and bad practice. The incompetence of the public to judge between these, the frequent success of ignorance and loud-talking assumption, are herein set forth.

We quote the points of distinction which the author gives between the *good and bad practitioner*. The skilful physician has—

"1. A less (smaller?) number of fatal cases in proportion to the whole number that come under treatment.

"2. A less number of bad cases, because he avoids converting light cases into grave ones, and succeeds in arresting disease in many cases in its very commencement.

"3. His patients have commonly shorter sicknesses.

"4. They are in a better condition after they have recovered, less apt to have bad results left behind, and less liable to disease in future.

"5. He has a less number of patients, and a smaller amount of sickness, in the same number of families."

These statements are undoubtedly true of the skilful physician, and they are also the test of his uprightness; for the man of honourable principles is

often put in contemptible comparison with the man of drugs and "medical manoeuvring," and will probably have to submit to present disadvantage in the comparison by those incapable of seeing through the frauds of the latter.

6. Theory and observation; their use and abuse.

7. Popular estimates of physicians. "There is no class of men (observes the author) whose talents and attainments are so erroneously estimated by the public as those of physicians." Of the truth of this statement daily experience must convince any one; and the most sanguine beginner in practice, albeit the most competent of all men, may live to find that he has never been duly appreciated by a "discerning public."

8. Intercourse of physicians. We could desire that a large proportion of the profession would carefully study and act upon the contents of this chapter. More harmony and less jealousy would ensue. The editor subjoins the American code of medical ethics, which, however, we may notice, is compiled in the main from "Percival's Medical Ethics." 9. Interference with physicians. The public may read this chapter with advantage. 10. Mental influence of mind and body in disease. 11. Influence of hope in the treatment of disease. 12. Truth in our intercourse with the sick. 13. Moral influence of physicians.

These four chapters involve the discussion of subjects closely allied, and of the very first importance to every conscientious practitioner.

The last chapter (14) treats of the trials and pleasures of a medical life. We receive it as a wise dispensation that the pleasures come first, and that the pains follow. No pleasure in a medical after-life can be put in competition with the delight which attends the incessant acquisition of fresh knowledge experienced during the diligent employment of the period of preliminary and hospital studies. Many would otherwise be deterred from encountering only toil, anxiety, and too often ingratitude, from "those upon whom they have conferred the highest favours." That "there is not as much gratitude in the world as is commonly supposed," and that "there is often much show of gratitude, but it is seldom, though sometimes, more than mere show," are truths which experience soon teaches. A higher

motive than that of the mere hope of reward is required for the performance of our duties, or they must be irksome indeed. After all, perhaps, it may be as well that we should, when smarting under the ingratitude of patients, put the inquiry to ourselves, whether we are as grateful as we should be to the Giver of all Good, and whether we have really made the best use of the "talents" which He has entrusted to our charge.

Dr. Hooper's treatise is very comprehensive, as will have been perceived from the preceding summary of its contents. Dr. Bentley, in editing it, has judiciously exercised the power of abridgment and condensation. To all interested in the discussion of the ethical relations of the profession, (and who among us is not interested therein?) we doubt not that it will prove acceptable.

On the Development of the Retina and Optic Nerve, and of the Membranous Labyrinth and Auditory Nerve. By HENRY GRAY, M.R.C.S. From the Philosophical Transactions. 4to. pp. 12, and Plates. London: Taylor. 1850.

THIS memoir demonstrates the mode of evolution of the essential parts of the visual and auditory apparatus. The author states, that so far as he is acquainted, many of his observations have not been made before; at the same time that they confirm the account given of the structure of these parts in his *Prize Essay on the Anatomy and Physiology of the Nerves of the Human Eye*, contained in the library of the College of Surgeons, but unpublished.

Mr. Gray traces the development of these organs from their first appearance in the anterior cephalic cell, at the thirty-third hour of incubation, up to its condition on the seventh day. Hence he concludes that the retina is originally a protrusion from the anterior cephalic cell, being hollow, and communicating with its cavity, becoming separated from its parent cell in proportion as the brain is developed, until it comes into relation with other parts of the visual apparatus, as described in this memoir.

In like manner the author traces the membranous labyrinth and auditory nerve to be a protruded portion of brain modified to adapt it for its particular connections and functions; thus,

"The membranous labyrinth, like the retina, is a protruded portion of the brain, being hollow, and communicating with the ventricular cavity from which it arises. As the progressive development of the brain proceeds, the auditory sac becomes more elongated and of a pyriform shape, the dilated portion being analogous to the vestibule, the contracted subular portion to the auditory nerve; this subsequently becomes solid, and the cavity in the vestibule does not communicate with the ventricle from the wall of which it is an offset" (p. 12).

The several steps of the development of these organs are illustrated by steel engravings. The entire paper reflects credit upon the author's industry and accuracy as an observer.

A REMARKABLE CASE OF PHOTOPHIA.

THE patient was a nervous, excitable lady, thirty-eight years of age; tall and thin, of an active temper, and possessing a strong understanding. She had suffered frequently from hysterical pains, flatulency, &c. and tape-worms. The patient first complained of weakness of the eyes in reading, &c. with *musca volitantes* and intolerance of light. She became very anxious on this account, fearing amaurosis. The eye showed nothing unusual but that the iris was rather less contractile than natural. Soon afterwards, photopsia followed; sparks and flashes of light constantly followed. Neither day-light nor candle-light could be borne, and lastly any degree of light caused such intense pain, that the room was kept entirely darkened, everything of a light colour was removed, the plates, spoons, &c. were changed for articles made of dark brown wood. Every object seemed to glow with a bright light, a continual fire appeared to her on all sides, suns seemed to glare above and below, from which she could not withdraw her regard, the whole street appeared in full glow, though three or four folds of curtain hung before the windows. Sleep was entirely absent; the night was passed in anguish, the day in delirium, so that it was clear, that not only the retina, but that the brain also, was the seat of the disease, which produced the optical delusions. The pulse was generally quick, the tongue was clean, the digestive functions unimpaired. This condition lasted for eight weeks with some little amendment. The symptoms then gradually subsided. With the aid of blue spectacles light became tolerable. Sedatives, antispasmodics, counter-irritation, were employed in the treatment of this case.—*Casper's Wochenschrift*, x

Proceedings of Societies.

ACADEMY OF MEDICINE, PARIS.

Oct. 7, 1880.

Influence of Ergot of Rye on Fetal Life.

M. DANYAU (in the name of a commission composed of MM. Orfila, Adelon, Villeneuve, Merat, and Danyau) read a report in answer to a letter from the Prefect of the Seine, which therein sought the opinion of the Academy on the following question:—"What may be the influence of ergot of rye on the lives of infants, and on the maternal health?" This officer having observed an almost regular annual increase in the number of still-born children, and having been informed on medical authority that in a large number of these cases ergot of rye had been administered during labour, he was struck with the coincidence. Having also learnt that the same drug is extensively employed to produce abortion, he had thought it advisable to consult the Academy before having recourse to any stringent measures with a view to putting a stop to these evils.

It was also submitted whether it is not desirable that the use of ergot should be forbidden to midwives, and that, by means of the medical press, greater prudence in its employment should be urged upon practitioners.

The reporters observed that ergot should be used only, where to facilitate labour, the pelvis being large, the soft parts well formed, an inflammatory state absent, the dimensions and position of the fetus normal, the os uteri dilated and soft, the perineum not offering rigidity,—the labour is prolonged from no other cause than the insufficiency of the uterine contractions. The reporters point out the evil consequences of the violation of these conditions, and state instances of the rash employment of ergot where one or more of them have been wanting or reversed. No careful or skilful practitioner, observe the reporters, will administer this drug until having satisfied themselves that all circumstances are favourable, and that other and simpler measures have failed, nor lose sight of the fact that the contractions induced by ergot are of a tetanic character, and capable of suspending the utero-placental circulation, thereby inducing a state of asphyxia more or less complete. The state of the fetal circulation after the administration of ergot affords an indication whether the labour should be permitted to be concluded by the uterine contractions or be terminated by

the forceps. If the fetal pulsations become either unusually frequent or slow, and at the same time irregular and feeble, the reporters state that the moment is arrived for extraction, and that to leave the case to the expulsive powers of the uterus would be attended with fatal results.

With due precaution, under the vigilant control of a practised ear, the dose of ergot being moderate (i. e. 2 or 3 grammes, = 30-45 grs. R&G), given at intervals of twenty minutes, or half an hour, the reporters pronounce that ergot may be given with safety to the fetus. The period of safety, however, is not of long duration; at most two hours; frequently one hour's duration of the labour under the full influence of the drug is sufficient to produce evil effects; the practitioner should therefore be capable and fully prepared to extract without loss of time.

To the appreciation of all these circumstances, and the adoption of the appropriate and judicious measures, midwives are incompetent; by readiness and ignorance they therefore often cause still-births, or, where sensible of the condition of the patient, they send for medical assistance, much valuable time for action is lost.

With reference to the second question—viz. the influence of ergot on the maternal health—the report states that with a few exceptional cases no decidedly poisonous influences have been shown to be exerted by its obstetric administration. At the same time the report notices the accidents that have ensued to the mother from its improper or ill-timed exhibition, e. g. rupture of the uterus, and laceration of the vagina and perineum, the last being more frequently met with now, than formerly.

The reporters state, that entire liberty of action must be left to midwives for the administration of ergot in cases of post-partum hemorrhage. They speak in the strongest terms of praise of the advantages of full doses promptly given when hemorrhage occurs after delivery, or even when administered a short time before birth, in cases where flooding may be anticipated. To give permission, on the one hand, to midwives, and to refuse it on the other, is obviously attended, as the reporters observe, with great difficulty, and must be the source of uncertainty. The only course which remains is to inculcate thoroughly the principles which should guide their conduct in its use, and control their practice.

The reporters add, with regard to the production of abortion by ergot, that there is no reason to believe that it is capable of influencing uterine contractions before the full period, unless they have been previously excited by some other means.

The reporters express regret that there exist no means of preventing the employment of those means of exciting uterine contractions, and the subsequent administration of ergot, for criminal purposes. For the determination of the direct effects of the drug, thus used, on the maternal health, facts (the reporters remark) are wanting.

The Report closes with the following conclusions:—

1. Ergot of rye, administered imprudently, causes death to the fetus and injury to the mother.

2. That in the present state of the law it is not possible to interdict the use of ergot by midwives, and that its interdiction would in some cases be attended with serious disadvantages.

3. That it is desirable to increase the education of midwives, and strictly to define their duties.

4. That the Academy, not having any control over the medical press, cannot compel the publication of the precautions suggested by the minister; but confines itself to the publication, in the medical journals, of those parts of the Report which seem calculated to answer the same purpose.

The Report was adopted after a short discussion. It was supported by MM. Moreau, Velpeau, Gibert, and Rochoux; and opposed by M. Gerdy.

ACADEMY OF SCIENCES, PARIS.

On the Employment of Mercurials in the Treatment of Typhus Fever. By M. BROQUELLE.

IN 1847 M. Serres presented to the Institute a series of communications tending to throw light on the nature and treatment of typhus fever. M. Serres was desirous of more particularly calling the attention of the medical world to two points in the history of this disease, at the same time pointing out a new mode of healing intended to prevent the formidable accidents, to moderate the most dangerous symptoms, and, lastly, to convert the severest form of typhus into a disease of a much milder character. Having been appointed by the hospital administration to replace M. Serres provisionally at the Hôpital de la Pitié, I thought I could not do better than continue the mode of treatment he had adopted with regard to typhus fever, and submit it to a new course of experiments. It is the result of this treatment that I now submit to the judgment of the Academy.

It will be as well here to recall in what

the mode of treatment adopted by M. Serres consisted, and the results which he obtained. The treatment by means of mercurials consisted in the employment of the black sulphuret of mercury (ethiops mineral) internally, in doses which varied from gr. 0.60 to 2 grammes,* and in the external application of mercurial ointment (Neapolitan ointment), by means of friction on the abdomen, in doses varying from 15 to 30 grammes daily: this treatment was continued eight, ten, twelve days, or even for a longer period, until the cessation of the characteristic symptoms of the disease. The results of this treatment were thus announced by M. Serres to the Institute:—The employment of black sulphuret of mercury in the doses above mentioned might be continued for eight, ten, or twelve days, without salivation being produced; and at the end of this time, whenever salivation was produced, it never manifested itself with such intensity as to cause any bad results to be apprehended.

The result of the mercurial frictions on the abdomen was the immediate disappearing of the red spots, and the rapid subsiding of the swelling. Under the combined influence of these two means the diarrhoea was modified; the frequency of the pulse considerably diminished; the fever subsided; the headache and the delirium were much relieved. The duration of the disease was not much shortened; it usually existed about the ordinary period, for three or four weeks; but it passed off without any serious consequences, and especially without the adynamic form of typhus becoming very severe. Such is an abridged description of the conclusions arrived at by M. Serres. Before giving any account of the results which I have obtained in fifteen cases of typhus fever, all very serious cases, and submitted to the above treatment, I should state that I have rigorously followed the formula adopted by M. Serres, and that the therapeutic results obtained, which have even been more successful than those of M. Serres, cannot be attributed to any other source.

The black sulphuret of mercury was given to the patients on their entrance into the hospital. The dose was at first one gramme daily, in powders or in pills, made up with mucilage. This dose was given five or six times. If no improvement took place at the end of two or three days, the dose was increased up to 1½ gramme, or even to 2 grammes; but this quantity was never exceeded. The employment of the sulphuret was suspended as soon as the patient showed signs of convalescence: in no case were the symptoms of salivation so

* One gramme is equal to about fifteen grains

strong as to require premature suspension of the treatment.

The frictions on the abdomen with mercurial ointment bore relation to the quantity of sulphuret internally exhibited; with 1 gramme of sulphuret 16 grammes of ointment were prescribed to be applied at two frictions, with the application of poultices, to facilitate the absorption; with 1½ grammes of black sulphuret 24 grammes of ointment; and with 2 grammes of sulphuret 30 grammes of ointment were applied, each at three frictions. Every other day the abdomen was well cleaned with soap, to facilitate the absorption of the ointment.

The additional means employed were, 1, ice, Seltzer water, lemonade; 2, simple or slightly laxative enemata when any constipation existed; 3, in the intermittent form of the disease, of which there were four cases, I gave, in addition to the sulphuret, 0.25 to 0.30 grammes of musk daily. This was discontinued when the delirium and agitation diminished. Fifteen patients, labouring under severe attacks of typhus fever, were submitted to the above treatment; of these ten were men, and five were women: the ten men were of the respective ages of 16, 18, 20, 22, and 36 years; the women 15, 18, 20, 21, respectively.

The ten men were attacked with typhus fever presenting the following forms:—four, the ordinary abdominal form, with stupor, headache, &c.; five, the most severe adynamic form; one, the intermittent form, with delirium and violent agitation. The five women presented two cases of the intermittent form; one of the intermittent adynamic form; and one the adynamic form alone; and one the ordinary abdominal form. These fifteen patients had all been a certain time in a more or less severe state of suffering previous to admission into the hospital. In each case the treatment was commenced on the day after their admission. The following is an account of the effects produced on the principal symptoms, and on the ensemble of the disease:—

Fever.—Under the influence of the first doses of the black sulphuret, and the first frictions, the skin became less hot and dry, and in some cases was slightly moist from perspiration. At the same time, the pulse diminished in strength and frequency: this effect also was produced in the only case which terminated fatally in consequence of intestinal perforation.

The tongue, at first dry and rough, and the gums and lips of a sooty colour, did not lose their characteristics until salivation supervened. Salivation occurred in twelve cases; nine were produced in two cases which were equally as well cured: in

each of these two cases the tongue remained quite dry until the fever ceased. In the case of intestinal perforation the tongue remained dry throughout. In the twelve cases in which salivation was produced it took place in two cases on the sixth day of treatment; in three cases on the seventh; in four cases on the eighth; in one case on the twelfth; and in the other case on the thirteenth day. Except in one case where the salivation and swelling of the gums took twelve days to remove, and was very considerable, it was in all the others slight, and of but little importance. In every other case it did not continue more than four or five days, and required no particular treatment.

As for the salivation, the following propositions may be considered as established:

—1. In the case of ordinary typhus fever of moderate severity, salivation shows itself more rapidly: it is stronger, and continues longer during convalescence. In general, it announces the approaching arrival of convalescence. 2. In the most severe cases, salivation takes longer time to produce: it is generally less intense, and continues no longer than the first days of convalescence: it precedes by a few days the cessation of the fever, and constitutes a sign of approaching restoration to health. Sometimes, however, it does not appear until the entire cessation of the febrile symptoms. 3. In the extremely severe cases salivation is with great difficulty produced; and as long as it does not arrive, more or less dangerous consequences may be feared. In such a case we must not be discouraged, but continue for a longer period increased doses of the sulphuret and the mercurial frictions. 4. In some cases of moderate severity salivation does not occur at all. In each of the fifteen cases the treatment was always continued, not only up to the appearance of the salivation, but up to the time of the cessation of fever, and the amelioration of all the symptoms.

The swelling of the abdomen, except in the case of intestinal perforation, has constantly diminished with great rapidity, and that from the commencement of the treatment: it is to the combined influence of the employment of sulphuret and mercurial ointment frictions that the result must be attributed.

As to the *stools*, out of the fifteen patients there was constipation in two cases, which enemata alone were able to remove: the sulphuret was without effect in removing it. In two other cases the sulphuret produced diarrhoea, which did not previously exist, and which, since its employment, induced one or two liquid stools daily. In three cases the characteristic diarrhoea which occurred on their admission sensibly

diminished action as a few doses of sulphuret were taken. In eight cases the diarrhoea neither increased nor diminished: it took its course, and diminished only at the same time that the other characteristic symptoms of the disease abated.

In every case the mercurial frictions were made on the abdomen, the red spots disappearing in the space of 24 to 36 hours; no others appeared but those which occupied the base of the thorax, on the breast, or elsewhere.

As to headache, no particular effect was remarkable in either of the cases; in fact, it did not constitute any leading feature.

In three cases where violent delirium existed (infopatientia) from 26 to 30 centigrammes of musk was combined with the black sulphuret, and this dose continued until the cessation of the symptoms. In these four cases the improvement was rapid, and five days was the longest period in which it was found necessary to exhibit the musk in combination with the sulphuret. In one case it was found advisable to use cold water applications to the head; the delirium was very violent, and a strait waistcoat was applied.

As respect of stupor in the comatose state did not disappear until an improvement had manifested itself in the other symptoms; the sulphuret produces no immediate effect.

The same may be said of the cough, &c., and there were no apprehensions of any serious consequences to the chest. The patient must perhaps be excepted who fell a victim to intestinal perforation on the twentieth day of the disease, and who at his admission presented the characteristics of very intense bronchial engorgement.

On this symptom the mercurials acted only by the influence which they exercised on the ensemble of the disease. In no case were there any serious hemorrhages.

The duration of treatment was in four cases seven days, in three cases eight days, in one case nine days, in three cases ten days, and one each 11, 12, 15, 16, and 17 days respectively; the mean time was 10 days. The minimum quantity of black sulphuret employed for the whole treatment was 7 grammes, and the maximum 26 grammes; the minimum quantity of mercurial ointment employed was 112 grammes, and the maximum 360 grammes; the mean quantity was 18·30 of sulphuret, and 300 grammes of mercurial ointment.

The total duration of the disease varied from 12 to 23 days; the mean duration of the disease—that is to say, of the fever—was 16 days.

Duration of Convalescence.—In all the cases this was very simple, proceeding uninterruptedly, and without any relapse.

In the case of one woman attacked with a most intense adynamic fever, there was sloughing of the sacrum; which required a month to cicatrise. Omitting this case, and reckoning from the time of the cessation of the fever, the patients remained in the hospital from eight to twenty-three days.

On the Presence of Casein in the Blood.

M. PAVEN read a communication from MME. Natalis Guillot and Felix Leblanc, on the presence of casein in a soluble state in the blood during lactation.

MM. Demas and Ouhours had previously pointed out the presence of casein in a male patient, and had given an analysis. M. Demas endeavoured, but unsuccessfully, to detect the presence of casein in the blood of sheep during the time of suckling; but up to the present time no person had succeeded in isolating this product of the organism from the others which circulate with the blood in females during lactation. MM. Guillot and Leblanc have proved that the serum of the blood of two nurses on which they experimented, after having been deprived of albumen by heat and by filtration, furnished an abundant white precipitate when boiled with acetic acid. The quantity of casein appeared to bear a relation to the proportion of albumen contained in the serum. In the blood of new-born children they could not detect the presence of casein.

Hospital and Infirmary Reports.

KING'S COLLEGE HOSPITAL.

Excision of the Knee-joint.

THIS rare and formidable operation has lately been performed at King's College Hospital by Mr. Ferguson. We shall give a detailed account of the case, which has unfortunately terminated fatally.

The patient, who was a young man aged 21, had been in very good health until about three years ago, when he had a fall upon his left knee. In consequence of this the part became swollen and inflamed, and he was compelled to lie up, and undergo a course of treatment for some months, at the expiration of which period he was much relieved; but shortly afterwards the bad symptoms recurred, and he was obliged to submit to treatment again. About nine months ago he became crippled, and has been laid up since. As the disease of the knee-joint did not appear to decrease in severity, the surgeon under whose care he was in the country considered that ampu-

tation would be necessary, and for this purpose he sent him to the hospital, to be placed under Mr. Ferguson's care.

On examination the knee was found to be in the following condition:—It was enlarged to some extent, being about two inches more in circumference than the other. The limb was kept in nearly a straight position, and the patient could not alter this. When the articulating surfaces of the joint were pressed together, considerable pain was experienced; but if the part were kept perfectly quiet the patient suffered comparatively little pain. The skin was entire, and there were no ulcerations or sinuses; but the existence of several cicatrices evidently showed that the integument had been considerably involved at one period: the leg below was not wasted to any extent. The general health of the patient was neither weak nor robust; it was not so much influenced by the local disease as is frequently seen in such cases.

After having carefully examined this case, Mr. Ferguson gave it as his opinion that there was some serious disease at the knee-joint; but yet he did not consider that it was sufficiently bad, considering the patient's general health, to warrant the removal of the entire limb. He, however, thought that it was a very fit opportunity to endeavour to remove the diseased parts, at the same time to save the limb more or less useful to the patient. In fact, he proposed to the patient that he should perform the operation of excision of the knee-joint, to which a ready assent was given.

On the 20th of July the man was brought into the theatre, and placed under the influence of chloroform. An incision was made on each side of the joints in a straight direction from each condyle of the femur to below the heads of the tibia and fibula. A transverse cut was now made across the front of the joint below the patella. The ligament was divided, and this, together with the bone, was then turned upwards; by this means the joint was fully exposed. The ligaments connecting the articulation were then divided, and the soft parts were cleared well away from around the lower extremity of the femur. A common saw was then carried across this bone just at the upper part of the condyles, and the section was rapidly made. The knife was then most cautiously used, and the soft parts were separated from the bone behind, and the latter was removed. After this was effected the soft tissues were well cleared away from around the upper part of the tibia and fibula, and the heads of these bones were removed. Several arteries bled freely, and it was necessary to use many ligatures. The wound was brought

together by means of sutures, and wet dressing was applied. The limb was placed in a box made for the purpose, which was furnished with a foot-board, and with lateral flaps, which could be closed or opened at pleasure.

On looking at the surfaces of the joint, they were found to be much diseased—deprived of their cartilage. A considerable quantity of pus escaped when Mr. Ferguson cut through the joint in the operation.

On the day after the operation the patient appeared to be doing well. There was a freedom from anxiety on the part of his countenance. He did not complain of pain in the wound. There had been some bleeding from it, which had been checked by cold water. The pulse is very rapid—180. He is taking beef-tea and milk.

22d.—He has had some attacks of shivering. His countenance is slightly mallow. His pulse still continues very quick: it is 160 in the minute. He has had no vomiting. He complains of some pain in the head. The respirations are very quick, but he has no cough.

26th.—Rigors have occurred from time to time, yet there is no very marked symptom to produce uneasiness, with the exception of the condition of the pulse: it is 168 in the minute. The respiration is also very rapid; but the tongue is perfectly clean and moist. He has no vomiting, and he takes his nourishment well.

28th.—He appears to be somewhat better than he has been. The pulse is not so rapid, and the wound, which was looking unhealthy, and was in a sloughing condition, looks better. Granulations of a florid colour are rising up on each side. Diarrhoea, however, has set in. To take detection of logwood after each liquid stool.

29th.—This patient rapidly sank at 1 this day, after having passed a very fair night. He had another severe rigor yesterday. The diarrhoea continued unchecked, and his pulse, which had diminished in frequency, became very rapid again. Some hæmorrhage took place from the wound the evening before death.

On a post-mortem examination of the body being made, the internal organs were first examined, but they were found to be in a perfectly healthy condition. The only abnormal appearance to be found was within the large intestine, at the situation of the sigmoid flexure of the colon. Here there was a well-marked patch of congestion, about the size of the palm of the hand: this most probably was the seat of the diarrhoea.

The femoral, epaenous, and iliac veins were next carefully turned up and examined; but there was not the least altera-

ties from a healthy condition. When the wound itself was examined, the cause of the symptoms and of death was then found. The lower part of the femur was deprived of its periosteum for about an inch and a half in extent. The portion of the periosteum which covered the bone above was very easily stripped off, and on doing this a large drop of pus was seen subjacent to the membrane. Several inches of the extremity of the bone was then removed; and on a perpendicular section being made through it, pus was found to be diffused both through its cancellated texture, and in its medullary cavity as well.

On looking into the substance of the head of the tibia the bone was found to be in a somewhat similar condition, although to a less extent.

Here, then, was the cause of the peculiar symptoms which appeared after the operation, and of the fatal termination which ensued. Acute or suppurative necrosis, the result of the injury done to the bone during the operation, destroyed the patient. That there was some serious mischief going on somewhere, was evident soon after the operation. One symptom alone caused uneasiness in the minds of those who watched the case, although it was a symptom by itself, and this was the excessive rapidity of pulse, which kept up, and even increased in frequency. There was no obvious explanation for this circumstance; there was no undue febrile excitement, no inflammation about the wound apparently, no erysipelas; the tongue was perfectly clean, and the patient took his nourishment well. This one circumstance, therefore, indicated coming mischief, as it almost certainly does. The surgeon may be nearly sure that something is going wrong, although general appearances are favourable, if after a severe operation the pulse beats with unusual rapidity, and especially if the frequency continues or increases, there being no obvious reason for it.

On the third day a new and formidable symptom, especially dreaded by the surgeon, showed itself—rigors occurred. It was now feared that inflammation of the veins had taken place, or that pus was being deposited in some of the important organs of the body. The rigors ceased for a time, and the pulse became somewhat less frequent, and it was hoped that the patient might rally; but these hopes were of short duration. Diarrhoea, a frequent concomitant of purulent depôts, set in, and rapidly ended the patient's life.

Acute necrosis of the bones, which have been sawn through in an excision or amputation is by no means of frequent occurrence; in fact, it may be looked upon as a

rarity. We do not remember, amongst the very numerous operations on bones that we have seen, to have witnessed a case where a fatal result has occurred from such a cause. It is one of those unfortunate events which are liable to happen after violence has been done to a bone, whether in operation or otherwise; but it is impossible to say whether its occurrence in this case was owing to the peculiar nature of the operation put in force, and whether it would not have equally occurred if simple amputation had been performed. We believe that it may have followed; for no more mischief was done by the saw in the excision than would have been necessary in an amputation. It would have been needful to saw through the substance of the bone either way; and, if the section had been made through the shaft of the bone itself, there would have been more likelihood of necrosis occurring; for every surgeon is aware that necrosis is more likely to attack the shafts than the extremities of long bones; and for this very reason an eminent surgeon has recommended that amputation of the thigh should be performed, if possible, through the condyles of the femur rather than at the lower third in the ordinary place.

We were sorry, but not surprised, to see, by referring to the September number of the Edinburgh Monthly Journal, that some very ill-natured and undignified remarks have been called forth by the occurrence of this operation, and by the unfortunate termination of the case. The ill-favoured and anonymous critic has the bad taste to preface his argument against this operation as follows:—

"In the *Lancet* of the 27th July a student complains of having been unable to witness an operation at King's College Hospital on the 20th of that month, from the area of the theatre being occupied by three and twenty surgeons. It now appears, from the "*Mirror of London Surgery*," that the object which had attracted this great congregation of doctors was to witness an excision of the knee-joint. We regret the occasion of such an assemblage, in the first place, on account of the patient, who, we believe, thereby lost a life which might probably have been saved; 2dly, on account of the practitioners, whose time, we think, might have been more profitably spent; and 3dly, on account of the science of surgery, which it seems to us has taken a step in the wrong direction."

We need make no comment upon a passage like this; but we find further on, that the critic, in order to prove the "inexpediency" of this operation, makes reference to two instances in which it was performed by Mr. Syme twenty years ago, one

of which patients died, whilst the other, although the operation was recovered from, "retained a limb which could not be considered superior to a wooden one, since it did not grow with the general growth of the frame, so that when the boy became a man there was a great disparity of length between the sound extremity and that which had been the subject of operation."

From these cases alone, then, the Edinburgh critic has come to a hasty and arbitrary conclusion respecting this operation. We must not suppose him, as he has ventured to give an opinion on this subject, to be ignorant of the cases in which other surgeons besides Mr. Syme have performed excision of the knee-joint.

The celebrated Park, of Liverpool, who is deservedly remembered for the introduction into British surgery of excision of carious joints, proposed and performed this operation on Hector McCaghen, aged 33, on the 2d of July, 1781. It completely succeeded; and the patient, who was a sailor, was actually enabled to follow his employment, as Park states; for, writing in 1789, eight years after the operation, he says:—"To the history of the case of Hector McCaghen there related I have now to add that he afterwards made several voyages to sea, in which he was able to go aloft with considerable agility, and to perform all the duties of a seaman; that he was twice shipwrecked, and suffered great hardships, without feeling any farther complaint in that limb; but was at last unfortunately drowned by the upsetting of a flat in the Mersey."

This case alone showed the expediency and benefit of the operation. By it the patient's life was saved, and the disease was extirpated, at the same time that a limb much more useful than any artificial substitute could possibly be was preserved, and must have been eminently serviceable to enable the man to follow such a laborious pursuit as that of a sailor.

Sir Phillip Crampton also performed excision of the knee-joint in two instances. Both recovered from the effects of the operation, although one of the patients died from general exhaustion three years and a half afterwards; the other case terminated most happily: so that Sir Phillip Crampton, after having examined the patient upwards of three years afterwards, was enabled to make the following report:—"She is now in town; and I have this day (November 3, 1826) examined the limb, and find that the femur and tibia are firmly consolidated; the leg and thigh are not in the slightest degree wasted; but the limb is considerably bowed outwards. She wears a shoe with a cork sole four inches thick; and, to use her own ex-pression,

is able to stand or walk the length of a day."

In other few instances also this operation has been performed, and with a certain amount of success. Therefore, it would have been more liberal and more fair had the critic in the Edinburgh Monthly Journal referred his readers to the cases of Park and Crampton, which turned out so successfully, and not have contented himself with measuring the expediency or the success of this operation by the two cases of Mr. Syme.

It is laudable on the part of a surgeon to attempt the removal of disease, and the salvation of a limb at the same time, by excising the affected joint; but an operation of this nature cannot be undertaken without placing the patient in a certain amount of danger, any more than when a whole member is amputated. It is true that, generally speaking, the excision of the elbow and shoulder-joints are more likely to be less fatal than removal of the corresponding limbs: at the same time, our own observation has shown us that they are not free from danger, and that possibly recovery might have taken place if amputation had been performed instead of the more protracted operation of excision: nevertheless, we should not refuse to undertake this latter proceeding in a fitting case. It is not to be denied that a large joint like the knee cannot be excised without considerable hazard to the life of the patient. It is impossible also to tell beforehand what will be the result of a simple amputation of the thigh: death may ensue after either. If, however, recovery takes place, the patient in the one case still has a useful limb, whilst in the other he only retains an artificial substitute, which would not certainly permit him to follow the laborious avocation of a sailor.

Notwithstanding, therefore, the denunciation of the Edinburgh critic, it is a question for serious consideration whether the surgeon is not acting wisely in certain instances in attempting to save a leg by excision of the knee-joint. If the advantages and disadvantages of the operation are placed before the patient, and he is willing to undergo it, the surgeon is certainly justified in doing so.

ROYAL COLLEGE OF SURGEONS.

GENTLEMEN admitted members on the 11th inst.:—Messrs. S. Henson—J. W. Baker—G. Harley—J. W. Walker—W. Browne—H. A. Aldred—T. S. Holland—W. Heron.

Medical Trials and Inquests.**CHELMSFORD COUNTY COURT.**

October 2, 1860.

*Before W. Gardon, Esq., M.A., Judge,***ACTION FOR RECOVERY OF CHARGES IN ATTENDING AS A PHYSICIAN.***Octavius Pritchard v. Maria Warmington.*

THIS action (which was tried by a jury) was for £25. 4s. for medical attendance and prescriptions for the defendant, at Hylands, the seat of J. Attwood, Esq., with whom she lives as housekeeper. The case was watched with much interest by the medical profession, a large number of whom from the town and neighbourhood were present.

Mr. DUFFIELD represented the plaintiff, and Mr. DURRANT the defendant, who had paid £12. of the sum claimed into Court.

It appeared from the evidence of Dr. Pritchard, who practises in Chelmsford as a consulting surgeon, under a Scotch diploma, that he had attended the defendant occasionally at her own request from March, 1847, to the month of May in the present year; but in April, 1847, a settlement took place, and the plaintiff's charge (five guineas, for about the same number of visits) was paid by the defendant without objection. By this payment, it was urged on the part of the plaintiff, that the defendant had admitted the reasonableness of the charge, and bound herself in a contract to pay after the same rate the present bill, which was for attendance extending over a period of three years. The total number of visits had been thirty-two, but certain deductions were made by the plaintiff, on account of his having on some occasions been sent for without expense of hire. The bill thus reduced was presented in May last, since which time the plaintiff, annoyed at the objections which had been made to it, had ceased to attend the defendant.

For the defence it was stated that the first account for £5. was for ten visits, and that therefore the plaintiff was only entitled to 10s. a journey on the present amount. His qualification as consulting surgeon was also disputed; and no less than six members of the profession, resident in Chelmsford, were called to prove their usual charges under similar circumstances. Some of them considered that 7s. for each visit (without the preparation of medicine) was an adequate remuneration; while another thought 10s. would not be too much; and the last gentleman called (Dr. Bird), who has been since attending

Miss Warmington, was of opinion, for a person of long standing and experience in the profession, a guinea for each journey would be only a fair remuneration.

His Honour carefully and at great length summed up the evidence, leaving it for the jury to give a verdict for the amount claimed, if they were of opinion that a guinea per journey had been paid on the first occasion; or of £16., if they relied on the evidence that only 10s. had been paid. The jury consulted for about a quarter of an hour, and then gave a verdict for £22.

The case, which occupied more than six hours, did not conclude till nearly seven o'clock. In the award of costs, two guineas were allowed to the plaintiff's solicitor.—*Essex Standard.*

Medical Intelligence.**MEDICAL COMMISSIONER OF THE IRISH CENSUS, 1851.**

HIS Excellency the Lord Lieutenant has been pleased to appoint William Donnelly, Esq., LL.D., the Registrar-General of Marriages in Ireland, to be the Chief Commissioner to superintend the enumeration of the population under the 18th and 14th Victoria, cap. 44. Surgeon Wilde, who was employed to compile the vital statistics in the Census of 1841, and whose able report has become a standard authority on this important branch of the social survey, has been appointed the Assistant Commissioner.

MEDICAL SOCIETY OF LONDON.

At a meeting of the Council of this Society, held October 3rd, G. J. Guthrie, Esq., F.R.S., and G. Owen Rees, Esq., M.D., F.R.S., were elected the Lettsomian Professors for the session 1850-51.

THE CHOLERA AT MALTA.

Oct. 2nd.—The last report of the cholera was favourable, and indicated a very sensible diminution, which has since continued, the attacks yesterday being but four, and no deaths to register.

Selections from Journals.**ON THE MEDICINAL EMPLOYMENT OF ARSENIC IN INTERMITTENT FEVER, AND IN GUTTA SERENA.**

M. MAILLOT, physician to the Military Hospital at Lille, has, in a recent number of the *Gazette Médicale*, recorded the results of his comparative observations on the use of arsenic and sulphate of quinine

in intermittent fever. M. Gibert, physician to the Hospital of St. Louis at Paris, has also communicated several papers to the *Bulletin Général de Thérapie*, on the use of arsenic in diseases of the skin. To these may be added Mr. Hunt's observations on the same subject. We present the following abstract of the above:—

M. Maillot states that from the 11th February to the 18th July of the present year, one hundred and sixty-six cases of ague had come under his care, furnished by one hundred and forty-eight individuals, sixteen of whom had experienced a second attack, and one a third.

The disease was generally simple; few cases presented any visceral complication or signs of miasmatic cachexia. Severe fever, with coma, occurred twice: the cases recovered.

With regard to type, M. Maillot gives the following table:—

Quotidian	48 cases
Tertian	114 "
Double tertian	1 "
Quartan	3 "

With reference to the duration of the disease, the cases were found to arrange themselves as follows:—First attack, 79; first relapse, 48; second relapse, 27; third relapse, 8; fourth relapse, 3; eighth relapse, 1; in all, 166. The relapses yielded generally to one or more doses of sulphate of quinine.

The cases were placed under treatment as follows:—

By expectation, or by an emetic	33
By arsenious acid alone	77
By sulphate of quinine alone	42
By sulphate of quinine and arsenious acid	14

In the majority of cases the treatment was not commenced until after the occurrence of at least one paroxysm in the hospital.

Of the thirty-three cases cured without any medicinal treatment, or by a simple emetic, the duration of the disease was in the following order:—First attack, 18; first relapse, 7; second relapse, 6; third relapse, 1; fourth relapse, 1. Six of these cases were cured by emetics, three had no recurrence of the fit, and the other three had each only one paroxysm subsequently.

The seventy-seven cases treated by arsenic alone presented the following order of duration:—First attack, 40; first relapse, 19; second relapse, 12; third relapse, 5; fourth relapse, 1. This table refutes the opinion that arsenic is useful only in long-standing cases.

The arsenious acid was administered in a solution of one centigramme (= 12½ Eng.

gr.) to thirty grammes (= 7·71 English drachms) of distilled water. In a few exceptional cases the dose was raised to three or four centigrammes. In general not more than two centigrammes (= 0·308 Eng. gr.) were given daily, taken in spoonful doses every hour—a mode of exhibition found to be safer than the administration of the entire dose at one time. The remedy was employed until no recurrence of the fit occurred, and was then continued in the dose of one centigramme daily until the patients were discharged. The average quantity thus taken was nineteen centigrammes (= 2·916 Eng. gr.) In some cases, in which, owing to relapses, it was necessary to increase the daily dose to four centigrammes (= 6·16 Eng. gr.), the whole quantity employed had risen to forty, forty-five, and fifty centigrammes (= 9·16—9·30—7·7—Eng. gr.) All the patients were placed on the most nourishing diet.

Mr. Hunt,* in a paper on the Medicinal Use of Arsenic in Cutaneous Diseases, points out, with regard to the time of administering this remedy, that it should be given with or after a meal, in order that it may enter the general system by the lacteals with the chyle during digestion; because, as remarked by Dr. Golding Bird, if taken on an empty stomach, the medicine will be chiefly absorbed by the venous capillaries, and enter the portal circulation.

Mr. Hunt also observes, with regard to the administration of arsenic, the very important point that it should not be given in increasing doses, as usually directed; because, being a cumulative medicine, its toxic effects are sometimes suddenly and alarmingly manifested. Mr. Hunt, on the contrary, decreases the dose as he observes its influence manifested on the tarsi. The advantage of this plan, Mr. Hunt remarks, is that there is no occasion to suspend its employment, owing to which the disease often reappears and becomes obstinate. Twenty or thirty drops of Fowler's solution is the average dose borne with impunity, according to Mr. Hunt; but he begins with five minims only, until he finds what each individual will bear without injury. "Where the susceptibility is not great," Mr. Hunt states that "a dose of two drachms of the solution can be borne as well in one dose as in twenty;" and that, where the susceptibility is great, the cumulative effects are shown after a few doses.

M. Maillot mentions the following phenomena as observed by him during the exhibition of this medicine.

The circulation did not manifest any change. In some patients the appetite was increased. The urine was not aug-

* Medical Times, Sept. 14, 1850.

mented in quantity. The arsenic could be detected in the urine, but in extremely minute quantities. It was not detected in the saliva or perspiration. The most marked effects were shown in the digestive canal. Under the use of three, or even of two centigrammes, thirst, nausea, bilious vomiting, and diarrhoea, have been observed. These effects have ceased on suspending or decreasing the dose: in some cases, without alteration in the dose, a tolerance of the remedy has been established. One case of gastro-enteritis of several days' duration occurred.

In seventy-seven cases the rate of cure, dating from the first dose, was as follows:—Thirty-eight had no return of the fever; thirty-one had only one paroxysm; seven experienced two; and one, several ague fits. Of the thirty-eight who were cured by the first dose, sixteen were suffering under their first attack. Out of the seventy-seven cases, fifteen relapses occurred.

M. Maillot accurately noted the results of the effects of sulphate of quinine in a comparative series of forty-two cases. Without entering into the details thereof, we may here give the conclusions at which M. Maillot has arrived.

Arsenious acid is a powerful febrifuge, but it is not so certain in its operation as sulphate of quinine; the latter medicine never having failed to effect a cure in his hands. The sulphate of quinine has in several cases been successfully employed where the fever has persisted despite the exhibition of arsenious acid.

The action of arsenic is not so prompt as is that of quinine. With the former the fever recurred in half the cases; with the latter in only one-fourth.

Arsenious acid seems to possess the advantage over quinine, that its use is less frequently followed by relapses, and these at more distant periods.

The injurious effects of arsenic have been unimportant, and have speedily subsided on a suspension or diminution of the dose.

There is no danger in treating intermittent fever with arsenious acid, which is the best substitute for quinine at present known. It is, perhaps, superior, if the quinine be given only in the usual small doses.

M. Gibert concludes, from his observations on the influence of arsenic on diseases of the skin, compared with that of other remedies, that preparations of arsenic do not possess a superior efficacy in these cases; that they are not more dangerous than other medicines (e. g. repeated purgatives), if given with due precautions. That, nevertheless, in many instances which have resisted other means, arsenic has often been given successfully, although M.

Gibert is of opinion that the powers of arsenical preparations have been much exaggerated, and that their use should be restricted to the more obstinate cases. x

ADVANTAGES AND DISADVANTAGES OF CHLOROFORM AS AN ANÆSTHETIC. BY DR. HAYWARD, U.S.

THE only advantages which chloroform possesses over sulphuric ether, Dr. Hayward states, "are that it is more agreeable to inhale than ether, and that a less quantity of it answers the purpose. On the other hand, it cannot be denied that fatal effects have followed its inhalation in several instances, even when administered by the most judicious hands; that in some cases convulsions have been produced, and in others a great disturbance of the brain, causing delirium. In some persons, this affection of the mind has continued for several weeks.

"There are other objections of a minor character. Chloroform is of an acid caustic nature, and if it comes in contact with the skin, unless it be protected by some oily substance, severe excoriation is the consequence. Its administration is generally followed by vomiting and headache, which continues for several hours, attended by a great degree of restlessness and want of sleep. Several cases have come under my care, in which the brain and nervous system have been affected to an alarming extent; though in every instance it was said that a small quantity only of chloroform was administered for the purpose of performing some operation on the teeth.

"An individual in the vicinity was thrown into violent convulsions, which continued for three or four days, during all which time she was in a state of complete insensibility, from the inhalation of the vapour of a few drops of chloroform administered by a careful and judicious physician. It would be easy to multiply examples of this kind; but it is not necessary, for there is a stronger ground on which we can rest our opposition to the use of chloroform, that is, its danger to life. This, it is well known, has already been in several instances destroyed by it. If it can be shown that it has caused the death of a single individual, when properly administered, we cannot fail to have our misgivings of the safety of its exhibition, though it may have been inhaled in almost numberless cases without any ill effect."—*American Journal of the Medical Sciences*, July 1860.

DIABETES MELLITUS IN A CHILD. BY DR. HAUNER, ANSTALT.

C. S. —, one year old. The child was born healthy, and had been brought up

by hand. Without any apparent cause, she began to look ill; to sleep badly; suffer from diarrhoea; and became emaciated. The mother applied to a medical man, who administered calomel and other purgatives, but gave no particular instructions as to the child's diet. The condition of the child continued to get worse, and when seen by Dr. Hauner she was regarded by him as suffering from advanced tabes mesenterica. The treatment at first was simply expectant, consisting of light nourishing diet, and a warm bath every night. The urine, though passed in considerable quantity, was not at this time regarded as being in greater excess than is often seen in children. It was noticed that the child began to crave for liquid food, and would eat incessantly for plain spring water, of which she drank large quantities. The urine dribbled away constantly; the child was restless; her countenance anxious; and her whole aspect sorrowful. The skin was dry, even after the bath. The lower extremities had a constant see-saw movement, which produced excoriation of the genitals. The body wasted away by frequent diarrhoea. The quantity of water drunk daily by this infant was between five and six quarts; sometimes as much as from eight to nine quarts. The urine was voided into a sponge, and its quantity, as nearly as could be ascertained, exceeded that of the water drunk. It was inodorous, pale, rather turbid, slightly coagulable, and sweet to the taste. Chemical analysis detected salts, albumen, and sugar, in abundance. When this was discovered, and the nature of the case was thereby made clear, the treatment was altered. The child was put upon a strictly animal diet of the strongest broths, with eggs. Morphia in small doses was also administered. For some time an apparent amendment was perceived; but in a few weeks the worst symptoms returned, and the child died. The body was greatly emaciated, and the internal organs were for the most part bloodless: the kidneys were the only organs that exhibited actual disease; they were double their ordinary size; their colour grayish white, with dark stripes and spots, and their substance hardened. Numerous small deposits of pus were also seen in their structure, and yellowish fluid could be expressed. The mucous membrane of the ureter and bladder was inflamed.—*Casper's Weekenschrift*.

BOOKS & PERIODICALS RECEIVED DURING THE WEEK.

The Historical Relations of Ancient Hindu with Greek Medicine. By Allan Webb, M.D.
A Manual of Qualitative Analysis. By Robert Galloway, F.C.S. &c.

On the Identity or Non-Identity of Typhoid and Typhus Fevers. By William Jenner, M.D. &c.

Report of the Proceedings of the Pathological Society of London. Fourth Session, 1849-50.

An Introductory Address, delivered at the London Hospital Medical School, by N. Ward, F.R.C.S.E. &c.

The Monthly Journal of Homoeopathy. October 1850.

BIRTHS & DEATHS IN THE METROPOLIS

During the Week ending Saturday, Oct. 12.

BIRTHS.		DEATHS.	
Males....	730	Males....	445
Females..	640	Females..	394
	1370		839

CAUSES OF DEATH.

ALL CAUSES	839
Specified Causes	830
1. Zymotic (or Epidemic, Endemic, Contagious) Diseases....	207
<i>Scarcely Diseases, viz.—</i>	
1. Dropsy, Cancer, &c.	54
2. Brain, Spinal Marrow, Nerves, and Senses	54
3. Heart and Bloodvessels	35
4. Lungs and organs of Respiration	35
5. Stomach, Liver, &c.	44
6. Diseases of the Kidneys, &c.	17
7. Childbirth, Diseases of Uterus, &c.	6
8. Rheumatism, Diseases of Bones, Joints, &c.	11
9. Skin	2
10. Premature Birth	26
11. Old Age	35
12. Sudden Deaths	5
13. Violence, Privation, Cold, &c.	16

The following is a selection of the numbers of Deaths from the most important special causes:

Small-pox	6	Convulsions	23
Measles	15	Bronchitis	20
Scarlatina	26	Pneumonia	40
Whooping-cough	17	Phthisis	105
Diarrhoea	40	Lungs	7
Cholera	1	Toothache	12
Typhus	55	Stomach	6
Dropsy	17	Liver	26
Hydrocephalus	20	Childbirth	2
Apoplexy	23	Uterus	3
Paralysis	21		

REMARKS.—The total number of deaths was 85 below the average mortality of the 41st week of ten previous years.

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer 29.73
" " " Thermometer 49.3
Self-registering do. Max. 60.0 Min. 22.0
" From 12 observations daily. " Sun.
RAIN, in inches, ".31.—Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was about the mean of the month.

NOTICES TO CORRESPONDENTS.

Seco.—We will make inquiry.
Mr. R. O. Clarke.—The paper shall be inserted.
We regret that we are again obliged to postpone Mr. Bate's contribution. It shall appear next week.
RECEIVED.—Dr. Snow. — A Subscriber. — A Constant Reader.

Lectures.

COURSE OF LECTURES

ON

DISEASES OF THE HEART.

Delivered at St. Vincent's Hospital during the Session 1849-50.

BY O'BRYEN BELLINGHAM, M.D.

Fellow of, and Member of the Court of Examiners of, the Royal College of Surgeons in Ireland, and one of the Medical Officers of the Hospital.

LECTURE XIII.

GENERAL SIGNS OF HEART DISEASE,
(continued).

The pulse as a means of diagnosis in cardiac disease—Morbid conditions of the heart which give rise to varieties of the pulse—Influence of the quality and quantity of the blood—Pulse in hypertrophy, and in dilatation of the left ventricle—Pulse in aortic regurgitation—Cause of the jerking pulse—Intermission, irregularity, and inequality of the pulse—Pulse in contraction of the mitral orifice, and in regurgitation through this orifice—Pulse in contraction of the aortic orifice—Pulse in fatty degeneration of the heart—Summary of the principal varieties of the pulse.

The Pulse as a Means of Diagnosis in Cardiac Disease.

PREVIOUS to, and even for some time subsequent to the discovery of auscultation, the pulse, as an aid to diagnosis in cardiac disease, was of very little value;—indeed, it is obvious that, until the order of succession of the movements of the heart was known, and the auscultatory signs which characterize the several diseased states of the valves were laid down with some precision, any information derived from the characters of the pulse must have been very vague. The obscurity which formerly invested this subject has, however, now been in a great measure dispelled; and the labours of successive investigators have shown that certain forms of cardiac disease are capable of impressing peculiar and well-marked characters upon the pulse.

The pulse is produced by the contraction of the left ventricle, which at each systole propels a certain amount of blood into the aorta; "the blood not being able to escape from the arteries as quickly as it is forced into them by the ventricle, on account of the resistance it experiences in the capillaries, necessarily exerts a pressure on the

elastic coats, and thus gives rise to the pulse." The pulse, therefore, corresponds with the ventricular systole, and with the first sound of the heart; and when the heart, the arterial system, and the blood, are each in the normal condition, the force, the strength, the frequency, and the fullness of the radial pulse, may be taken as a measure of the strength or feebleness of the systole of the left ventricle, of the rapidity with which the movements of the heart are performed, and of the amount of blood transmitted at each systole of the left ventricle.

At each systole of the left ventricle the arterial tubes become elongated: hence, in situations where these vessels have naturally a curve, this curve is increased; and where they are naturally straight, they become slightly curved; when the ventricular systole ceases, they return to their former position. In thin subjects this locomotive movement is perceptible in vessels near the surface, as in the temporal artery, and in the radial at the wrist; and this, which is in reality a natural phenomenon, is not unfrequently set down as a sign of disease. It undoubtedly becomes more marked in cases where regurgitation through the aortic valves or into the aorta itself occurs; but a visible and locomotive pulse in these arteries, unless accompanied by other signs of disease, has no value by itself, and is frequently observed in cases where the heart and arterial system are in a perfectly healthy condition.

At the moment that the pulse is felt, the arteries, in addition to becoming elongated, dilate, and increase in volume. "Every fluid exposed to pressure from behind," Weber* observes, "reacts on the surrounding parts, not merely in the line of direction of that pressure, but on every side; and, therefore, at each contraction of the ventricle, the blood the arteries contain reacts, in consequence of the impulse pressure of the injected blood, not merely longitudinally, but laterally, the arteries being fixed at both extremities." It has been denied by some physiologists that any dilatation of the arteries occurs: it does, however, take place; and M. Poiseuille,† by means of an apparatus capable of being applied round an artery, has determined the amount of this increase to be, for the larger arteries, about the one-eleventh of their diameter. The increased capacity of the arterial tubes depends more, however, upon their elongation than upon their dilatation.

The beat of the pulse in the radial artery is a little later than the ventricular

* Quoted by Dr. Graves in Dublin Journal.

† Magendie's Journal.

systole; but the interval is almost inappreciable, unless the pulse is unusually slow: in arteries further from the centre of the circulation this is somewhat more marked. Thus, according to Weber, the beat of the anterior tibial artery upon the dorsum of the foot is between the sixth and the seventh part of a second later than that of the axillary artery. According to Hamersyk,* the pulse at the wrist, at the temporal artery, and at the femoral, near Poupert's ligament, is perfectly asynchronous; and according to Weber, the pulse of the axillary artery, and of the external maxillary artery, is synchronous.

The radial pulse is said to follow the ventricular systole by a more appreciable interval when the aortic valves permit regurgitation: if it really does so, it is not a sign of any importance. "In debilitated and atonic states of the system the radial pulse," Dr. Williams† observes, "follows the first sound of the heart by a distinct interval, which is occupied by the transmission of the wave along the course of the comparatively lax and yielding artery." "Want of synchronism of the arterial pulse with the heart-beat," Dr. Douglass‡ observes, "has a doubtful connection with valvular disease, and does not occur except in conjunction with more or less dilatation."

In some individuals, to all appearance in perfect health, the pulse is preternaturally slow; in others it is preternaturally quick; and in others, again, it is constantly intermittent. These peculiarities are connected with a similar condition of the heart's action: they appear to be the result of idiosyncrasy, and are sometimes hereditary. The curious fact has been occasionally noticed, that in an individual whose pulse in health intermitted, it became regular on the invasion of illness of a febrile character. It is important to bear in mind that these peculiarities in the pulse are occasionally met with, lest they should be too hastily set down to disease of the heart, with which they may have no connection.

Disease of the right side of the heart exercises no direct influence upon the pulse, but valuable information may sometimes be obtained from its characters in diseased states of the left side of the organ. Although the radial pulse is, in the great majority of cases, an indication of the frequency of the contractions of the left ventricle, it does not necessarily indicate the force of the contraction. Thus, in some cases of disease, although the action of the heart is violent, and its impulse strong, the radial pulse is small and feeble; while, on the other hand, we know that in cases of

local inflammation, the artery going to an inflamed part may beat very strongly, although the heart's action is not at all increased. In all doubtful cases, therefore, it is advisable to place one hand upon the præcordial region, or to auscultate the region of the heart, while the finger is on the radial pulse. "We can thus," Dr. Williams observes, "take a far more accurate survey of the condition of the circulation, than by examining these parts separately; and the utility of this method is by no means confined to affections of the heart."

In several morbid conditions of the heart the pulse becomes quicker than natural: I am not aware of any diseased state of the organ in which it necessarily becomes slower, though this condition of the pulse has been regarded by some as a sign of softening of the muscular tissue of the organ; by others as a sign of a contracted state of the aortic orifice. The pulse, in some diseased states of the heart, becomes stronger, fuller, and harder than natural; in others, softer, weaker, smaller, or feebler than natural; and in others, again, it becomes jerking and receding. In all these cases the pulse may be regular; but it not infrequently happens that its rhythm is disturbed, when the pulse intermits, or becomes unequal or irregular; and these conditions of the pulse may be either combined together or with some of the other states mentioned above.

The morbid conditions under which these varieties of the pulse occur may consist in inflammation of the investing or lining membrane of the heart; in alterations of its muscular tissue; in changes in the capacity of its cavities; in diseased conditions of its valves or orifices; in alterations of the blood; in diseased states of the arteries; and in derangement or disease of other organs reacting upon the heart.

The quality of the blood contained in the vessels exerts a material influence upon the pulse; when this fluid is attenuated, when its red particles are diminished, its serous portion increased, and its viscosity lessened, whether this depends upon profuse hæmorrhage, or arises under other circumstances, the contractions of the heart become more frequent, and the radial pulse is consequently quickened; at the same time, as the strength of the ventricular systole is less than in health, the blood is impelled with less force, and the pulse is weak; while the healthy tension of the arteries being diminished, and their tubes not being filled, the pulse acquires a jerking character, resembling, in a slight degree, the pulse of aortic regurgitation.

The quantity of the blood contained in the vessels, no less than its quality, exercises some influence upon the pulse. "When there is a due correspondence be-

* Gazette Médicale, March, 1844.

† On Diseases of the Heart.

‡ Edinburgh Monthly Journal.

tween the capacity of the vascular system, and the quantity of blood circulating in this system, the coats of the arteries will (Dr. Copland observes*) be kept in that state of healthy tension, or tone, favourable to a regular, firm, free, natural, or healthy pulse, varying chiefly in frequency with the state of the heart's action." When the amount of blood in the system is greater than natural—in other words, in that state of the system denominated plethora—the pulse is full and hard, but not increased in frequency. The sensation produced by an artery in cases of excessive vascular fulness Dr. Copland designates by the term "oppression;" the vessel feeling as if "it were kept in a state of tension, or of distension, in the interval between the beats."

Pulse in hypertrophy of the left ventricle.

—When the parietes of the left ventricle are increased in thickness—in other words, in hypertrophy of the ventricle—its systole will be strong in proportion, the blood will be propelled into the aorta with increased force, and the radial pulse will be strong and hard: its velocity will not, however, be increased; but, as the systole of an hypertrophied ventricle takes a longer time to be completed, the pulse will "dwell longer under the finger." When some dilatation of the cavity of the ventricle is combined with hypertrophy of its walls, the pulse, as long as the circulation continues free, will have the same character, and, in addition, it will be full, because a larger amount of blood will then be impelled at each systole.

Pulse in dilatation of the left ventricle.

—When the cavity of the left ventricle is dilated, and its walls are attenuated, or even though they may preserve their normal thickness, the ventricular systole will be performed with less vigour, and the blood will be propelled with less force, than in a healthy state of the heart: hence the radial pulse will have nearly opposite characters to those of the former state; and, instead of being hard and strong, and dwelling long under the finger, it will be soft and weak, though it may be large.

Pulse in aortic regurgitation.—When the aortic orifice, or its valves, become altered by disease, or when, from any other cause, the valves imperfectly fulfil their functions, the blood will regurgitate into the left ventricle at each diastole, and the pulse in consequence will acquire a peculiar character, being jerking and receding, though regular, while the pulsation of the arteries of the upper extremities and neck is visible and locomotive. This, which is sometimes termed the pulse of unfilled arteries, is very peculiar, and, when once

felt, can scarcely be mistaken. In well-marked examples it appears as if the blood was divided into separate little masses, which pass in rapid succession under the finger. This sensation becomes more marked if we apply the finger to an artery of larger calibre than the radial, as the trachial, and if we lay two or more fingers upon the line of the artery. M. Simonnet has given the name *frôtement globulaire* to this variety of pulse, although he is mistaken respecting its cause. Dr. Hope compares the sensation to that of a hard ball shot with force under the finger, the artery feeling empty; and Dr. Watson,* to the blow of a hammer, without any prolonged swell of the artery, as if successive balls of blood were suddenly shot along under the finger.

Cause of the jerking pulse of aortic regurgitation.—The explanation of the cause of this peculiarity in the pulse is sufficiently simple. We have seen that the arteries,

at the moment that the ventricular systole occurs, dilate; when the diastole occurs, they return to their former state, owing to the elasticity of their coats. We have seen, also, that the arteries, in the healthy state, are always filled: there is a continuous column of blood in them; and the fresh portion, transmitted into the aorta at each contraction of the left ventricle, displaces a column of blood of equal size; an impulse is communicated, and the pulse is felt. Thus when an artery is wounded, or divided across, the blood escapes in a continuous stream, but is accelerated at each ventricular systole, the continuous stream being produced by the elasticity of the artery reacting upon the blood, in the intervals between the systole of the ventricle. Now when the aortic valves imperfectly fulfil their office, the moment that the ventricular systole ceases, a portion of the blood returns from the aorta into the left ventricle, there is a backward motion of the blood in the aorta, as the ventricle dilates, which may be favoured by the elasticity of the coats of the vessel. The arteries of the upper extremities and neck of course feel the effect most: they are less perfectly filled, the healthy state of tension of their coats is diminished, they react with less force upon their contents, and the blood has a forward motion, or, at least, is propelled with any force only during the period that the ventricular systole lasts: hence the column of blood appears to be interrupted, and the pulse has a jerking and receding character.

In cases of anæmia the same effect seems to be produced by the coats of the arteries

* Dict. of Pract. Medicine.

* Lectures on the Practice of Medicine.

losing in some measure their tone or elasticity, in consequence of the deficient nutriment which they receive: the vessels are consequently imperfectly filled, and the blood is moved forward apparently only at the period of the ventricular systole.

This peculiarity of the pulse is not, however, limited to the cases mentioned: it is observed also sometimes in aneurism of the ascending or transverse portion of the arch of the aorta, as well as in cases of disease of the aorta itself, when this vessel has become rigid and inelastic from adventitious deposit, which condition is often accompanied by dilatation of the artery. Here it is produced by the regurgitation of the blood from the large arteries, during the ventricular diastole, into the aorta itself, or into an aneurismal sac.

In addition, in patency of the aortic valves, the pulsation of the arteries, particularly of the superficial arteries of the head, face, and upper extremities, is remarkably visible and locomotive. This was first pointed out by Dr. Corrigan as a sign of patency of the aortic valves; and he observes that it is best seen when the arm is elevated above the head. This phenomenon is most remarkable in the temporal and radial arteries and their branches, in which "the arteries sometimes (as Dr. Williams observes) appear like worms under the skin, wriggling into tortuous lines at each pulse."

Although, in the great majority of cases, the radial pulse indicates exactly the frequency of the contractions of the ventricles, it occasionally happens that every contraction of the left ventricle is not propagated to the radial artery, owing to its systole being too feeble to propel the blood with sufficient force to give an impulse capable of being felt with the finger. If this recurs with regularity at every second beat, the pulse will appear to be preternaturally slow; and this, no doubt, was the condition in some of the cases of remarkably slow pulse reported previous to the discovery of auscultation. The error will be corrected by laying the stethoscope upon the precordial region, while the finger is upon the pulse. When this occurs at longer and irregular intervals, intermission of the pulse is said to occur; which we have next to consider.

Intermission and Irregularity of the Pulse.

The rhythm of the motions of the heart, and the irregularity of the contraction of the ventricles, are in general manifested by the pulse at the wrist: the heart's action cannot be intermittent or irregular without its being communicated to the radial artery: but the pulse may intermit, although the heart contracts regularly, if

every fifth, sixth, or seventh ventricular systole, as the case may be, is too feeble to propel a sufficient quantity of blood to communicate an impulse to the radial artery.

Intermission may be regarded as the slightest degree of derangement of the heart's action. It is not uncommon, as has already been observed, in individuals in whom the heart and arterial system are perfectly healthy: the intermissions may occur at regular or irregular intervals, and this state may continue through life. This form of derangement of the rhythm of the heart's motions is not uncommon in persons advanced in life, in gouty subjects, and in individuals labouring under derangement of the digestive organs, accompanied by flatulence. It is frequently likewise met with in cases of disease of the valves, or of the muscular tissue of the heart, when it is often accompanied by irregularity, or inequality of the pulse, and it becomes then a sign of considerable importance.

By an *unequal* pulse we understand one in which some pulsations are strong, and others weak. By an *irregular* pulse, one in which a few rapid beats are succeeded by one or more slower beats, and when the interval between them is different. Inequality and irregularity of the pulse are much more unfavourable signs than simple intermission, and are not observed except in cases of disease. Both are frequently met with in the same cases, and both accompany certain diseased states of the valves at the left side of the heart, of the muscular tissue of the organ, and of its investing membrane, the pericardium.

Pulse in contraction of the left auriculo-ventricular orifice.—When the mitral valve or the left auriculo-ventricular orifice become diseased, so as to obstruct the passage of the blood from the left auricle into the left ventricle, the radial pulse will necessarily be weaker than natural, and will occasionally intermit. When this condition of the valve or orifice is more advanced, and the obstruction to the passage of the blood through the orifice becomes greater, the pulse, in addition to being weak and intermittent, will become small, irregular, and unequal, although the heart's action is strong. Mr. Adams,* of this city, was the first to call particular attention to the want of correspondence between the strength of the pulse at the wrist and the impulse of the heart in this form of disease: "the heart often beats so violently (he observes) as to shake the patient in his bed, while the pulse is small, weak, and irregular." "I know not how," he adds, "to describe

* Dublin Hospital Reports.

it otherwise than by saying that it appears as if there were two pulses; one slow and deliberate for two or three beats, succeeded by three or four rapid and indistinct pulsations."

Pulse in mitral regurgitation.—When the mitral valve imperfectly closes the left auriculo-ventricular orifice, and regurgitation into the auricle occurs at each ventricular systole, the pulse will be scarcely affected if the aperture is very trifling; when larger, the pulse will become weak and small, and will intermit, particularly when the circulation is hurried. When the mitral orifice is much enlarged, and permits a considerable portion of the blood to regurgitate into the auricle at each ventricular systole, the pulse, in addition to being small and weak, will become irregular and unequal, and will almost exactly resemble the pulse of considerable contraction of the mitral orifice.

Hence, in the advanced stage of these two opposite conditions of the mitral valve and orifice, the pulse presents similar characters: thus, when this orifice is considerably diminished in size, a smaller quantity of blood can enter the ventricle from the auricle at each diastole; the ventricle, therefore, is imperfectly filled, and less blood than natural is transmitted into the aorta at each systole; the pulse necessarily becomes small and feeble, and eventually intermittent, irregular, and unequal. Again, when the mitral orifice is preternaturally dilated, a considerable portion of the blood is transmitted backwards into the auricle at each systole of the ventricle, and the same result follows; a smaller quantity than natural is transmitted into the aorta at each systole, and the pulse likewise becomes weak, small, irregular, and unequal in proportion.

Pulse in contraction of the aortic orifice.—When the aortic orifice is slightly contracted the stream of blood transmitted into the aorta at each systole of the left ventricle must be somewhat less than natural; but as the walls of the ventricle under such circumstances almost always become hypertrophied, what is wanting in quantity is made up by the strength of the ventricular systole, and the pulse will be neither weaker nor smaller than natural, and will be perfectly regular. When the contraction, however, becomes extreme, as sometimes happens, though much less frequently than at the mitral orifice, the pulse will then become small and intermittent, irregular and unequal, resembling the pulse of considerable contraction of the mitral orifice, or of free regurgitation through it.

Pulse in softening of the heart.—A small, weak, irregular, and unequal pulse is not, however, peculiar to cases of valvular dis-

ease: the radial pulse has the same characters, and in a still more marked degree, in the advanced stage of softening of the heart from fatty degeneration or other cause, particularly when combined with dilatation of the cavity of the left ventricle. The contractile power of the ventricle being necessarily weakened when the muscular tissue is softened, the force with which the blood is propelled into the aorta will be diminished; while the weakened ventricle, being unable to empty itself at each systole, the quantity of blood which it propels into the aorta is less than natural: hence not only is a smaller quantity of blood transmitted into the aorta, but it is transmitted with diminished force, and the pulse under such circumstances becomes sometimes so small, weak, irregular, and unequal, that it is impossible to count it.

A small, weak, and irregular pulse is also observed in cases of pericarditis accompanied by copious liquid effusion, by which the heart's movements are materially impeded; or where fibrinous concretions form in the cavities of the heart previous to death, by which the passage of the blood through them is obstructed; or in any case where much embarrassment of the cardiac or pulmonary circulation exists, as towards the close of several diseases of the heart or lungs.

Summary of the Principal Varieties of the Pulse.

In the following summary of the more prominent characters of the pulse in the several diseased states we have been considering, the varieties of the pulse are arranged according to their regularity or irregularity. The first division includes the cases in which the pulse is usually regular; the second, those in which the pulse is usually irregular.

Pulse Regular.

1. In plethora the pulse is full, hard, and resistant, but not more frequent than natural.
2. In anæmia the pulse is smaller, weaker, and quicker than natural, and in addition has a jerking character.
3. In hypertrophy of the left ventricle the pulse is "strong and prolonged," but not quicker than natural. When dilatation is combined with the hypertrophy the pulse is in addition full.
4. In dilatation of the left ventricle without hypertrophy the pulse is large, but weak.
5. In dilatation with attenuation of the left ventricle the pulse is weak and soft, easily quickened, and occasionally intermittent.
6. In patency of the aortic valves the

pulse is jerking, visible, and locomotive, but its frequency is not increased.

7. In diseased states of the coats of the arch of the aorta, with increase of the calibre of the vessel, the pulse has the same characters, but in a less marked degree.

8. In aneurism of the arch of the aorta the pulse may have a different strength in each wrist, or it may be absent altogether in the radial artery on one side. It may also have a jerking character, resembling that of aortic regurgitation.

Pulse Irregular.

1. In extreme contraction of the mitral orifice the pulse is small, weak, intermittent, irregular, and unequal.

2. In cases of regurgitation through the mitral orifice, when slight, the pulse is little altered; when extreme, the pulse has the same characters as in considerable contraction of the orifice.

3. In contraction of the aortic orifice the pulse presents no peculiarity unless the degree of contraction is extreme, when it becomes small, irregular, and intermittent.

4. In fatty degeneration of the muscular tissue of the left ventricle, the pulse, in the advanced stages, is small, weak, irregular, and unequal.

5. In pericarditis with copious liquid effusion the pulse presents somewhat the same characters.

6. In cases where fibrinous concretions form in the cavities of the heart the pulse suddenly becomes small, weak, intermittent, and irregular.

From what has been said, it may be gathered, that in several forms of cardiac disease the characters presented by the pulse are of a nature materially to aid the diagnosis: and if the different cardiac lesions were met with always distinct from each other, and uncomplicated with other affections, the signs derived from the pulse would be most valuable. Unfortunately, however, this is not often the case. Two or more forms of structural disease are frequently combined; for instance, hypertrophy and dilatation of the left ventricle are very frequently associated; and, as these morbid conditions are a frequent result of disease of the valves or orifices, we may have obstructive or regurgitant disease of the aortic or mitral valves, or of both, combined with hypertrophy, or dilatation of the left ventricle, or with the two latter states. In addition, disease of any part of the heart may be associated with a state of anæmia or hysteria, with functional derangement, or a nervous habit, or with some of the other morbid conditions that have been alluded to; when the pulse, if

trusted to alone, would be more likely to lead into error than to assist the diagnosis.

It must be borne in mind, too, that intermission and irregularity of the pulse may ensue in any diseased state of the heart, when the pulmonary circulation becomes greatly obstructed. It should also be recollected that a very trifling morbid alteration of the valves at the left side of the heart, which may be accompanied by a loud *bruit de soufflet*, often causes no alteration of the pulse; while in a very advanced stage of the same disease, when the pulse becomes intermittent and irregular, an abnormal sound may be absent. Finally, as the systole of the right ventricle exercises no direct influence upon the pulse, the walls of the right side of the heart may be hypertrophied, or their cavities dilated; or its valves or orifices may be diseased, without the pulse indicating in the slightest degree morbid alteration.

Original Communications.

BRIEF REMARKS UPON

THE CHOLERA AS IT APPEARED AT CARDIFFE AND DOWLAIS IN THE SUMMER OF 1849.

By G. E. WEBSTER, Esq.

(Read before the *Abernethian Society*,
December 6th, 1849).

HAVING been resident for some considerable time last summer at Cardiff and Dowlais during the visitation of cholera, it is my intention to record a few of the various incidents which struck me as worthy of remark, and to notice any causes which may seem to have accelerated, retarded, or obstructed the progress of this malady in those places.

Epidemics of all kinds are very frequent in the town of Cardiff. It is an over-populated, densely-crowded seaport, having considerably increased in commerce and population within the last few years, without due attention being paid, either by public enterprise or private speculation, to the domestic comforts, necessities, and conveniences of the humbler classes. The site of the town, from lying almost on a dead level, afforded no facilities for drainage; consequently overflowing cesspools and stopped-up drains were of too frequent occurrence. In addition to these evils the Glamorganshire canal, which runs

through the centre of the town, acting as a convenient receptacle for all sorts of filth, was entirely drained of its water for some distance, owing to repairs being requisite, leaving the refuse matter exposed to the scorching rays of the sun.

In this locality, and the small streets composed of lodging-houses filled with Irish to an extent almost incredible, the disease committed the greatest ravages.

The interference of sanitary measures was not less needed at Dowlais than at Cardiff, though, the former having the advantage of being situated on the side of a hill, the natural drainage was good.

The people, generally speaking, were in a better condition of life, their wages good, and work constant; their habitations, however, and conveniences equally bad. In conjunction with these the severe and exhausting nature of their employment, and the consequent incitement to drink, must be considered among the predisposing causes of disease.

The experienced eye might frequently detect in the patient's appearance and expression of countenance indications of the forthcoming evil. My attention was drawn to this point by Dr. Sutherland, a commissioner of the Board of Health, in order to induce persons to leave those localities where cholera had made its appearance, and my own subsequent observation fully bore out the truth of this assertion.

Purposely passing over a minute detail of the symptoms of cholera, I may remark that the cramps and vomiting usually commenced and recurred together; but what relation they bore to each other in the shape of cause and effect I cannot tell. It was remarkable, in almost all those cases which recovered from the stage of collapse, how well the mechanism of life was depicted, and what functions are most necessary for supporting and prolonging human existence, an alteration taking place in those functions first, and secondly, in those which cannot be considered essential to life. Thus nature, gradually reviving, begins to develop a degree of animal heat; the pulse, returning at intervals, ultimately becomes quick and hard, the respiration is somewhat easier, the features and hands become less blue, the vomiting and purging gradually cease; the excretions being changed in appearance often assume a greenish or dark cast, and,

finally, the secretion of urine, before so totally suppressed, is restored.

I found there was no difference in the time of day in which persons were attacked, as they were taken ill at all hours throughout the twenty-four, but generally after returning from work. This may be attributed to the fact that their employment lasts incessantly throughout the day and night, different relays of men going on every eight hours. At Cardiff, on the other hand, the hours of seizure were most frequent between sunset and sunrise.

The days of the week also had their respective increase in mortality and number of attacks: I found that the early part of the week almost invariably had the heaviest mortality. This was attributed in great measure to the wages being paid on the Saturday, and the idle day Sunday intervening, a greater part of them was expended in the beer-shop during that period. By way of experiment, through the instrumentality of the manager of the works, all the beer-shops were closed from six o'clock in the evening, and to remain so during Sunday. The rate of mortality certainly was diminished on the two subsequent days, but increased to the usual amount on the Wednesday and Thursday.

These measures, with some others which were adopted, for restraining persons from the abuse of stimulants, were, I think, calculated to do, and I have no doubt did, some harm; for whilst, on the one hand, as it was seen, they did not restrain the habitual drunkard, on the other they frightened numbers of individuals into a different mode of living, so that they abjured *in toto* their accustomed amount of stimulus, which from long habit had become almost necessary to their existence. Nor can I advert too strongly to the folly of persons, even in the higher classes of society, totally abstaining from articles of diet to which they have long been accustomed, simply because statements have gone forth from individuals no wiser than themselves that such articles have a choleraic tendency. All sudden changes, I believe, have a very injurious tendency, and may be fairly ranked among the predisposing causes of cholera.

The migratory nature of, and the mode in which this malady travelled was very remarkable—that it should

pursue the same course, visit the same localities, break out in exactly the same houses in these localities, rage with violence in the circumference of an isolated spot, leaving that spot scatheless, free, and untouched, overlook towns, and not only towns, but a whole county, leave one part of a town and break out in the opposite direction, and then becoming mitigated, subsequently return to the scene of its former visit.

Instances of all these have come under my notice. It was foretold, long before its appearance at Tooting, from its course in foreign countries, that England would again be visited, and the result verified the prediction. Exactly the same locality and house in Dowlais was first visited as in 1832. The town of Tiverton, in Devonshire, escaped entirely, as it formerly did, being surrounded by numerous towns on the south-west coast in which it was raging. No record appears of a single case in the whole county of Hereford; and, finally, having committed great ravages on the south-west portion of Dowlais, it gradually travelled up to the north-east point, where the mortality became very great; but in a single night the wind veered round to that quarter, the disease became mitigated, and broke out again in its former locality.

Upon the first outbreak of the epidemic at Cardiff the usual premonitory symptoms were very much neglected, medical assistance frequently not being obtained until the more decided characteristic symptoms had set in, and sometimes even the stage of collapse come on.

Persons, however, soon became so panic-stricken, that every headache and pain was construed into an approaching attack of cholera; and I have no doubt that fear alone in some instances was the sole cause of an attack, of which the following is a powerfully striking instance, and where collapse came on suddenly; it occurred to a woman whose husband was labouring under consecutive fever:—The medical man having left the house but a very few minutes, and seeing some more patients in the same street, was suddenly sent for back again to the identical house, on account of the woman being struck. She was in the stage of collapse, cold, and pulseless, her vomitings and evacuations being very characteristic. It was ascertained upon inquiry that a neighbour had dropped in to ask after the

health of the man; the wife told her that he had hiccough, to which she officiously replied, "that it was all over with him if it came to hiccough." From that moment the wife was seized, and died in four hours, no treatment appearing to rally her in the least.

Upon the advice of Dr. Sutherland the system of house visitation was adopted and rigorously carried out. Certain districts were allotted to lay individuals, whose business it was to visit every house in the district twice in the day, and inquire concerning the health of every occupant. Any symptoms of illness being manifest, I was immediately sent for to the patient, and by me subsequently transferred to the respective medical officer of that district. By this means persons were seen from the very first, their fears and apprehensions frequently dispelled, and a most searching investigation into the sanitary condition of their domiciles was made.

The general routine of treatment adopted was, in the first instance, calomel and opium every hour, with brandy, counter-irritants, the application of warmth, and the administration of salines. The opium and brandy were, however, soon abandoned, for this reason—it was found that those who had passed through the acute stage generally had a severe attack of consecutive fever, and most of these died comatose; and it was often difficult to tell how much of the evil was to be attributed to the opium, and how much to the decreased action of the kidneys. Again, opium, when administered in small and repeated doses, takes on the nature of a stimulant; and it was found that stimulants of all kinds did harm; very often they had little or no effect upon the system, and when any effect was produced, the subsequent amount of depression was greater than that prior to the exhibition of the stimulant.

Counter-irritants I found of service in three ways, but not until the disease had become somewhat mitigated, and there was hope of ultimate recovery. Frequently, when the collapsed stage had passed off, and the evacuations were diminished in quantity and altered in quality, vomiting would still continue, and resist all endeavours to allay it. The stomach would seem to have contracted a morbid habit of expelling its contents the wrong way: in these cases the application of a smart

blister over the region of the stomach would produce the desired effect. Again, not unfrequently an intense burning pain would be complained of at the pit of the stomach, even if vomiting had ceased; in these instances also they were useful. And thirdly, they were advantageous in consecutive fever when there was a tendency to coma.

The routine of treatment adopted at Dowlais was very similar, still adhering to calomel, which was administered pretty freely in doses from one to five grains every quarter of an hour without opium, the simple effervescing saline being given in conjunction with it. In some instances this treatment acted as a charm in checking the profluvial discharges, whilst in numerous others it had not the slightest effect. Carbon, in the form of burnt cork, was occasionally substituted for the saline, and produced good as well as bad results; but I think, on the whole, it was more beneficial to children and young persons than grown-up people. In not a few instances even of active cholera, but especially of insidious diarrhoea, the following prescription of Mr. Girdlestone's—viz., Dover's powder, carbonate of soda, and sulphate of copper, in very small doses,—produced a marked effect when other astringents had failed.

The indiscriminate administration of chalk mixtures I think very frequently did harm in the premonitory diarrhoea; for if any good was to be the result, it generally became manifest after the first two doses; but if not, and still persisted in, it only irritated an already too highly irritable mucous membrane. Induced partly by the modest manner in which two proposed remedies were promulgated, and partly because the result could not be much worse than with other means, I put them to the test of practical experience;—these were olive oil and colchicum. I administered an ounce of olive oil to six patients every quarter of an hour with three grains of calomel for four doses, and afterwards ordered them to drink copiously of lukewarm water until either the evacuations lessened or became altered in appearance. The result was that five of them died; the sixth was put upon the saline treatment, and ultimately recovered. With the colchicum I had no better success, for in four cases they all died. During an acute attack of cholera, as to what the patient might eat or drink, my

directions were very concise,—“Let him eat what he likes, and drink what he likes,”—for I felt perfectly sure that solids were quite out of the question, the general demand being for cold water; and I do not believe that the imbibition of fluids in any way aggravates the purging, as stated by some persons, but, on the contrary, proves an useful auxiliary to that incessant dry retching, than which nothing is more distressing, fatiguing, and exhausting to the patient.

Finally, I think that altogether, during an acute attack, too much reliance is placed upon medicine, and too little attention paid to sustaining the system, which has to undergo such a severe ordeal: and I would strongly advocate the administration of nutritious substances from the very first, either by the mouth or by injection, at least three or four times a day; for though it be a mooted question whether absorption goes on or not, harm cannot possibly be done; and it may be a means of diminishing our awful lists of mortality, should a fresh outbreak occur this spring, or return again at the expiration of another sexdecennial period.

With regard to the cause of cholera, that is, I may say, entirely obscure, but perhaps not more so than of other epidemics. We cannot tell why at one time, small-pox; at a second, continued fever; at a third, influenza; and at a fourth, cholera should prevail: but we can tell in what regions and localities these epidemics, when prevailing, commit the greatest havoc. The epidemical nature of cholera is a most important topic for consideration and discussion in two aspects,—the first, as to the nature of epidemic force as a cause in *inducing disease*; in other words, *epidemic force per se*; and secondly, with reference to those causes and circumstances which assist in adding to its power, and giving it a greater tendency and facility to spread.

Supposing (to use an arbitrary explanation in numbers) that the sum of cause or causes required to produce the given effect is five, then, if the *epidemic force* were equal to five, it would attack any one with whom it came in contact; but supposing it were equal to three only, it would not affect a healthy individual: but if the additional two were made out by intemperance, fear, anxiety, bad air, or other cause or causes, the effect would be the same as if the neces-

gery number were made up of epidemic force alone.

The epidemic character of certain diseases is not only a remarkable, but a most important, circumstance. Their influences appear to visit, and impart certain properties to unhealthy places in which diseases are ordinarily found, or even healthy places to which no diseases are peculiarly attached, and also act upon causes which usually produce certain illnesses, so that these illnesses shall be changed or converted, and invested with the type, nature, and power of the prevailing epidemic. Thus, during the whole time I was at Dowlais, I do not remember attending a single case of any other illness but cholera, except accidents, which, from the vast number of men, and the nature of their employment, were extremely common; nevertheless, even *these* were considerably below the average, and the ratio per week very much diminished. Cholera will sometimes suddenly appear in apparently healthy places where no disease before existed, and where none could be well expected to appear; but it will more certainly invade unhealthy localities, or impure dwellings, ill ventilated houses, the neighbourhood of foul drains, open cesspools, &c., formerly the abodes of typhus and other fevers, and ravage such places to the exclusion of fevers before prevalent. It will, again, select for its victims the weakly, the fear-stricken, and the intemperate; but even the healthy and temperate will not be invariably exempt from its attacks. Its abode is uncertain, and its stoops or pounces are often sudden and capricious: its type and fatal character also are very varying, inconstant, and ever changing. We frequently find cholera in a place affected to be one week deadly in its effects, and utterly bidding defiance to remedies; in the next, it may be more benign, and readily controllable by curative agents.

Many other diseases have similar attributes; and against these, in the same manner as against cholera, full health furnishes no certain security; yet the prone or predisposed are for the most part the victims. Healthful localities, again, do not possess entire immunity; for they are occasionally the seats of the ravages of disease; yet places of a contrary nature are the chief scenes of its presence and its power, and supply the greater number of fatal cases.

The abodes of plenty are not safe, but the dwellings of want, woe, and wretchedness, are far less so.

OBSERVATIONS

ON THE

NATURE AND CHEMICAL CHARACTERS OF THE "DUMB-BELL" CRYSTALS DESCRIBED AS OXALATE OF LIME.

By DR. GOLDING BIRD, F.R.S.

MORE than eight years have elapsed since, among other forms of crystalline deposits in the urine described by me, I especially drew attention to the frequent occurrence of oxalate of lime in octahedral crystals. These observations (although they appear, from the tone in which they were alluded to by other writers at the time, to have been received with some hesitation) have since been amply confirmed by almost every one who has taken the trouble to examine the urine in health and disease with any amount of care. Perhaps too much importance can hardly be attached to the examination of the urine in many forms of disease with a view to the detection of the oxalate of lime, on account of the very serious phases of disease it often indicates, and of the important therapeutical hints it affords.

It is, however, with relation to one peculiar form of crystalline deposit, which I described as a variety of oxalate of lime, that I am anxious very briefly to direct the attention of your readers: I allude to the very remarkable crystals I observed about nine years ago, and first figured in a paper published in Guy's Hospital Reports (vol. vii. for 1842): these crystals, which, from their remarkable shape, I called *dumb-bells*, obey under the microscope the action of the same solvents as the octahedral crystals of oxalate of lime, and as, moreover, they always alternated with these, and were often found with them, I regarded them as *zeolitic* crystals of oxalate, under the idea that they were made up of a number of acute octahedra arranged in a radiating manner. I confess, however, that I was never quite satisfied with this view of their nature in consequence of their optical characters differing so completely from

those of the octahedral oxalate. The latter salt, in obedience to the well-known law of cubic and octahedral crystals, not possessing the power of doubly refracting a ray of light, did not affect plane polarised light. But the dumb-bells, on the contrary, possess an energetic doubly-refracting power, and powerfully depolarise the light transmitted through the Nicol's prism of the polarising microscope. The rays thus transmitted, when analysed by a plate of tourmaline, or a single image prism, exhibit a very beautiful image, the dumb-bells presenting a series of concentric coloured rings, traversed by the well-known black cross. These facts have induced me to take every opportunity in my power of carefully examining the crystals in question; and very lately, having a patient under my care whose urine deposited a large quantity of dumb-bells unmixed with any other crystal, I was able to submit them to examination with the following results:—

A. Observed during a fortnight these dumb-bell crystals never occurred for two days in succession, either disappearing completely, or existing in mere traces, always alternating with octahedral crystals, and ultimately quite disappearing.

B. Kept under water for ten days in a greenhouse, in July, the temperature seldom being below 85 or 90° in the day-time, they underwent some change; the water became turbid, and evolved a foetid ammoniacal odour. Under the microscope some of the dumb-bells had disappeared, and large octahedral crystals of oxalate of lime, which did not exist previously, were now visible.

C. The dumb-bells dissolved slowly in boiling dilute hydrochloric acid; and from this solution ammonia threw down an amorphous white precipitate. The whole was left to evaporate spontaneously in a watch-glass; large and beautiful cubes of hydrochlorate of ammonia, unmixed with any of the usual plumose crystals of that salt, were left.

D. Ignited in a platinum spoon, these dumb-bells became black, evolved an odour of burnt horn, and at a moderate red heat became quite white. Examined under the microscope, the form of the crystals was in no way affected. The dumb-bells had simply lost their transparency, and had become quite opaque. It was, however, evident that

they had undergone a most important chemical change; for they now restored the colour of moistened red litmus paper, and dissolved in acetic acid (in which they were, before ignition, quite insoluble) with active effervescence. In water they were quite insoluble.

E. The ignited dumb-bells (D) dissolved with active effervescence in dilute sulphuric acid, and the solution left by evaporation crystals of sulphate of lime.

F. Some of the dumb-bells recently collected from the urine and washed, were boiled in strong nitric acid; they dissolved without any evolution of gas, and formed a clear white solution. A drop of this, spontaneously evaporated on a plate of glass, left a beautiful mass of dumb-bells, rather larger and more perfect in outline than they were before solution. Hence it is evident that boiling nitric acid simply dissolves, but does not change, the crystals.

F. Boiled in hydrochloric acid, the fresh dumb-bells dissolved, and, by spontaneous evaporation, left beautiful tufts of long radiating acicular crystals, insoluble in water, but soluble in dilute hydrochloric acid.

These experiments are sufficient to invest the dumb-bell crystals with great chemical interest; and I can only regret the few opportunities we possess of collecting them in sufficient quantities for more minute investigation. We may, however, safely draw some conclusions from the above investigation, superficial as it is.

That these crystals are a salt of lime, capable of being converted into a carbonate by ignition, is proved by experiments D and E. That the acid combined with the lime cannot be the uric or sulphuric, is obvious from the remarkable circumstance of their dissolving in boiling nitric acid without change (exp. F). The peculiar change presented by these crystals by keeping in a warm place under water (exp. B) indicates their undergoing a putrefactive change with the evolution of ammonia; hence it may be inferred that the acid combined with the lime contains nitrogen. The cubic form of the crystals of hydrochlorate of ammonia in exp. C is that assumed by this salt in the presence of urea.

Every one at all familiar with the microscope is well acquainted with the

crystals of oxalurate of ammonia and their action on polarised light: this salt presents so close a resemblance in its radiated structure, in its doubly-refracting power, in the arrangement of its rings and cross in polarised light, to the dumb-bell found in urine, that I think we have much presumptive evidence (for I dare not say more) in favour of their being an oxalurate of lime instead of an oxalate, as I had declared it to be. It is true that the properties of the crystals above described are not to be regarded as conclusive; but the evolution of ammonia during the putrefactive fermentation (exp. B), the crystalline form of the hydrochlorate of ammonia (exp. C), indicates the probable presence of urea. Now, although oxaluric acid does not contain urea ready formed, yet it contains its elements, and oxalate of lime differs from oxalurate of lime only in their absence, and in containing the constituents of water; for

	C	N	H	O
2 atom oxalic acid . . .	4			6
1 „ urea	2	2	4	2
	6	2	4	8
-1 „ water			1	1
= „ oxaluric acid	6	2	3	7

Nothing is more likely than the combination in the blood of the *nascent* elements of oxalic acid and urea. And in connection with this, it must not be forgotten, that dumb-bell crystals, exactly like those I observed in urine, have been detected by Dr. Garrod in the blood. Admitting this, we at once see the simple relation of the dumb bells to the octohedra; of oxalurate, to oxalate of lime,—their alternation, mixture, and mutual replacement in urinous deposits all become clearer.

In venturing to regard the dumb-bell crystals as those of oxalurate of lime, I cannot help here alluding to the opinion expressed to me by Baron Liebig some years ago, that he had no doubt the oxalurate of lime would be one day detected as a constituent of urinary deposits.

Dr. Hassall,* so favourably known

* May I be permitted to express an anxious hope that the work projected by this gentleman on Human Crystallography will, notwithstanding the delay of its publication, be placed before the profession. I have been favoured with an opportunity of inspecting very many of Dr. Hassall's drawings for this work, especially of those

as a microscopical observer, has informed me that he has met with crystals exactly like the dumb-bells I have described, in the residue left by the spontaneous evaporation of a drop of urine on a plate of glass, but differing from them in their being soluble in water. I am not aware whether he has made any researches into their chemical properties.

I must beg to apologise for the length of this paper, which I should not have ventured to have inflicted upon the readers of your valuable journal, as I fully intended deferring any allusion to these observations until the publication of the third edition of my book (*Urinary Deposits*), now in the press, had not my attention been drawn to a paper by Dr. Frick, of Baltimore. In this communication—which, from its republication in that excellent and admirably conducted periodical, the *Edinburgh Medical and Surgical Journal* for this month, will obtain a large circulation,—the author has declared the dumb-bell crystals to consist of uric acid. Such an opinion requires no serious refutation, as crystals which can be ignited without losing their form, but are converted into carbonate of lime, and which can dissolve in boiling nitric acid without change, cannot, even by those most ignorant of chemistry, be regarded as consisting of uric acid. It is difficult to understand how so careful an observer as Dr. Frick could have committed such an error. I should not have thus alluded to this statement, had not it involved a most important indication of treatment, for it is obvious that if the crystals in question consisted of uric acid, their therapeutical indications would be very different from that which I have assumed them to be.

48, Russell Square, Oct. 16th, 1850.

ROYAL COLLEGE OF SURGEONS.

GENTLEMEN admitted members on the 18th inst. :—Messrs. C. Brettingham—T. B. Washbourn—R. G. Horton—M. W. Flewitt—C. Gordon—T. Lowe—C. Booth—T. A. Haigh.

made from preparations I had great pleasure in placing at his disposal. Their accuracy and beauty are unequalled; and a work of this kind will prove of the greatest value to those who employ histological observations for the elucidation of disease.

NOTES
ON THE STRUCTURE, &c., OF THE
TEETH.

BY C. SPENCE BATE, ESQ.,

[Continued from p. 333.]

Enamel.

1. In mammalia, both in relation to the development of the whole structure of the tooth, as well also in its respective importance in fulfilment of certain required conditions, the enamel succeeds the dentine. For scarcely has a scale of the latter been deposited upon the apex of the pulp from which it is developed, than lines of enamel are deposited upon its external surface.

2. The tissues of the pulp from which the enamel is developed are of a description similar to those which exist in the dentinal pulp; but they are present under peculiar modifications, not only in comparison with the pulp which develops the dentine, but also as regards the character of enamel in different classes of animals.

In *man* and *carnivorous animals*, in all of which the enamel covers the crown of the tooth, the enamel-pulp consists of nucleated cells larger in size generally than those which exist in the dentinal pulp of the same tooth; also areolar tissue, which has the peculiarity of nucleated centres exhibited, which gives to the structure a reticulated appearance, a circumstance which renders to this tissue a character peculiarly its own (Fig. 9), the whole is em-

FIG. 9.



Areolar tissue from enamel pulp of kitten at birth.

bedded in an unctuous fluid plasma, and yellowish transparent, confined by an external membrane. The organ in this character of tooth is wholly ex-

travascular, at least during the period of the development of the enamel, and is supplied with calcareous material from bloodvessels which ramify upon the capsule which invests the whole tooth, which I presume passes through the outer wall of the enamel pulp by endosmosis.

Among the *rudimentary* tribe of mammals the enamel is developed from a pulp which dips into clefts or fissures existing between processes formed by the dentinal pulp, and in this class of tooth, to unassisted vision, the pulp from which the enamel is developed cannot be distinguished from that which develops the dentine; but when the test of the microscope is applied, certain conditions may be observed in the enamel pulp which cannot be seen in the organ whose attribute is to develop the tooth substance; these conditions are, that the ultimate secreting cells of the enamel pulp are not interspersed with bloodvessels; that whereas in the dentinal pulp bloodvessels extend into the newly deposited material, in the enamel a layer of cells, the true extra-vascular enamel pulp, separates the vascular portion of the pulp from immediate contact with the enamel (fig. 10); these cells are transparent,

FIG. 10.



Transition cells from enamel pulp of lamb at birth, as they exist between the vascular portion of the pulp and deposited enamel.

and seem to correspond with the organ which develops enamel in carnivorous

teeth, and hold a relation to the diameter or thickness of the enamel which it develops; the rest of the pulp goes to form the crusta petrosa or cementum of the tooth, under which head it will be again alluded to.

The cells which float in the liquid plasma obtain a uniformity of character as they approach towards the completion of their development (fig. 11), which

Fig. 11.



Transition cells from enamel pulp of kitten at birth.

is carried out in a manner nearly corresponding with that exhibited in the development of the intertubular structure of dentine.

3. The attachment of the enamel to the dentine is chiefly mechanical; and the mechanism is so constructed as to yield the most enduring strength, both in the arrangement of the enamel cells upon the surface of dentine, and the conformation of that surface;* for although, as before stated, the process of its development resembles that of intertubular dentine, yet it is not like it deposited in layers or scales succeeding one another, each directly united to the last, but the earliest deposition of enamel upon the already developed dentine takes place in lines, which trace a series of corresponding curves in a more or less waving character upon its surface in a manner, as far as I have been able to make out, peculiar to genera.

Thus in man the curves are formed more abruptly than in any other animal that I have examined (fig. 12); while in the donkey they traverse in a line more straight, in a direction from the apex to the base of the tooth, sometimes one line uniting itself to the next. In the calf the lines are direct in their course from one extremity of the tooth to the other, and run very equidistant and

* Additional strength the connection probably receives from the ossification of the intermediate membrane, which is common to the two pulps: this idea is strengthened by the occasional passing of the dentinal tubes into the enamel. This fact is pointed out by Mr. Tomes in a paper lately read before the Royal Society, and published in the *MEDICAL GAZETTE*, to exist in the hare; but I have also seen the same phenomenon in the teeth of the ass, sheep, and monkey.

Fig. 12.

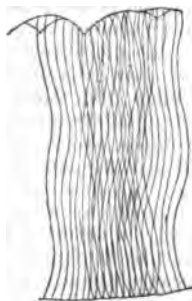


Enamel as seen first deposited on the surface of dentine; from child at birth.

parallel. Although it may be described as being deposited in lines, yet these lines are not continuous—at least, in the early part of the process; but spots are deposited, at tolerably regular intervals, in the same line, which spots become the centres of enamel growth, and continue to increase longitudinally until they form one continuous line: they also increase in their diameter, but not relatively to the length. The intermediate spaces are filled up by fresh deposit brought forward from the original centre of ossification of the enamel, in a manner repeating that which has been attempted to be described.

How far the intermediate lines which fill up the spaces between the original ones may correspond with each other, or differ in the latter deposition, I cannot say, but I believe that it is due to this cause that each row of basaltic prisms of enamel directs their curvatures in opposite ways—that is, the convex bend of one row overlies the concave of the next (fig. 13), and

Fig. 13.



Enamel prisms as they appear in relation to each other; from human tooth.

so on from the base to the apex of the prism. The whole fabric by this means receives great additional strength. But, as a structure, when completed it possesses less peculiarity in its conformation, by which its animal origin might be discovered, than any other formation of which the teeth are composed, or perhaps of any structure in the whole range of the animal kingdom above the mollusca, to the testaceous covering of which it has been compared.

4. The office which the enamel fulfils to the tooth is twofold: first, by its hardness, it precludes wearing from friction; consequently its presence will be found in all those animals in which the teeth form more important agents in the animal economy than mere prehensile organs; in some teeth covering the crown: in others it is again covered by a layer of osseous tissue (*crusta petrosa*), and only becomes exposed under the wearing influence of mastication. In the former class of tooth it fulfils a second purpose—that of protecting the dentine from the action of other external agents (chemical, &c.). To carry out this condition the enamel is peculiarly adapted, it being so closely built up that it is impervious to the most subtle fluid; and therefore, under normal conditions, no moisture external to the system can come into contact or act upon the more porous substance of dentine without previously destroying the enamel. Often has the experiment of saturating a tooth in common writing ink been resorted to as a test to show the porosity of its structure; but the evidence so obtained is most inconclusive, since all inks contain a certain quantity of acid: thus the appearance of saturation given to a tooth which has long been so immersed is due not to the porosity either of the dentine or enamel, but to the chemical action of the ink upon the substance itself.

5. The chemical composition of the enamel differs from that of dentine only in relation to the respective quantities of the component parts: thus it will be found that the true bone salt, the phosphate of lime, abounds in a greater proportion; while the organic matter and carbonate of lime are present less than in dentine. If a specimen of enamel which has been immersed for some time in ink be examined under the microscope, it will be found to be more or less equally over the whole

ternal surface; but upon cutting out a section from the deepest parts, where the action of the ink can only be seen upon the edge, it will be found that the chemical action of the ink commences first along the line which marks the boundary of each prism, and then across the diameter, at, I presume, the point of union between the several cells which unite to make up a single prism: that is, the chemical action takes place first upon the walls of the cells, as is shown in the accompanying illustration.

FIG. 14.



Enamel treated with common writing ink.

This is an interesting fact, and goes far to prove an assumption made in a previous paper, that the membrane of which the cell walls are composed ossifies through the agency of an amorphous salt of lime, which is probably the carbonate, which salt is much more readily soluble than the phosphate, and therefore first acted upon. First the cell walls yield to the chemical action of the ink, after which the contents of those cells; so ultimately the whole structure of the enamel becomes decomposed.

[To be continued.]

THE PULSE IN INFANCY.

ACCORDING to the statistical researches of Dr. Guy, the pulse in infancy varies with the sex.

The following table may, he considers, be presented as a fair average:—

	Males.	Females.
Under 2 years	110	114
2—5 „	101	103
5—8 „	85	93
8—12 „	79	92

It is thus uniformly quicker in the female than in the male, and the difference between the two at five years is very

AN ESSAY ON
UNHEALTHY INFLAMMATIONS.

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[Continued from p. 631.]

ERYTHELMA (continued).

Erythema and erysipelas very generally confounded by systematic writers—Difficulty of laying down practical rules of distinction between them—The author's definition of erythema—Asthenic nature of erysipelas in all its forms and types—This position disputed by some eminent authorities—ex. gr., Sydenham, Cullen, Dr. Andrew Duncan, jun., Lawrence—Mr. Lawrence's views of the pathology of the disease.

HAVING now disposed of that portion of the inquiry which has relation to the innate cause or essence of the disease, I proceed to the consideration of the second proposition with which I opened the subject of erysipelas—to wit, its “*immediate connection with an adynamic condition of the system.*”

If, in the prosecution of the first of these inquiries, I approached the subject with a distrust inseparable from the consciousness that its nature did not admit of demonstration, as well as from that of the difficulty surrounding it, I am discouraged in the present instance by no such embarrassing considerations; for, though I shall be thought by some practitioners to have mistaken Nature's meaning in the conditions of this inflammation, the *evidence* in support of my present proposition crowds fast and thickly upon us. The *nil quod non demonstrandum*, how applicable soever in the one case, can be allowed no “habitation” in the other, for, now our business is alone with demonstration. Before we enter, however, upon an analysis of this evidence, a question of some importance engages us *in limine*, upon which it is necessary we should have something like a definite understanding. That question is, *What is, and what is not, erysipelas?* The reader is aware that this is debateable ground, and that some of the fairest names in medicine—among the moderns

more particularly—have ranged themselves on opposite sides of the same. It is not my intention now to canvass the arguments that have been brought forward upon this vexata quæstio: I shall do little more than allude to their existence. But, it may not be a profitless task altogether, to adduce an example or two of the discrepancy of opinion to which I have adverted, and I shall borrow them from amongst the highest authorities in medicine.

It is to the confusion existing in relation to the terms *erysipelas* and *erythema* that I now more particularly invite the reader's attention.

Bichat and Dessault appear to recognise no independent existence for erythema. The former says of erysipelas—“*La première et la plus simple, et celle que Sauvages et Cullen appellent erythema.*”*

Bursarius, again (one of our best writers on erysipelas) makes no sort of allusion to erythema by such title, though he probably meant to describe that affection when he speaks of the “simple” form of erysipelas, as thus—“*Simplex quoque chirurgi illud nominant, quod nullo gravi aut periculoso symptomate stipatur.*”†

Pearson treats of erysipelas and of cedema-cum-erythemate as two diseases, and yet is very happy in his perfect fusion of, or confusion between the two.‡

Callisen describes erythema as the lightest of all forms of erysipelas, which perhaps is no unhappy definition of the disorder.§

The two Franks describe erythema and erysipelas as diseases as opposite to each other as the poles.||

Cullen lays it down that they are two disorders, and their phenomena connected with different orders of disease:—“When the disease is an affection of the skin alone, and very little of the whole system, or when the affection of the system is only symptomatic of the external inflammation, I shall give the disease the name of *erythema*; but when the external inflammation is an

* Œuvres Chirurgicales de Dessault par Bichat, tom. ii., p. 581.

† Institutionum Medicinæ Practicæ, &c., volum. secundum, p. 15.

‡ Principles of Surgery.

§ Systema Chirurg. Hadiern, vol. i., p. 483.

|| De Curandis Hominum Morbis, lib. iii. Præceps Medic. Univ. Præcepta, vol. ii. and iii.

exanthema, and symptomatic of an affection of the whole system, I shall then name the disease *erysipelas*.*

"The erythema is a nearly continuous redness of some portion of the skin, attended with disorder of the constitution, but not contagious. This appearance, though in general symptomatic, merits our attention, as it should be carefully distinguished from other eruptions, particularly from the contagious exanthemata." "The erysipelas is a febrile disease, in which some part of the body is affected externally with heat, redness, swelling, and vesications."†

Bateman is still more explicit:—"The word *ερυθημα*, as used everywhere by Hippocrates, signifies simply redness, and is therefore correctly appropriated to this affection, which differs from erysipelas, inasmuch as it is a mere rash or efflorescence, and is not accompanied by any swelling, vesication, or regular fever." Again—"The presence of tumor, together with vesication, distinguishes the disease (erysipelas) from erythema."‡

Mr. Hunter would appear to have considered the two diseases synonymous, if we may judge from the fact that he mixes up the symptoms of both in his first description of erysipelas, and makes no individual mention of erythema. Like Willan and Bateman, he describes *vesication* as a natural feature of erysipelas, though we shall presently see that a subsequent authority considers a peeling of the cuticle to be characteristic of erythema. He tells us that "the erysipelatous inflammation is very peculiar;" and that "most inflammations that are not of the true adhesive and suppurative kinds are called so, although probably they do not in the least belong to it." Mr. Hunter subdivides *inflammation* into a genus consisting of *five* species—viz., "the adhesive," "the cedematous," "the erysipelatous," "the carbuncular," and "that which leads immediately to mortification." And, while he tells us in one breath that "the erysipelatous inflammation is very peculiar," in another he remarks "it is probable there is no specific distinction between any of these inflammations but what arises from the constitution of the parts; for we find them all proceeding from what may be called the same accidental cause, which,

therefore, cannot produce anything specific."* Thus, while this classification of inflammations to myself appears both defective and contradictory, it is wanting, for our present purpose, in an intelligible definition of erysipelas, and makes no mention of the independent existence of such a disease as erythema.

Dr. Mason Good distinguishes, or fancies that he distinguishes, erythema from erysipelas by the following description:—"Erythema is local inflammation *tending to vesication*; erysipelas an idiopathic fever producing an erythematous efflorescence."* Erysipelas, he adds, is "occasionally" contagious; erythema has "no such tendency." Again, "This genus of inflammation" (erythema) has "been very generally confounded with an exanthem or eruptive fever (erysipelas) which, in one or two of its species, it frequently accompanies, but of which it is then a mere symptom."† It would not be difficult, I think, to point out the weak points of this accomplished physician's diagnosis, dogmatically as he insists upon its truthfulness: for instance, are we to understand that erysipelas never terminates by *vesication*; and that it is incapable of doing so because, forsooth, it is "idiopathic fever producing an erythematous efflorescence?" Why, upon this showing, scarlatina should be equally free from such termination, and so should typhus fever! And then, after falling foul of some of the best writers upon erysipelas (not sparing the incomparable Galen) for the confusion they evince between the two diseases, we find him betraying the inconsistency of introducing an "erysipelatous erythema" into his laboured classification of erythema (of which he makes out *seven* species), and of thereby himself acknowledging the family relationship, if not the identity between them: but, *allquando Homerus*, &c.!

Mr. Lawrence is altogether at issue with this view of the question. The two diseases (if I may be allowed the *Milesianism*) in his eyes are one and the same. "By erysipelas I understand in-

* The Works of John Hunter, "Fundamental Principles of Inflammation" vol. iii.

† The History of Medicine, by John Mason Good, vol. ii. p. 363, 4th edit.

‡ Ibid. vol. ii. p. 59.—Mason Good thus contends, with many pathologists, for the *exanthematous* nature of erysipelas, although its striking departure from the rule of that family of disease ("morbis contagiosis, *semel tantum* in decurrit: aliquem accitescent") is sufficiently well known.

* First Lines of the Practice of Physic, p. 274.

† Willan on Cutaneous Diseases, p. 472-480.

‡ Bateman on Cutaneous Diseases, pp. 118-25.

inflammation of the skin, either alone or in conjunction with that of the subjacent adipose and cellular tissue. Like other inflammations, it varies in degree. When it affects the surface of the skin,—which is red, not sensibly swelled, soft, and *without vesication*," (when doctors disagree, &c.!)—"it is called erythema." "As erythema, simple erysipelas, and phlegmonous erysipelas, are merely three degrees of the same affection, they ought not to be separated."* Mr. Arnott would limit the expressions erysipelas and erysipelatos to the *face alone*, as he looks upon the affection as peculiar to that region of the body, and as quite foreign to the nature of inflammations occurring in other parts of the external skin.†

One of the most classical writers of our own times upon this disease has sought to contract the conditions of erysipelas within a very narrow circle:—"Erysipelas is constantly confounded with inflammations of the integuments, which arise under different circumstances, and are of a distinct character. Thus the inflammatory oedema so commonly following the bites of leeches on the eyelids, prepuce, scrotum, and other cellular textures, parts much disposed to erysipelas; the sympathetic erythema, or even suppurative oedema, which takes possession of the integument of a limb, in inflammation of the fascia, veins, or absorbents, and in sub-fascial collections of matter; and the sympathetic blush of the skin covering a gangrene, as of the tunica vaginalis testis, or a mortified hernia, are denominated erysipelas, by which it is intended to designate an adventitious form of the disease. The state of the integument which results from the infiltration of the cellular membrane by an extravasated fluid, from sympathy with inflamed absorbents or veins, and from changes of the subjacent membranes, or accumulations of them, is widely different from erysipelas; and we should greatly reduce the list of cases classed and reported under this head in surgical practice if we would confine the term to those in which the skin, or skin and cellular membrane, are *primarily* affected." "Commencement in the integument, external or internal, deficiency

of adhesive inflammation, and a peculiar state of the system with which the inflammation is connected, whether spontaneous or the result of injury, are diagnostic characters of erysipelas."* And yet the terms of this definition might not be excelled in correctness if selected for an exposition of erythema, so great in reality is the difficulty of affording a verbal distinction between the two!

Andral is chargeable with the same want of precision in the looseness with which he employs erysipelas to characterise an erythematous blush of the integuments consecutive on a puncture in a dropsical individual. Hunter would have designated it "oedematous inflammation;" Mason Good "oedematous erythema;" Travers "inflammatory oedema:" while, in Andral's eyes, it is a true erysipèle. "Peu de jours après son entrée" (speaking of a patient's entrance into La Charité) "la ponction fut pratiquée: deux autres furent faites dans les trois semaines suivantes. Cependant, le malade s'affaiblissait, et son appétit avait diminué, lorsqu'à la suite de scarifications pratiquées sur les deux membres abdominaux considérablement oedématisés, une rougeur livide s'empara de la peau de la jambe droite; le troisième jour de l'apparition de cette rougeur, la peau où elle s'était manifestée était déjà frappée de gangrène; en même temps prostration rapide. Mort six jours après l'invasion de l'*érysipèle*."

"La mort fut évidemment le résultat de l'*érysipèle* gangréneux qui s'empara de la peau de l'une des jambes à la suite des scarifications. Certes, la terminaison de cet *érysipèle* par gangrène ne sera point ici attribuée à un excès d'inflammation."‡

Thus, among the distinguished authors whom I have quoted as having bestowed their attention upon this question (but who compose a small minority only of those who might be enumerated), we observe a remarkable disagreement as to the real distinctions between the two diseases,—one declaring that they are totally opposite in their characters and nature; another that there is no distinction between them: this pathologist laying it down

* "Observations on the Nature and Treatment of Erysipelas," by W. Lawrence, Esq., F.R.S.: *Medico-Chirurgical Transactions*, vol. xiv. p. 2-30.

† *London Medical and Physical Journal*, vol. lviii.

* A Further Inquiry concerning Constitutional Irritation and the Pathology of the Nervous System, by Benjamin Travers, F.R.S., p. 129-130.

‡ *Clinique Médicale*, tome iv. p. 204-5.

that vesication is the characteristic feature of erythema; that, that erythema differs from erysipelas in never vesicating: some declaring erythema to be a disease *sui generis*; others averring that there is no such affection at all!

They tell an anecdote of James the Second, that, having gone to Westminster Hall to hear the pleadings of eminent counsel, he was so bewildered by the confusions and contradictions of the *dramatis personæ* on that celebrated stage, as to have lost all patience and decorum, and exclaimed, "*Rogues all, Rogues all, to the De'il wi' ye, to the De'il wi' ye!*" I speak it not irreverently of their high mightinesses,—but what would the simple-minded monarch have conceived of the luminaries of our "craft," had he stepped in to a debate at the "Medico-Chirurgical," or, at the "Académie de Médecine" across the water, upon the question now in hand, and sought for information upon the same!

I might well be excused, then, for shrinking from a closer intercourse with this disputed point, and for leaving it in its present uncertainty. Nevertheless, I will venture to suggest the following brief outline of a definition of erythema, as offering at least an approach to a practical distinction from erysipelas.

A rash or efflorescence devoid of swelling, unattended by febrile movement, never suppurating, sometimes vesicating, sometimes not, unconnected with epidemic influences, and incapable of communication to others by contact or through the medium of the surrounding atmosphere.

I do not pretend to offer this as a definition by any means embracing all, or nearly all, the distinctive features of erythema, but merely as an enumeration of the salient points of the affection.*

* Those who may wish to prosecute this inquiry further, I may refer to Dr. Weatherhead's valuable Essay on the "Diagnosis between Erysipelas, Phlegmon, and Erythema;" as well as to the article "Erythema" in the viii. vol. of the "Dictionnaire de Médecine," by M. Rayer, where the diagnosis and natural history of the disorder are particularly well laid down, but which are deformed by the asserted connection of the same, at least in one of its forms—the "symptomatique"—with that ubiquitous monster endemic to the soil of French understandings—the *causæ-anterio!* "*L'érythème doit être combattu par les antiphlogistiques, quelle que soit sa forme; et par la saignée lorsqu'il est général.*" Ple on't! oh, fie, Monsieur Rayer! "There has been much throwing about of brains" among your countrymen in this matter!

Erysipelas essentially an asthenic or adynamic inflammation.—There is, happily, far less disagreement at the present day amongst the aristocracy of medical learning upon this question, than upon any of the preceding that I have examined in connection with this large and extended subject. With the great body of the profession, however, it is to be feared that it is still much the practice to regard it as an *healthy*, or, in other words, a common inflammation, and to administer heroic remedies for its relief. The favourers of this view of the nature of erysipelas may boast of the support of no less an authority than Sydenham, who was wont to bleed his patients largely under this disorder. "*Ubi primum accedo,*" says he, "*satis largam sanguinis quantitatem è brachio extrahi præcipio, qui quidem pleuritico-rum sanguinem ferè semper æmulatur.* Die sequente blandam illam potionem catharticam mihi in praxi familiarem exhibeo." Then follows an account of his local appliances, immediately after which he continues,—"*Hæc methodum febris, tum alia symptomata, citissimè ut plurimum fugantur.* Sin aliter, rursus venam seco; quod *et tertium* non nunquam fieri debet, interposito semper die uno, si prava nempe adsit sanguinis diathesis, et febris intensior."*

Cullen followed in the footsteps of Sydenham. "The erysipelas of the face," says this excellent physician, "is to be cured very much in the same manner as phlegmonic inflammations, by blood-letting, cooling purgatives, and by employing every part of the antiphlogistic regimen; and our experience has confirmed the fitness of this method of cure."† The same principles are enjoined in his "First Lines" (vol. ii. p. 392).

The same views have been strongly urged upon our notice by a writer of our own times, Dr. Andrew Duncan, junior, who, in the 17th volume of the Edinburgh Medical Journal, has published a series of cases of erysipelas, with the particular view of recommending free depletion for that disease. But, had not Dr. Duncan's *own detail* of those cases impressed me with a contrary view of the propriety of his practice in the same, I should have expected

* Observationis Medicæ circa morborum acutorum historiam et curationem. Febris Erysipelacea, p. 416.

† Practice of Physic, vol. ii. p. 268-8.

rienced grave suspicions on the subject, from a remark with which the paper in question sets out. "After the abundant experience we have lately had of the safety and advantage of venesection in many diseases in which it was formerly prohibited, especially in *continued fever, arising from contagion, in small-pox with typhus fever, and scarlatina with malignant sore-throat*, the extension of the same powerful remedy to another febrile disease, with cutaneous inflammation, seemed to be naturally suggested."*

We have another advocate of this view of the nature of erysipelas in the justly celebrated Dupuytren, who has lately been removed from among us. Nor am I acquainted with a happier instance than is to be found in this writer, of a complete *misinterpretation* of the pathology of the disease.

In a clinical lecture, delivered at the Hôtel Dieu, and subsequently translated for the *Lancet*, the Baron relates the particulars of four cases of what he denominates "Diffuse Phlegmon." The result of the first is not stated. The second presented such formidable appearances, that he seriously meditated amputation of the limb: the patient, however, recovered at the end of *three months* from the commencement of the attack. The third passed into a state of gangrene, but was saved after two months duration of the disease. The last died. These cases were all *largely bled and leeches*. That the reader may form his own idea of the Baron's management of particular symptoms, I shall subjoin a passage or two from his own words. *After gangrene had made its appearance* in the third case, he says

* P. 537-8.—In Dr. Duncan's second case, after *fifty-two ounces* of blood had been abstracted by the lancet, and as much more taken as could be solicited by forty-four leeches (not to speak of the collateral assistance of calomel, antimony, and jalap), the report, *ten days subsequent* to the commencement of this sharp practice, stands thus—"Erysipelas spreading down the back; and, on the following day, erysipelas continues to spread." Pp. 544-5. Some of the other cases manifest the same want of subservience on the part of the disease to the depletory measures addressed to them. But, Dr. Duncan's faith was strong in the charm of his specific, so strong indeed as to blind his judgment to the real truth of the matter: to wit, that the disease progressed in spite of the most determined application of his treatment; or, as I think myself warranted in assuming, as a consequence of that treatment. Yet, this physician was so fascinated by a theory, as to have published thirty pages of tolerably close print, whereby to introduce a misshapen offspring to public favour!

—"we immediately drew blood from the arm." "This patient continued delirious during the whole night; and the vomiting persisted to the morning: twenty leeches over the epigastrium." "On the third day the delirium had ceased, but the patient was excessively feeble: forty leeches to the thigh." The gangrene continuing to spread, the question of amputation was entertained: The erysipelas in this case ensued upon the operation of venesection in the foot; and, alluding to its severity, the Baron, after bringing the subject of it to death's door by depletion, with an artlessness deserving of a better cause, observes—"Having thus explained the unfortunate consequences of venesection, let us return to the causes of diffuse phlegmon." Finally—"The facts I have related have given you an idea of the character of diffuse phlegmon, of its danger, progress, termination, and the treatment which is adapted to it!"*

Though not strictly *à propos* to the present subject, yet, in connexion with one of the same family of diseases, I shall here indulge the reader with an agreeable and pertinent anecdote, by way of relieving the dulness of this matter of fact contribution. "The Princess Anne" (of Denmark, afterwards Queen of England) "kept the eleventh birth-day of her son, the Duke of Gloucester, with great rejoicings, little anticipating the result. The boy reviewed his little regiment, exulted in the discharge of cannon and crackers, and presided over a grand banquet. He was very much heated and fatigued, and probably had been induced to intrench on his natural abstemiousness. The next day he complained of sickness, headache, and a sore throat: towards night he became delirious. The family physician of the princess sought to relieve him by bleeding, but this operation did not do him any good. There was a general outcry and lamentation in the young duke's household that he would be lost, because Dr. Radcliffe was not in attendance on him, owing to the affront the princess had taken. Dr. Radcliffe was, however, sent for by express, and, though unwilling, he was prevailed on to come. When he arrived at Windsor Castle, and saw his poor little patient, he declared the malady to be the scarlet fever: he demanded who

had bled him. The physician in attendance owned the duke had been bled by his order. 'Then, said Radcliffe, *you have destroyed him, and you may finish him*, for, I will not prescribe.' The event justified the prediction of the most skilful physician of the age; but, he was as much abused by the people, who clung to the last scion of their native princes, as if he had wilfully refused to save the child.*

But the great champion of a depletory line of treatment in erysipelas in this country is Mr. Lawrence, whose work on the subject has so much influenced the views and practice of English surgeons in their dealings with this disease that I deem it necessary to devote some space to an analysis of the merits of that publication.

"Regarding it" (erysipelas) "as an affection essentially inflammatory, some adopt the antiphlogistic plan, including general and local bleeding; while others, conceiving that the part, the constitution or both, are in a state of debility, endeavour to remove this by the free use of stimulants and tonics, more especially by bark, ammonia, and wine. The former appears to me the correct view and practice. I accordingly consider the latter notion completely erroneous, and the treatment founded on it not only inappropriate, but injurious."

The foregoing passage will serve as a text to Mr. Lawrence's observations, the latter affording a striking and consistent commentary upon the same.

The reader, in consideration of the importance of the subject, will bear with me, I trust, whilst I summon Mr. Lawrence himself into court, and afford him an opportunity of pleading, *ipse dixit*, his own course.

"A consideration of the origin, development, and effects of erysipelas, of all its phenomena, whether local or general, leads us irresistibly to the conclusion that the nature of the affection is inflammatory. In its four leading characters of redness, swelling, heat, and pain, and in its effects of effusion, suppuration, and sloughing, it agrees with what is called common or phlegmonous inflammation; while the gene-

ral disturbance preceding and accompanying the local affection is often exactly alike in the two cases. Erysipelas, then, is merely a particular modification of cutaneous or cutaneous and cellular inflammation."

"The difference between erysipelas and phlegmon is not merely in the original seat or degree of the disturbance; there is also a difference in kind." "The most striking and important distinction between the two affections is, that inflammation is confined to one spot in phlegmon, and is distinctly circumscribed in its seat; while it is diffused in erysipelas, and spreads without limit." And then, explaining that this difference would appear to depend upon a lack of effused coagulable lymph in the latter, he continues—"We cannot at present explain the cause of this difference—that is, we do not know how it happens that coagulating lymph is poured out in the one case, and serum in the other. We are equally ignorant of the essential nature in many other modifications of inflammation which are yet obviously distinct. No one could overlook the differences between inflammation of the finger from a wound, that of whitlow, of chilblain, of erysipelas, of gout; yet, who could explain the differences of vascular action which cause these distinctions?"

"We must therefore admit, what even superficial observation will teach, that erysipelas is a *peculiar modification*—(this is *something*, at any rate, from Mr. Lawrence)—of inflammation in the skin and cellular tissue. I can, however, by no means agree with those who regard it as a distinct species of inflammation, and as capable in that character of affecting various parts of the body as well as the skin.*

Again: "I am quite at a loss to discover in this affection those marks of debility which some have so much insisted on. Erysipelas, like any other inflammation, may occur in old and feeble persons; and the effects of the disease, when aggravated by injudicious treatment, or protracted from any cause, will soon weaken the most robust; but, however weak the patient, the local disturbance is one of excitement: there is increased activity in the circulation of the part, clearly marked by all the symptoms. Indeed, speaking of the part, I

* *Lives of the Queens of England*, by Agnes Strickland, vol. xi. pp. 468-9.

† *Observations on the Nature and Treatment of Erysipelas*. By William Lawrence, Esq. *Med. Chirurg. Transactions*, vol. xiv.

* *Med. Chirurg. Trans.* loc. cit. pp. 17, 18, 19.

am unable to recognize debility as the cause of any inflammation whatever; and, in reference to the seat of disease, ~~to regard these expressions of passive and asthenic inflammation, and venous congestion, as either unmeaning, or calculated to convey erroneous notions.~~*

Without stopping at present to argue the latter point, (upon which, however, I venture to believe I could defeat Mr. Lawrence by many weighty facts and authorities), I have next to observe that our author, after stating that, "In attempting to establish the distinction between erysipelas and phlegmon, we perceive a fresh proof that they are not different in their essential nature" (p. 29), remarks upon its "causes" thus:—

"There is really no difference in this respect between erysipelas and other inflammations. The habitual excitement of the vascular system, or the long-continued disturbance of the stomach, alimentary canal, and liver, consequent on intemperance and excess, lays the foundation of inflammation generally, and it depends on individual peculiarity, or on local causes, whether the skin or other parts shall be the seat of disease" (p. 36).

[To be continued.]

DISCOVERY OF A NEW METAL. ARIDIUM.

M. UGGER has announced to the Academy of Sciences, of Stockholm, that he has discovered a new metal which he designates *Aridium*. It is found in the mineral chromate of iron of Reoras. Its oxides are analogous to those of iron, but they exhibit distinct reactions. The metal has not yet been obtained separately from its combinations with oxygen.—*L'Union Médicale*, Oct. 10th.

X

* Ibid. pp. 28-9. Mr. Lawrence would appear to have selected Dr. Andrew Duncan, Jun., (whose views on erysipelas he frequently quotes with much satisfaction,) as a model whereon to form his own views of inflammation. "I have long been inclined," says the latter, "to consider inflammation, upon whatever proximate cause it may depend, as identical in kind, and differing only in degree, or in respect to the texture or functions of the part affected; and that, with slight modifications, the same treatment is adapted to all." "I believe that there is no such thing as typhoid or asthenic inflammation, neither do I conceive that the disease differs in kind, or requires an opposite treatment, because it attacks a different texture, and is diffuse and erratic, instead of being limited and fixed."—*Edin. Med. Journ.* Loc. cit.

ON

RETENTION OF THE PLACENTA.

By B. W. Richardson,

M.F.P. and S., Glasgow.

THERE are in obstetric practice, as is well known, three causes of placental retention: the first arising from adhesion of the placenta to the walls of the uterus; the second, from an irregular contraction in some part of the body of the uterus, in which the placenta is grasped; and the third, from the contraction of the cervix uteri upon it while it is passing from the uterine cavity into the vagina. The first of these causes is least common; the second, more so; and the third, most of all. Of the first I do not wish to speak now as to treatment, but to state a simple plan which I have for some time past successfully been pursuing in practice for avoiding the occurrence of the two latter, and which I do not think has ever been employed before. It is this: Placing the patient in the usual position on the left side, I order, as soon as the head of the fetus is born, the hands of a nurse to be placed, the one over the uterus on the abdomen, the other on the back of the patient, and direct her, as she feels the uterus sinking into the pelvis and becoming smaller, to follow it up with firm grasping pressure. Then, in aiding the delivery myself, after the shoulders are delivered, and the danger past of the perineum being torn, I support the born part of the child with my right hand, and, passing two fingers of the left hand on to the body of the infant, seize the cord about three inches from the umbilical opening, and make gentle traction downwards as the remainder of the child is expelled, by which process the placenta is made to descend from the uterus with (i. e. immediately after) the feet, and so into the vagina before the uterine contractions entrap it. By this simple procedure I always ensure a safe and rapid delivery of the placenta, when there is no adhesion, and when the head presents; and since using it I have never had the placenta retained by contraction, with one exception, though I have often felt the uterus irregularly

contracted when empty, as in the second case related by Mr. Kirk.*

The operation is most easily done, is attended with no risk to the child, nor is the traction required such as to cause the least danger of tearing the cord; while, at the same time, the external pressure alluded to is an excellent means for keeping up the contractions of the uterus, and for preventing hæmorrhage.

In conclusion let me add, that I was led to adopt this plan by observing that in numerous cases, when the cord was folded round the neck of the child, the placenta was always expelled at once; and as I could only account for this by supposing that traction had thus been accidentally made, and had drawn down the placenta, I determined that in the next case where the cord was natural, to make gentle traction as described. The trial was very successful, and has been since so often successfully repeated, as to lead me to think that, were the system universally adopted, the introduction of the hand through the vagina would never be needed but in cases of adhesion, and that thus our fair patients would often be relieved from what is always a painful operation, be it performed ever so expertly and delicately.

Sept. 20, 1890.

CASE OF BIRTH AFTER THE DEATH OF THE MOTHER.

DR. SCHNEIDER relates, that being summoned in haste to a woman in labour, he found her dead on his arrival. On placing the hand on the yet warm abdomen, he felt the uterus contracted and sunk in the pelvis. By an examination per vaginam, a foot was detected, and by rapidly completing the delivery he had the satisfaction of bringing into the world an apparently still-born child, which, however, soon revived.—*Casper's Wehenschrift*.

* Dr. Schneider also cites instances, from the German journals, of the expulsion of the ovum at some distance after death, and when even putrefaction had occurred. Is it not probable, however, that the phenomena were due to the pressure exerted on the then soft and yielding uterus by the gases liberated in the intestines, as the result of decomposition? X

* Vide MED. GAZ., Sept. 25, 1890.

ON THE FREQUENCY OF THE ESCAPE OF THE LUNGS

IN GUN-SHOT WOUNDS OF THE THORAX.

By E. H. A. HUGHES, Staff Surgeon.

Vulnera thoracis.—Here we mean only very briefly to advert to the frequent escape of the lungs in small gun-shot wounds of the thorax. At Kilat, for instance, in November 1839, where the ordinary ball was that of the small rifle matchlock, we had twenty-one through the chest in the 2d, or Queen's Royal, then about 800 strong. Of these ten or eleven proved fatal immediately, or within a very short period. Among the immediately fatal was that of an officer, Lieutenant Gravatt (a very old and intimate friend), in whom the ball entered a little above the right nipple; but of the rest, only one proved fatal even eventually. In this a crepitating rale was distinctly audible next day in the vicinity of the wound, when first examined with the stethoscope, and which rapidly extended as from a centre till about the third day, when he died; but in none of the others could anything abnormal be detected in the murmur, though, from the site and direction of the wounds, it seemed almost impossible for the lungs to have escaped. One, too, an officer, was expectorating even frothy blood when first seen, yet he also recovered, without a single bad symptom afterwards, and is still in the corps. It may not, however, just be admitted that, because no crepitating rale followed, therefore there was no wound of the lungs, as that would but indicate at most a consequent pneumonia. Nevertheless, we implicitly relied upon this, and in every case the prognosis was fully verified by the results. At Ghuznee, however, what seemed only to be a wound of the thoracic walls soon afterwards proved fatal, and very unexpectedly; but, as the stethoscope was not made use of on this occasion, it bears less upon the point. The ball had penetrated directly through the breastplate; and, after being traced round by the emphysematous crackling and lividity, was cut out about the scapula, but the piece of the plate was not found. It is therefore possible that it may have

lodged where the ball first diverged, and eventually found its way into the thorax. We were ourselves then at Caubul, nearly a hundred miles off, but understood he had suddenly become very ill, and died shortly after. There was, I believe, no autopsy.

Also in our jungle sports in the East we were frequently disappointed with these gun-shot wounds of the thorax; for this, in the larger carnivora, was my favourite aim, and that of many others at my recommendation (for who should know so well as the doctor?); the smaller size of the head, and the strength and obliquity of the cranium in these animals, rendering that, as we thought, very uncertain. On one occasion we had to put no less than five balls (about musket bore) through the chest (most of them quite through) of a royal tigress before she was finally settled; and on another occasion a royal tiger went off as if untouched after two balls, though it was subsequently discovered that one of them had transixed him, as it were, from the right hypochondrium to the left axillary region, where it was found under the skin quite in a pool of blood. The other ball was in the liver.

Comparing these with the results of gun-shot wounds of the head, abdomen, and pelvis, we do think the thoracic organs, and particularly the lungs, are the least vulnerable parts of the body, but when wounded are more frequently followed by fatal results than even they are assumed to be. Taking, for instance, again, the 2nd, or Queen's Royal, at Kilat, the balls and other circumstances being equal: in six wounds of the head five proved fatal; of nine of the pelvis eight (the only recovery an officer, in whom the ball passed through the os ilium from in front without wounding the peritoneum), and every one of the abdomen. Those of the head were immediately fatal, as were those of the chest generally: those of the abdomen and pelvis within eighteen or twenty hours.

Bristol, Oct. 5, 1850.

CASE OF INDURATION OF THE BRAIN.

M. DUFLAY has communicated to the Biological Society of Paris the following case:—

— Vautier, aged 41 years, was admitted on the 22d June, 1849, into the Hospital of Incurables. He had complete paralysis of the lower extremities, and the

superior extremities also were weak, and their movements feeble and uncertain. His intellect seemed unimpaired: he understood what was said to him, but had difficulty in articulating.

His history was as follows:—He had always enjoyed good health until the spring of 1839. He was strong, and had followed the trade of a locksmith. He had not been addicted to excess in drinking, but he had not been equally prudent as regarded sexual gratification. In the course of 1839 he frequently experienced numbness of the lower extremities: these became during the same time weaker. Walking, at first uncertain, became more and more difficult, until he was obliged to have recourse to crutches. At the end of two years he had completely lost the use of both limbs. Caution, moxas, and all other means, were employed without avail. In the course of the next year a difficulty occurred in speaking, and in a short time he became almost unintelligible. The sphincters had also become paralysed.

In October 1849 he was admitted into the Infirmary, suffering from pericarditis and pleuritis on the left side, from which he recovered in the course of fifteen days, although his system received a severe shock from the additional maladies.

The morbid condition which he presented while in the Infirmary, and which remained until the day of his death, was as follows:—His legs were flexed on his thighs, and these on the pelvis; the prominence of the flexors over the extensors was so great, that, if the limbs were extended, they suddenly returned to the flexed state; the sensibility of the surface was not impaired; the senses and the intellect also retained their integrity. The patient gradually became weaker, and sank from the effects of profuse suppuration from ulcers of the integuments on those parts exposed to pressure. He died on the 23d November, 1849.

Autopsy.—The cerebral arachnoid presented a slightly opaque appearance. There was a small quantity of serous effusion beneath the arachnoid. There was no alteration in the grey substance of the brain. The outer third of the white substance was also unaltered; but, of the next two thirds, in proportion as the ventricles were approached, the substance was so much indurated that it was difficult to tear or crush it with the fingers. This portion of the brain had lost its whiteness, and had become of a dull yellowish colour. The optic thalami and corpora striata had undergone the same induration, but in a less degree, as also the Pons Varolii. The spinal cord appeared to be atrophied.—*Gazette Médicale.*

X

MEDICAL GAZETTE.

FRIDAY, OCTOBER 25, 1850.

At a meeting of the Middlesex magistrates, held on the 17th instant, a resolution was moved by Mr. Armstrong to the following effect.—

"That the Court is of opinion that measures should be taken for the substitution of a fixed salary in lieu of fees to the Coroner, and for the appointing of a medical officer to the Coroner's Court for the purpose of post-mortem examinations."

In support of this proposition, Mr. Armstrong stated that the practice of remunerating coroners by fees upon the number of inquests held was highly objectionable, since the power of determining when an inquest was to be held was exclusively in the hands of the officer who was the recipient of the fees. Although there are many honourable exceptions, there can be no doubt whatever that this mode of payment leads to the holding of more inquests than are absolutely necessary. As a confirmation of this view, it was stated that, since the inquiry out of which the present motion had arisen, there had been a marked diminution in the number of inquests in the county of Middlesex. We have no hesitation in stating our belief that, in thirty per cent. of the sudden deaths, inquests would be found entirely unnecessary if a proper medical inquiry were made into the circumstances attending death. The natural causes of sudden death are now very well understood. In the days of Edward I., when the law regarding inquests first came into force, "sudden death" uniformly implied the dagger or poison. Acting upon the ancient statute, many coroners have persisted in holding inquests upon cases which any expe-

rienced medical practitioner would have pronounced beforehand to be merely cases of death from natural causes—poisoning being impossible, either from the nature of the symptoms, or from the circumstances under which death took place. Of course, in a case involving the least degree of suspicion, there should be an inquiry: nothing less than this will satisfy the demands of justice, or clear the characters of persons unjustly suspected of having caused the death: but an exception of this kind certainly does not apply to numberless inquests in which verdicts of "death from apoplexy," "death from natural causes," "from the visitation of God," and of "found dead," are returned. The inquest is held; medical evidence is not required, or a neighbouring practitioner is called, but is not permitted to make a post-mortem examination, as it is deemed unnecessary; and the coroner and jury are quite satisfied without putting the witness to the trouble, or the county to the additional expense. How a really mysterious case of sudden death is to be cleared up, and the public justice of the country be satisfied, by such a mode of proceeding, we are at a loss to comprehend. These are the unnecessary inquests which the motion of the Middlesex magistrate, if ultimately carried, will assuredly clear off, to the great relief of the County rates.

The Coroner is, we believe, the only judicial functionary who is now paid by fees on the cases which come before him. He certainly is the only judge who can create cases for his own court; and is at the same time paid a certain sum per case. This is surely a strange and ridiculous anomaly in our criminal jurisprudence: it places an honourable man in a very unsatisfactory position, and it throws a great pecuniary benefit into the hands of one who may not be over scrupulous in the selection of cases for inquiry. Let us imagine that a

County Court judge is paid in a similar way, and we shall soon perceive the gross absurdity of the present system of remunerating coroners. A parochial rumour reaches the ears of the clerk of the County Court that A owes a certain sum to B, and will not pay him. The judge directs inquiry to be made, finds that the report is well grounded, and compels B to come into Court to prosecute his claim. Litigation would then be compulsory on all creditors in order that the judge might receive his fees. We admit that criminal differs from civil jurisprudence in the mode in which it is carried out, and in which it affects individuals; but the difference is certainly not of that nature to require a different mode of payment to the judges. A metropolitan magistrate is not paid so much per case for the cases brought before him,—sometimes the same cases as those actually under investigation by the coroner; and there is not the smallest reason why the latter functionary should not, like the magistrate, be paid by a yearly stipend. Common sense is in favour of the change; and we have reason to believe that it is strongly advocated by many coroners whose names are honourably known to the public.

It is proposed, however, to carry further this reform of the Coroner's Court. It is suggested, that instead of employing medical practitioners indiscriminately, and paying them a certain fee for each post-mortem examination, it would be better to appoint a *Medical Officer* at a fixed salary, whose duty it should be to superintend all post-mortem examinations in the county. In the opinion of Mr. Wakley, one medical officer, by a proper arrangement, would be able to make all the post-mortem examinations which the coroners of the county might deem advisable. We have no means of ascertaining correctly the number of inquests held yearly in Middlesex; but the post-mortem examina-

tions, unless the inquests be greatly reduced as an effect of the change, are not likely to be fewer than six hundred per annum. This would be a very laborious office, for any one individual: it could only be held by a man of experience, and would therefore require a high salary, with provision for an assistant. Nothing is said respecting chemical analyses; but it is not probable that any one would be found with the experience and ability requisite for conducting these, and at the same time of making the numerous post-mortem examinations required by all the coroners of Middlesex.

Another excellent suggestion is, that the inquests, instead of being held in low taverns or inns, should be conducted with all due formality in buildings or rooms set apart for the purpose. In the metropolis the court might be held, with greater convenience to all parties, in the vestry-room of the parish; and in the provinces, if not in the vestry-room, at the police station.

The proposition for thus reforming the office of coroner, at the recent Magisterial meeting, met with only two supporters. One magistrate, who thought *sufficient data* had not been put before the Bench for their guidance, moved the following amendment to the resolution, and this amendment was carried by a majority of ten:—

"That it be referred to a Committee to consider whether it would be for the benefit of the county that any and what different arrangement should be made for the performance of the duties now devolving upon the coroners, or respecting the remuneration annexed thereto, and to report their opinion thereon to the Court."

This is a quiet and respectable attempt to postpone the matter *sine die*; but we think it must fail. The magistrate who moved the amendment, and who talks of there not being "*sufficient data*," &c., can know little or nothing

about coroners' inquests, or the mode in which they are conducted. The abuses which it is proposed to remedy have been for years the subject of disgraceful contests between magistrates and coroners, and of unseemly disputes between these officers and medical witnesses. Every grown person in the habit of reading the newspapers is well aware of the necessity of a change; but the magisterial opponent of reform in the coroner's court either does not indulge in this habit, or, like Rip Van Winkle, he has just awakened out of a long slumber, and is wholly ignorant of what has been passing around him.

We hope that, now the subject has been mooted, it will not be allowed to drop. The proposition of Mr. Armstrong contains the germ of an excellent measure of reform—one which we think will be satisfactory to coroners, to medical practitioners, and the public. If carried out in a proper spirit it cannot fail to improve the administration of criminal justice.

We have received the following letter in reference to some remarks lately made in this journal on the new patent process for poisoning the public by sugar prepared with subacetate of lead:—

"Sir,—I read with great interest the letter of Mr. Herapath, and your remarks upon it, in the No. of Sept. 20, 1850, p. 518. Your two conclusions seem quite legitimate. This infinitesimal quantity (1-500,000th part of carbonate of lead), according to Mr. Herapath, was quite sufficient to endanger health; and, secondly, Mr. Herapath has done good service by showing how minute a quantity of lead will affect human health. With these two deductions, legitimate, I think, from the premises, I cannot reconcile the statement with which you conclude the notice of the election of the medical officers to the Hahnemann Hospital—"If Hahnemann's principles be ~~not~~ ^{fully} carried out in the new Hospital in cases of acute disease, we predict that the emoluments

of the coroner for Middlesex will be very considerably increased."

It seems, indeed, strange that Mr. Herapath's skill in detecting the action of an infinitesimal quantity of lead on a person should be recognised, and the discovery by homœopaths that the action of medicines in infinitesimal quantities upon diseased states should be condemned; and that Mr. Herapath should be regarded as having done good service by showing how minute a quantity of lead will affect human health; and homœopathic practitioners, for using infinitesimal doses, which they find will affect human diseases, for the cure of those diseases, should be liable to the tender mercies of Mr. Wakley, and should be considered as fit game for the coroner.

This latter conclusion of yours seemed the more extraordinary, because the whole tenor of your valuable journal is to show the incompetence of the non-professional public to judge medical questions; and yet here you write, approvingly, of submitting a question of medical treatment to twelve jurymen.

A SUBSCRIBER AND CONSTANT READER.

We must inform our friendly monitor that he has committed several notable errors in endeavouring to draw an argument in favour of the efficacy of homœopathic doses from Mr. Herapath's observations on the action of lead in river water, and from our comments on his letter. It is not implied that each person took one half-millionth of a grain of lead, and suffered any observable symptoms from this dose. On the contrary, it was to the constant use of this water, containing *one grain of lead* in nine gallons, and to the well-known accumulation of this metal in the system, that the injurious effects of lead were due. The same would happen whether the lead were prescribed in this dose homœopathically, or taken accidentally, provided it were frequently repeated for a considerable time. The writer of the above letter leaps to an extraordinary conclusion, if he considers that a *decillionth* taken at one dose will be as potent in its effects on the system

as a half-millionth proportion of the carbonate of lead diffused through water which is continued daily for weeks and months together,—the lead acting not by its small proportion, as contained in the water, but by its special accumulation in the system. The effects of these small doses of lead are often not manifested for many months, and then only by their aggregation. In homœopathy it is pretended that a special effect is produced in a few hours by a globule containing only a millionth-millionth part of a grain of something,—*e. g.*, charcoal, sulphur, or Cayenne pepper, which in the dose of a grain is without any action on the body! This is obviously a gross and barefaced assumption, not only insusceptible of proof, but in complete violation of common sense. It is, when men pretending to medical knowledge, give what they profess to be billionth and decillionth doses, to persons labouring under acute diseases, requiring the active and immediate application of visible and ponderable remedies, that we think a coroner's jury justified in inquiring into the cause of death, and whether the patient has not fallen a victim to positive neglect. We cannot look upon the "globule" system as *medical* treatment, and herein we wholly differ from our correspondent. We defy him to prove that there is *any* medicine in any one globule prepared *bonâ fide*, or that there is any demonstrable difference in any two globules labelled differently. A homœopathist treating a case of pneumonia according to the globular system, resembles an orthodox practitioner who professes to treat it with pills of *mica panis*. In the event of death in either case, there should be a coroner's inquest.

Reviews.

A Theoretical and Practical Treatise on Human Parturition. By H. MILLER, M.D., Professor of Obstetrics and the Diseases of Women and Children in the Medical Department of the University of Louisville. 8vo. pp. 468. Louisville: Cowling and Davies. London: Delf. 1850.

On the Theory and Practice of Midwifery. By FLEETWOOD CHURCHILL, M.D., M.R.I.A., &c. &c. 2d edition, corrected and improved. Small 8vo. pp. 496. London: Renshaw. Dublin: Fannin. 1850.

We here bring under the notice of our readers two works on Obstetrics, from the pens of authors deservedly celebrated, in their respective spheres, for their practical experience and scientific attainments. We may therefore, with advantage, take the opportunity to compare on a few points, as the tests of the whole, the present state of obstetric science in the American, and in the British, schools of Midwifery. We institute this comparison entirely free from national prejudice, our object being to show, that, in every branch of the Anglo-Saxon family, the arts and sciences which benefit mankind are equally progressive, and are equally adorned by their professors.

As our readers may probably not be so well acquainted with Dr. Miller as with Dr. Churchill, we shall take the liberty of introducing him in his own words, adding, that, for his works more than for his words, they will do well to cultivate his acquaintance.

"You are to know, indulgent reader, that the author of the work you are now peeping into is a man rather advanced in life (though you need not be precisely informed of his age—that is a delicate point*), who has seen much and read some, touching the subjects he proposes to handle; and has withal been accounted qualified to teach others what it has been his chief pleasure to practise, since he was a very young man. He loves his

* So "delicate" a point is this among our American brethren, that lately, having an American lady under our care whose age it was of some importance to know, we were informed by her husband that he did not know his wife's age. That it would be received as a gross affront to ask the age of any individual, male or female, in the United States!

vocation, notwithstanding the difficult and important duties it imposes, and would not exchange it for any other; to this attachment, more than to superior capacity, he honestly ascribes whatever proficiency or eminency he may have attained in it. Reader, if you have not this love, you have mistaken your calling. For the rest, the author is a backwoodsman, having been brought to light in Kentucky by a process which it is his purpose to unfold in this treatise. His education was not acquired in academic halls, but in the primitive school-houses of his native state, and upon the ample sward, shaded by forest trees, appurtenant thereto: so that, you see, he was reared after the fashion of Socrates, imbibing knowledge in the schoolhouse under the shade of trees, and not unfrequently perched upon their boughs." (*Preface*).

Dr. Miller states that the aim of his book is no lower than to establish a classification and nomenclature for foetal presentations and positions. The classification which he recommends is that of M. Dugès; the nomenclature is his own. His principles of practice are those of Hamilton and Burns; and although the author considers that by this confession of his principles he incurs the application of the epithets "officious" and "meddlesome," we think that no person who carefully studies Dr. Miller's work will be warranted by his own experience (if he have any) in lightly making such a charge.

The subject of Dr. Miller's work is then, as just stated, the process of parturition; with especial reference to the nomenclature of midwifery. It contains, first, observations on the obstetric characters of the pelvis; and the obstetric aptitudes of the foetus and its appurtenances, the membranes, placenta, &c. The description of the phenomena of labour is preceded by the consideration of the structure of the uterus, its muscular and nervous systems, as expounded in Dr. Robert Lee's investigations, "who," in the words of Dr. Miller, "has laboured more successfully in this branch of obstetrical anatomy than any of his predecessors." The cause of labour is next considered at some length by Dr. Miller; this, which Dr. Churchill observes, "all search has failed to discover," is supposed by Dr. Miller to be explained by "irritation of the cervix, and especially of the os uteri, arising from the contact of the ovum with it,"—a theory, however, which, Dr. Miller remarks, originated with Dr. Power.

Labour is divided by Dr. Miller as by most authors, into three stages. The consideration of the management of these is succeeded by that of the various presentations and positions of the foetus, and of their several and particular treatment. The conduct of the third stage of labour, and of asphyxia neonatorum, of morbid retention of the placenta, and of uterine hæmorrhage, concludes Dr. Miller's work.

Our readers are familiar with Dr. Churchill's name, as that of an obstetric authority of high reputation, founded on the soundness of the principles of practice which he has taught, and upon the extent and accuracy of the statistics on which he has based the theoretical deductions, or whence he has drawn his practical conclusions. Many also among our readers are, we doubt not, acquainted with the first edition of Dr. Churchill's "Theory and Practice of Midwifery." The author's object in the publication of that work was "to offer to the student a work embracing the modern discoveries in the physiology of the uterine system, with all the recent improvements in practice, in a condensed form." That this object was fully attained has been affirmed by the general voice of the obstetric department of the profession. That the second edition has in no degree departed from the "object" of the first, we take upon ourselves to affirm. No improvement in practice has been overlooked, and no mere innovation, of any importance, has been suffered to pass unrebuked. We are disposed to make one single exception to our remarks here. Dr. Churchill approves of the employment of anæsthetics in labour, we think, on insufficient grounds. As we have, however, on former occasions, fully expressed our own views on this question, we shall dwell upon it no further at present. We would add that the illustrations, which are abundantly furnished, are both apposite and well executed.

Dr. Churchill divides his work into three portions:—

1. The anatomy of the pelvis and organs of generation.
2. Of the physiology of the organs of generation.
3. Of parturition.

With a *parallel* we have over the two *first* *Churchill's work*.

simply observing that they contain a very large amount of valuable information on the points of which they treat. We shall confine our attention to the third division, which is concerned more exclusively with the process and management of labour in all its varied forms, natural and preternatural.

We notice in the first place the more especial object of Dr. Miller's work—the nomenclature of midwifery. The classification of parturition adopted by both the authors before us does not differ much. Thus Dr. Miller adopts that of M. Dugès, which admits *five genera and fourteen species*; by *genera*,

meaning presentations, and by *species*, positions. The *back* of the fetus serves as the point of comparison, and is placed anteriorly or to the left, in the first species of each genus—posteriorly or to the right, in the last.

Dr. Churchill arranges every variety of *presentation* under four heads, and adopts M. Naegle's classification of *positions*.

We have placed the classification adopted by the authors in juxta-position, to show their points of resemblance and of difference. These varieties are, by both authors, arranged in the order of the frequency of their occurrence.

MILLER.	
GENERA. Presentations.	SPECIES. Positions.
I. Vertex.	1. Back anterior and left. 2. Back anterior and right. 3. Back posterior and right. 4. Back posterior and left.
II. Pelvis.	1. Back left. 2. " right. 3. " anterior. 4. " posterior.
III. Face.	1. " left. 2. " right.
IV. Right shoulder.	1. " anterior. 2. " posterior.
V. Left shoulder.	1. " anterior. 2. " posterior.

CHURCHILL.	
Presentations.	Positions.
1. Head.	1. Posterior fontanelle to left foramen ovale. 2. To right foramen ovale. 3. To right sacro-iliac synchondrosis. 4. To left sacro-iliac synchondrosis.
2. Breech, including the hips and loins.	1. The back of the child, anteriorly.
3. Inferior extremities, including the knees and feet.	1. The toes directed backwards. 2. Do. forwards.
4. Inferior extremities, including shoulder, elbow, and hand.	1. Back of the child, anteriorly. 2. Do. posteriorly.

The following quotation from Dr. Churchill's work will show that it is more easy to classify positions and presentations at the study table, than to detect their diversities at the bed-side. With reference to head presentations, this author remarks:—

"There is a great difference of statement, however, as to the comparative frequency of the *second* and *third* positions; thus Naegelè, in 1290 cases, only met with the second position in *one* instance, or in the proportion of .07 per cent. M. Halmagand describes it as occurring in 5 per cent.; Madame Boivin in 19 per cent., and Madame Lachapelle in 20 per cent. On the other hand, Naegelè found 359 cases of the *third* position in 1210 cases, or 29 per cent.; while Madame Lachapelle gives only .077 per cent. of such cases, and Madame Boivin only .06. Dr. Simpson observed accurately the positions in 336 cases of cranial presentation; and found the first

position in 256 cases, the second in 1, the third in 76, and the fourth in two cases.

"It is extremely difficult to explain these discrepancies satisfactorily. M. Naegelè conceives that the examination was not made until after the change from the third into the second position had been effected; and he thinks that this opinion is confirmed by the fact that the frequency of the second position of authors agrees with the frequency with which he has observed the head to present in the third position."

Dr. Churchill concurs in this opinion.

We may in the next place compare the authorities before us, on the operation of *turning* or *version*, it being one requiring some judgment in determining upon its adoption, and skill and care in its performance. Both authors are agreed upon the most favourable moment for the attempt—that is, when the os uteri is fully, or nearly fully, dilated, and the membranes are still entire, or

as soon as possible after the rupture of the membranes, before the presenting part shall have become impacted by the uterine efforts into the upper strait.

Dr. Churchill advises that where the membranes have been ruptured for some time, as is often the case before additional advice is obtained, and the soft parts of the woman have become hot, dry, and tender, and in an inflamed and tumefied state, that recourse should be had to bleeding or opium, or to the employment of tartarized antimony, as recommended by Dr. Collins, of Dublin. Dr. Miller, as we understand, counsels turning at an early period to avoid these ill effects, and in this opinion we feel disposed to concur. Dr. Miller recommends, with reference to the position of the patient in turning, that the patient be placed

“across the bed, upon her back, and with the hips so near the side of the bed, that the perineum projects a little over the mattress on which she lies. Her feet may rest on chairs, or in the laps of two assistants, who are charged with keeping her knees far enough apart to make room for the operator to stand or sit between them. A sheet or blanket, according to the season, must be thrown over her to screen the patient from exposure, which is as indelicate as it is unnecessary; for the operation from first to last is to be performed under the guidance of touch alone.” (p. 396.)

This position is not advised by Dr. Churchill; it is not one which we should be disposed to adopt, neither is it generally, if even, ever, employed, in British obstetric practice. The position on the left side is preferred with us, as in ordinary labour; it has the advantage, besides that of having a more decent appearance, of admitting of more free motion of the operator's arm in following the axes of the pelvis, and adapting itself to the positions of the child.

Dr. Churchill does not give explicit instructions as to the choice of the hand with which the operation is to be performed; we therefore infer that he intends the right hand, as in ordinary labour, in conformity with British practice generally.

Dr. Miller, however, lays down the following rule:—

“The right hand must be used for the right shoulder, and the left hand for the left shoulder. When there is obscurity or uncertainty as to the presentation, the right

should be preferred, because it is that which most persons are accustomed to employ, and can, therefore, use with the greatest facility.”

Dr. Miller attaches considerable importance to the choice of the hand in turning; he observes:—

“The choice of a hand for the operation implies a perfect knowledge of the presentation, which ought, in fact, always to be attained, whenever it is practicable. This is the more necessary where the uterus is in a contracted state, as by guiding the hand in the right direction, namely, towards the feet, we shall be saved a deal of toil.”

Diversity of opinion exists as to the best method of bringing down the feet when they have been reached. By some authorities, delivery by turning has been advised to be completed as a perfect breech presentation; others recommend that the knees only should be brought down; while again it has been proposed by Dr. Radford “to bring down one foot only to save the trouble and difficulty of seeking for the other; and inasmuch as the breech with the thigh turned up is more bulky than the hip with the leg extended, by so much the passage will be the better prepared to admit the quick transit of the child's head, upon which the safety of the infant depends.” (Dr. Churchill, p. 269.) We have ourselves found this statement to be practically trustworthy. Dr. Churchill observes, in reference to arm and shoulder presentations:—

“The turning of the child is accomplished *during an interval of pain, the feet being brought over the front of the child, and not over the back, which would risk dislocation of the spine; and as the feet are drawn down the hand will ascend.*”

From this extract, the use of the word *feet*, which we have marked by italics, indicates the author's practice, which corresponds with that of Dr. Miller. There are of course cases in which either of these physicians would content themselves with bringing down one foot only.

Connected with the subject of turning, it may be proper here to notice Dr. Churchill's condemnation of Dr. Simpson's proposal, lately published in the Provincial Medical and Surgical Journal, to substitute turning as an alternative for craniotomy and the long forceps in deformity of the brim of the pelvis. Although this proposal has been made known through the pages of the Provin-

cial Journal; with the exception of Dr. Radford's criticism in the pages of the same journal, Dr. Collins's letters to Dr. Simpson, and the remarks of Dr. Churchill in the work before us, we have met with neither examination nor disapproval of this monstrous proposition (which, by the way, has not even originated with Dr. Simpson, although he does not state that he is indebted to Velpeau for the idea, as stated by Dr. Churchill.) We quote a few remarks from Dr. Churchill on this matter:—

"Now let us examine into the practical application of his proposal. The bi-mastoid diameter, in the six cases of measurement he gives, varied from $2\frac{1}{2}$ in. to $3\frac{1}{2}$ in.; and a living child can pass through a pelvis of $3\frac{1}{2}$ in. antero-posterior diameter, with or without the forceps. With a pelvis of this size the operation is then unnecessary; and if the antero-posterior diameter of the pelvis be less than $2\frac{1}{2}$ inches, the operation would be impracticable. Then these are the limits of the operation: for us to attempt to drag a child through a smaller space would be unjustifiable. For the success of the operation, then, we must be able to ascertain that the pelvis is within these limits, and perhaps in some few cases, with whose former labours we are accurately acquainted, we may do this, but in an immense majority of cases it will be, I think, impossible; and it happens, as Dr. Collins has shown, that the greater number of cases of difficult labour he met with were first cases, in which, of course, no such precise judgment could be obtained." (p. 263.)

Dr. Simpson would have it to appear that he has supported his views by statistics taken from the works of Dr. Collins and Dr. Robert Lee; but, as Dr. Churchill observes, he has done this "without sufficient care and caution." Certainly Dr. Simpson has adduced no experience of his own in support of this extraordinary proposition, beyond the relation of *one* case, and the loose statement that he has acted upon it "in other instances in which the head of the child had been morbidly detained at the brim, from the existence of a disproportion between the two." Beyond hypothetical reasoning on the dimensions of the pelvis, on the capability of the head of the fetus to suffer compression, on the capability of the child's neck to bear traction, and on the dangerous effects of duration alone of labour as far as the mother is concerned, we discover nothing in Dr. Simpson's papers which

have the slightest pretensions to be regarded as proved facts, in support of his proposition.

Dr. Simpson has, however, applied to Dr. Collins for his opinion of his proposed practice: we have a copy of Dr. Collins's answer, from which we make the following extract:—

"The first statement I shall notice is the *partial*, and consequently *erroneous* calculations, you have given from my work, withholding the *total mortality*, as well as the *cause* of the fatal result in *every case* of protracted labour met with out of the vast number of sixteen thousand six hundred and fifty-four births minutely recorded by me. Surely this is a great omission on your part; and thus, as you so justly condemn in others, you have drawn your deductions 'not from the *whole* of the practice, but from parts only: you have not reckoned upon the certain results of the *general* collection of facts, but depended upon the fallacious results of isolated and individual instances.'"

So much, then, for the statistics which Dr. Simpson borrows in order to support his new line of practice, which consists in dragging a child's head *vi et armis* through a contracted pelvis, almost to the certainty of decapitating the child, and the infliction of fatal injury upon the soft parts of the mother's pelvis.

But we again quote Dr. Churchill on this matter:—"If in turning with an ordinary-sized pelvis, one-third of the children are lost, the mortality will surely be more than doubled if its diameter be reduced more than one-fourth." Again, if the relative size of the pelvis or head be miscalculated, Dr. Churchill justly remarks, "We must then craniotomise the child, after incurring the hazard of turning, and in a most unfavourable position." But we must not further dwell upon this subject on the present occasion: we shall thus, entering our solemn protest against a proceeding so rash, so unscientific, and so dangerous, content ourselves with referring our readers to Dr. Churchill's work, to Dr. Collins's Letters, and to Dr. Radford's papers in the Provincial Journal, in all which they will find its enormity well exposed.

The nature of the instrumentality by which the placenta is detached is a question of great interest in connection with the occurrence of *post-partum* hæmorrhage. We shall therefore place

before our readers a full statement of the views of the authors on these subjects.

Dr. Miller observes:—

"In many cases of labour there can be no doubt that the pain which expels the child detaches the placenta at the same time; for it can be felt by the finger over the uterine orifice immediately after the birth of the child. Where, however, this does not take place, and the separation is a distinct and special part of labour, it will be found, I apprehend, that *tonic contraction* of the uterus is the means employed by nature to accomplish it. This is not the account usually given by writers, who speak of the return of pain (muscular contraction), after a longer or shorter interval, to separate as well as expel the placenta and membranes. From many observations, carefully made, I deem myself justified in concluding that, when the placenta is not detached by the last labour—three preceding the expulsion of the child, it is by the agency of the tonic contraction alone that the uterus dissolves the connection between itself and the placenta. I have many times introduced my fingers up to the os uteri, passing them along the cord as a conductor, immediately after the birth of the child, without being able to reach the placenta; and I have repeated the examination several times at short intervals, until the placenta could be reached in this way, and satisfactorily ascertained it to be lying loose and unattached, notwithstanding *pain* had not been complained of by the patient, although frequently asked if she felt pain. From observations like these it may be safely concluded that the placenta is detached without pain—viz., without muscular contraction of the uterus; and the only other agent that can be operative is *tonic contraction*" (p. 408).

The nature of uterine muscular action, Dr. Miller observes, is expulsive, and implies the presence of something to be expelled; "but the placenta and membranes, so long as they are attached to the inner surface of the organ, are in bonds of vital union with it, and cannot in any sense be reckoned as extraneous matters" (p. 410).

Dr. Miller's account of the manner in which the placenta and membranes are expelled is as follows:—

"The separation begins with the placenta, and commences usually about its centre, extending gradually towards its margin. While this is going on more or less blood escapes from the denuded mouth

of the uterine vessels; and, by its pressure, forms the detached portion of the placenta into a cup-like cavity for its reception. When the attachment of the margin of the placenta is broken up, the entire mass falls by its gravity, or is pushed by uterine contraction to the external orifice of the womb. "The separation of the placenta sometimes takes place differently: its margin may be detached first."

In this case, the author observes, the placenta assumes a cylindrical form, and its uterine lobulated surface is presented to the finger, attended with more or less hæmorrhage, from want of the placental cup to detain and restrain it.

On the means employed to detach the placenta, Dr. Churchill remarks:—

"The contractions which expel the child may detach partially or wholly the placenta, or it may be unaffected by them: in this state it will of course remain until the recurrence of uterine action. But cases not unfrequently occur in which the uterus remains quiescent after expelling the child, owing sometimes to the length and severity of the labour, and sometimes apparently to a peculiarity of uterine constitution,—in other words, to a cause unknown" (p. 389).

This "cause unknown," we may be permitted to suggest, is the absence of the "tonic contraction" to which Dr. Miller, we think with justice, attributes the complete expulsion of the secundines, and without the occurrence of which great risk of hæmorrhage is incurred.

Dr. Miller regards the application of a second ligature to the umbilical cord as entirely superfluous. "It is now well known that whatever bleeding may occur (from the placental portion of the cord) proceeds from the foetal vessels in the placenta, and cannot, of course, affect the mother, while by depleting the placenta it only makes its expulsion easier." Dr. Churchill still enjoins the application of the second ligature. We concur in Dr. Miller's views. This is a point, however, of no great importance practically, although it affords some indication of the scientific character of the practitioner.

Both Dr. Miller and Dr. Churchill wisely counsel the speedy removal of the placenta: its remaining in the inert uterus can only be the source of evil. But the method of its removal differs in the hands of these authorities. Thus Dr. Miller employs traction on the cord: Dr. Churchill discounts

the practice, from the risk of breaking the cord, and of producing hæmorrhage by withdrawing the placenta whilst the uterus is relaxed. For our own part, the plan and *precautions* recommended by Dr. Miller are those upon which we have ourselves always acted, and have generally found efficient in inducing its expulsion by the tonic contraction of the uterus, when the muscular contraction has not detached it, as it seldom does. The hand placed on the abdomen has always served to indicate whether the uterus be so much relaxed that hæmorrhage would follow its removal. In such cases the hæmorrhage will probably have already occurred. The introduction of the hand may become indispensable; but, as Dr. Churchill truly remarks, it is "an operation not to be lightly undertaken, as it is one by no means free from danger." We would impress this caution upon junior practitioners; at the same time we would add Dr. Miller's rule, "to deliver the placenta at the expiration of an hour from the close of the second stage." Whatever may be the cause of the morbid retention, we believe that the best practitioners, both in America and in Great Britain, agree that it is not advisable to interfere sooner, nor safe to defer the delivery of the placenta beyond that time.

In the hæmorrhage which occurs after the expulsion of the child, Dr. Miller lays great stress upon the effect of the introduction of the hand into the uterus, in exciting that organ to contraction when other and external means have failed. Dr. Miller also testifies to the good effects of cold applied externally.

Dr. Churchill regards the introduction of the hand into the uterus as "so hazardous a practice that nothing would justify it but the failure of all previous means" (p. 413). We fully agree in Dr. Churchill's opinion, which is in no degree counter to that of Dr. Miller. Among the other means which we have found of much use in post-partum hæmorrhage is the injection of cold water into the uterus. We have also found the ergot of rye a valuable aid, and cannot, therefore, concur in Dr. Miller's utter disregard of this drug. We agree in Dr. Churchill's commendation thereof, *when judiciously used*. But at the same time, we have frequently seen retained placenta and hæmorrhage

from irregular contraction of the uterus, caused, we believe, solely by the too early use of ergot.

We regret much that our space forbids our further extension of the comparison of these works. We think, however, that the points we have already touched upon may be regarded as fair indications of the character of the principles and practice of the respective teachers. If our readers should feel disposed to follow our example, and make a comparison for themselves, we are sure they will derive much instruction from the investigation. Each author has produced a work of very great merit, and such as we can confidently recommend to the study of every obstetric practitioner. Both authors have ably represented their several schools; and, as the result of our comparison, we cannot say that one is in any respect behind the other; but we may observe that Dr. Miller's work bears a feature which is possessed by most American works on midwifery,—that of partaking very largely of the literature of the French school. The authors quoted by Dr. Miller, although Burnet and Hamilton are his models, as well as by Dr. Meigs, whose works we reviewed some time since, are for the most part French. We think it only fair to claim for Dr. Churchill, as the representative, for the occasion, of British midwifery, that his work displays greater self-reliance. He trusts more to his own facts and observations in the practical conclusions at which he arrives.

Wonders displayed by the Human Body in the Endurance of Injury. From the Portfolio of DELTA; with Etchings by the Author. Pamphlet, 8vo. pp. 39. London: Renshaw. 1850.

THIS is a chapter of accidents,—marvellous accidents,—including the most extraordinary and out-of-the-way instances of such phenomena as phosphorescence, spontaneous combustion, voluntary control over the action of the heart, the endurance of pain, protracted fasting, premature sepulture, the swallowing of spoons, forks, knives, &c. &c., the endurance of most severe injuries, the reception of foreign substances, such as instruments of various kinds, within the body.

This pamphlet presents a curious collection of wonders displayed by the human body in its endurance of injury.

Corpulence, or Excess of Fat in the Human Body; its Relations to Chemistry and Physiology; its Bearings on other Diseases and the Value of Human Life; and its Indications of Treatment: with an Appendix on Emaciation. By THOMAS KING CHAMBERS, D.M., Fellow of the Royal College of Physicians, and Gulstonian Lecturer for 1850. Small 8vo. pp. 166. London: Longman and Co. 1850.

We should scarcely have thought it possible that so interesting and instructive a volume could have been composed on the subject of "Corpulency" as we have before us in Dr. Chambers's work. The author treats this state in all the relations indicated on his title-page, so very fully and ably, as to excite surprise at the variety and extent of aspects under which it may be viewed.

The following extract points out the character and limitations of the morbid condition which constitutes the disease of which he treats:—

"Obesity, or polysarcia as it is called by systematic writers, must be viewed as a true hypertrophy of the *tela adiposa*,—an increased growth, without change in chemical or anatomical characters. I am the more anxious to give this definition, in order to draw a strong line of demarcation between such a state and that degeneration of muscle and other tissues into fat, to which the labours of Professor Rokitsansky have latterly drawn such deserved attention. He designates this degeneration as a *false* hypertrophy. 'False hypertrophies,' he observes, 'are recognisable at the first glance, by the alienation of the whole habit of the organ. . . . They appear as fatty disease of the liver, as albuminous, lardaceous infiltration of the same, of the spleen, of the kidneys . . . as a change of the muscular fibrils into molecular fat, with destruction of the transverse striae, and irregular distension of their sheath.' What he calls false hypertrophy is better classed by Mr. Paget as an atrophy; for in it, while the organ is altered in shape, and sometimes also in size, the interstitial nourishment of its tissue by the substance proper to it is certainly diminished. Indeed, this last state is so entirely different from an increased accumulation of fat, that the two may be pathologically contrasted; in that, the substance of some necessary part of the body is removed, and replaced by a matter foreign to it; but in obesity, the tissues of all the members remain intact, possess all the parts requisite to execute their offices, and are only impeded in the

performance of them when the additional superimposed matter alters the shape so far as to offer a mechanical obstacle, to free action." (p. 3.)

The author carefully draws a distinction between the two kinds of fat; molecular and vesicular. He gives a minute description of the anatomical characters of the fat vesicles. The mechanical and chemical uses of fat are also related. The origin of fat, its chemical and physiological history, are treated of *in extenso*.

Dr. Chambers has fully considered the value of the weight of fat as influencing the value of life in relation to insurance, has pointed out the variations observed in different ages, seasons, &c., with the predisposing causes of obesity, its anatomical characters, the mode in which it operates injuriously, and lastly its treatment.

In the discussion of all these considerations, many topics are embraced to which we have not been able to allude.

The reverse condition, that of *Emaciation*, is considered in the Appendix, in relation to the absorption of fat in health and disease, and the chemical theories which have been advanced to account for its assimilation. The author refers in this portion of his work to the emaciation of phthisis, and the fattening properties of Cod-Liver Oil, quoting the Report of the Brompton Hospital for testimony as to the powers of the last-named remedy.

We regret that we have been able but very imperfectly to represent the contents of this book: we would strongly advise our readers to possess themselves of the work itself, which they will find amply to repay the short time it will occupy in its perusal.

Vision in Health and Disease; the Value of Glasses for its Restoration, and the Mischiefs caused by their Abuse: being the substance of Lectures delivered at the Central London Ophthalmic Hospital. By ALFRED SMEE, F.R.S. Pamphlet, pp. 64. London.

THE nature of these lectures will be apparent from the title-page above quoted. The reason for their publication is stated by Mr. Smea to be the repeated solicitations by various persons that he would publish a Treatise

on Vision, &c., and the assurance that such a treatise would be acceptable to the public. To this he "consented upon the express condition that the book should be published at the lowest possible rate." The book thus resulting, and which has now been some time before the public, is, on the whole, a very good popular exposition of vision and its morbid states. It contains much interesting information; and we consider it to be one of Mr. Smees's best productions.

Proceedings of the Medical Society of the State of Pennsylvania, at its Annual Session held in the City of Philadelphia, April 1850. Pamphlet, 8vo. pp. 55.

THE proceedings of the Society are introduced by an appeal to the body of practitioners throughout the State in behalf of the Society, and calling for an organization of local Societies in communication with the central Society.

The business of the session consisted in the passing of various resolutions having for their objects the promotion of medical science and the improvement of the *status* of the profession.

A report was presented by a committee appointed to investigate the recent prevalence of small-pox. From a table which is given it appears that variola has prevailed epidemically in America at various intervals. The following years exhibited its greatest prevalence in Philadelphia:—1808-11, 1816-17, 1823-24, 1827-28, 1833-34, 1841-42, 1845-46, 1848-49, showing periods or intervals of from two to five years. The accompanying remarks on the protective influence of the disease itself, and of vaccination, are deserving of perusal, as tending to explain the discrepancies met with in the history of vaccination.

The volume concludes with an address to the profession on the importance of the organization thereof, argued from the analogy of nature and of society in all ages. We trust that the talent and the zeal displayed in the promotion of the objects of this Society will not be exerted in vain among our Pennsylvanian brethren.

Proceedings of Societies.

MEDICAL SOCIETY OF LONDON.

Oct. 15, 1850.

DR. J. R. BENNETT, PRESIDENT.

[THE first meeting of this Society took place at the new rooms, in George Street, Hanover Square, on Saturday, the 12th instant.]

Cases of Extra Capsular Fracture of the Thigh-bone. By Mr. COULSON.

The first case occurred in a man aged 77, who caught his foot in the carpet whilst crossing a room, and fell on the trochanter-major of the left side. Considerable effusion took place in the region of the joint; the limb was shortened two inches, and slightly everted. There was great pain about the joint, and involuntary twitchings of the muscles of the limb; and, at the end of a month from the accident (Aug. 11) he died, worn out by his sufferings. On examination, the bone was found to be broken at the neck, the fracture extending obliquely from without inwards, and involving a portion of the shaft of the femur; but leaving the trochanter minor untouched. Posteriorly there was another fracture, including the trochanter major, the inter-trochanteric space, and the trochanter-minor, with a portion of the bone beyond it.

The second case was that of a woman aged 76, who, on getting out of bed in the dark, fell down and struck her left hip. The foot was shortened about two inches, and everted, and the pain was considerable in the region of the joint on pressure or motion, but the pain was not great when the part was left quiet. The patient lived three months, and died more from bed-sores than from the irritation caused by the accident. On examination, the neck of the femur, as in the preceding case, was found broken obliquely from without inwards, the fracture enclosing a portion of the shaft of the bone. Posteriorly there was a comminuted fracture of the trochanter-major and inter-trochanteric space, extending to the trochanter-minor, but not including it.

Among the points of interest connected with these accidents, Mr. Coulson observed that in both cases there was a double fracture—a fracture through the neck, extending down the shaft of the bone, and a second through the inter-trochanteric space. Mr. Smith, in his valuable work on "Fractures in the Vicinity of Joints," in

alluding to this subject, remarks, that in upwards of a hundred cases of extra-capsular fracture of the neck of the femur, which he examined, he had found, and in all without a single exception, a second fracture, traversing some portion of the inter-trochanteric space. He considers that "the neck of the femur is, in the first instance, broken by the fall on the hip, and then driven into the cancellated tissue, between the trochanters, by the weight of the body, and the prolonged action of the first shock; but, as soon as the neck of the bone is broken, the femur is rotated outwards, even before the action of the first impulse has ceased: thus, the posterior inter-trochanteric ridge being thrown forwards, is forcibly driven against the back of the neck of the femur. Two forces, therefore, combine to produce the fracture through the inter-trochanteric space, one of which consists in the impaction of the cervix into the shaft, whilst the other is found in the collision which takes place between the broken neck of the bone and the posterior inter-trochanteric ridge."

Angina Membranacea, or Diphtheritis, and its Treatment. By Dr. BENNETT.

The peculiar form of pharyngeal inflammation which has been so ably investigated and well described by Bretonneau, and to which he gave the name of diphtheritis, is a disease comparatively little known in this country. Some of our best authors make no mention of it: many practitioners have never seen it; and, though we can by no means assent to the accuracy of the views taken by Bretonneau and other French writers, as to its identity, in certain forms or complications, with the ordinary true inflammatory croup of this country, yet those only who are practically acquainted with the disease as well as with its history, as detailed by the French writers, can judge of the grounds for inferring a relationship between the two. The disease has usually appeared as an epidemic rather than as a sporadic affection; and, like most other epidemic diseases, has doubtless varied somewhat in its aspects with variations of the epidemic constitution. Hence, also, the different names which there is good reason for believing have been given to the same disease: for, whilst by some it has been confounded with, and described as croup, it has by others (and this more frequently) been designated as angina maligna, or malignant sore-throat. At all events, the disease which Bretonneau described was, previous to his investigations, considered to be characterised by extensive and malignant ulceration of the throat as its most prominent feature; and there is sufficient evidence in the writings of our

own countrymen to prove that, under the designation of croup, or some analogous term, diseases have been described which primarily were diseases of the pharynx. But the investigations of Bretonneau demonstrated that, except in the rarest cases, the formidable epidemic angina which he described was unattended by sloughing, or even ulceration, of the mucous membrane. The comparative infrequency in this country of the disease to which Bretonneau gave the name of diphtheritis (but for which angina membranacea is a preferable term) may to a certain extent account for the paucity of any very accurate or precise information respecting it as a British disease. There are not wanting, however, accurate and distinct histories of the malady in the records of British medicine, and doubtless there are many medical men who have had practical acquaintance with it. The investigations and views of Bretonneau are, indeed, sufficiently well known among us. But, unless I am mistaken, there are comparatively few who can say that they have tested, by their own observation, the accuracy of the pathological views of Bretonneau.

The author then related several cases which had fallen under his notice in the course of the present year, and concluded as follows:—The first point to which I would call attention is the epidemic character of the disease. All the cases I have mentioned, and others of which I have heard, occurred about the same period of the year. With respect to the question of contagion, without venturing to express any decided opinion, I may observe that the first and the last cases mentioned occurred in families where there were other children, who were not separated: some of them took the disease. The French writers are pretty unanimous in attributing a contagious character to the disease. With the exception of the first case, all the children were apparently in good health when attacked; in all, the disease was of an adynamic type, languor and depression of system from the first characterizing all the cases. In all, the throat affection had attained a very serious and formidable character before there was either much difficulty of swallowing or external swelling, and before there was any difficulty of breathing, or other symptom referable to the larynx or lungs. There was nothing like preceding catarrh in any of the cases. Stomatitis prevailed at the same time, and my own observation would lead me to concur with most of the French pathologists in assigning a close relationship between this affection and diphtheritis, if not an absolute identity. But it may naturally be asked, does not the existence of

ulceration in the one case, and its absence in the other, indicate an important pathological distinction? To this it may be replied, that stomatitis is essentially characterized by the same fibrinous exudation, and that, although ulceration does undoubtedly frequently attend it, the great similarity of appearance in the two conditions often leads to the supposition that there is more ulceration than actually exists in ordinary cases of stomatitis. I have frequently had occasion to observe, that on the separation of the ash-coloured patches, around which the mucous membrane was red and tumid, so as to give them the depressed form, as well as colour, of ulcers, the mucous membrane was still entire. It must, however, be admitted that, whilst it is only rarely that ulceration attends the disease, when confined to the fauces and soft palate, as a general rule, when assuming the form of stomatitis, there is more or less of ulceration associated with the fibrinous exudations. This may perhaps depend on the difference of situation, the inflamed membrane in the one case being more exposed to mechanical and other sources of irritation. As to the general or constitutional symptoms, there is the closest resemblance; and I believe that the same general plan of treatment will be found applicable to both sets of cases. The fact, also, of the two affections having very generally been found to prevail at the same time, renders it highly probable that they are the result of the same general cause. This question of relationship with the more common affection (in this country at least) of stomatitis is, I think, important as regards treatment. For many years I have now been in the habit of prescribing the *chlorate of potash* in the stomatitis of children, and with such marked advantage that I cannot doubt its exerting a peculiar and specific influence. Of its great superiority over the more ordinary plan of giving mercurial alteratives I have myself no doubt. Mercury has appeared to me to be of very questionable utility in most cases, and in others to be positively prejudicial. The author then proceeds to say that he regards the type of the disease as essentially adynamic, and that the treatment must always have regard to this fact. With respect to treatment, he lays great stress on the value of the application of the solution of nitrate of silver to the affected parts, and cautions the practitioner to be constantly on his guard, and not to be taken by surprise by the sudden invasion of croupy symptoms. The general treatment must be tonic and supporting.

I have been led to look on the disease as the result of a specific epidemic influence, operating on the system through the blood,

characterized by an adynamic type, and calling for special treatment; and that, however much the general symptoms may be modified by the occurrence of laryngeal complication, the secondary croup which then sets in is a distinct affection from the common sporadic croup of this country, and not amenable to the same treatment, whether in the simple or complicated forms, is of the utmost importance; that the more common form of stomatitis is essentially the same affection as that which attacks the pharynx. As, however, the term stomatitis, like croup, has been applied in the most confused way to various totally distinct diseases, I ought to say that, in using this term, I restrict its application to the buccal affection chiefly seen in children, and characterized by the effusion of fibrine and spreading superficial inflammation, and excluding, on the one hand, the more trivial cases of aphthae and infantile thrush, and, on the other, the still more formidable cancrum oris, or sloughing inflammation of the cheek. I have made no allusion to diphtheritis, as affecting the adult, because I have never seen a well marked instance; but I have reason to believe that several cases occurred last summer; and the well-known paper of Louis, on croup in the adult, describes, in fact, cases of angina membranacea, complicated by extension of the disease to the larynx.

ACADEMY OF MEDICINE, PARIS.

Oct. 8, 1850.

Vaccination—its protective power depending on the manner of its performance.

M. DELAFRAYSSE stated in a note, that he had arrived at certain conclusions as the result of his investigations into the causes of the failure in the protective power of vaccination against small-pox. His experience had convinced him that vaccination is an infallible preventive where the vesicles are sufficiently numerous to produce a degree of febrile reaction, and that its failure is to be attributed to the insufficient development of this febrile reaction, or to its non-occurrence. He proposes that, instead of the four or five punctures usually made on the arm, from twenty to thirty should be practised on different parts of the body. In confirmation of his views, M. Delafraysse states that he has with impunity exposed all the children that he has so vaccinated to the greatest risk of contracting small-pox.

Functions of the Bulb of the Urethra.

M. GUÉRIN (of Vannes) read a memoir on this subject, in which he stated that, from his observations and experiments, he had arrived at the conclusion that the bulb must be regarded as an appendix to the canal of the urethra, its function being to assist in the expulsion of the last drops of urine by the afflux of blood from behind, propelled by the contraction of the bulbocavernous muscle, thereby producing a modified state of congestion of the cavernous and spongy structures of the penis and urethra.

This discovery M. Guérin considers sufficient to explain the turgidity of the penis in cases where ineffectual efforts at urination are made; also the fact of the retention of a few drops of urine in the urethra in cases of chronic inflammatory stricture, the altered mucous membrane preventing the due propulsion of the blood towards the gland during the contraction of the muscle.

ACADEMY OF SCIENCES, PARIS.

Oct. 7, 1850.

Diseases of Persons employed in the Manufacture of Quinine.

M. CHEVALLIER stated the following results of his investigations on this subject:—

1. The workmen are attacked by a cutaneous disease which compels them to suspend their labours for fifteen days, a month, or more.
 2. Some workmen are compelled entirely to abandon their occupation.
 3. Herr Zimmer, of Frankfort, has observed that the workmen employed in the pulverisation of quinine have been subject to a peculiar febrile attack, to which they give the name of "quinine fever" (*china fieber*). This malady is so painful that many workmen quit their employment on that account.
 4. This disease has not been noticed in France.
 5. The proper prophylactic measures are not known.
 6. The cutaneous disease is found not only to attack those actually engaged in the manufacture of quinine, but to extend also to those exposed to the emanations.
 7. It attacks the temperate equally with the intemperate.
 8. Its predisposing causes are unknown.
- M. Chevallier suggests as precautions, free ventilation and the avoidance of contact with the decoctions, &c.

Correspondence.

THE MEDICAL PROFESSION AND ASSURANCE OFFICES.

SIR,—I have a suggestion to offer to my brethren, touching upon life-assurance certificates, founded on the following fact:—

Some time since I received a printed form, signed by a patient of mine, requesting me to answer certain questions respecting his life to the Standard Assurance Office. The paper of course came from the Office, the whole of it being filled up by the agent in this place, whose handwriting I knew.

I replied to the agent, that on his assurance that I should be paid a fee I would afford all the information in my power, but not otherwise. Upon this that gentleman called on me, and endeavoured to convince me that it was for my patient's benefit, and not for the benefit of the Office, that the information was sought, and that, as his medical attendant, I was bound to give it. I replied, that when my patient employed me he always paid me, and that now, far from wishing to employ me, he was willing to pay his insurance bonus without troubling me, and thus to fulfil his part of the contract; and that, as the Office, for their own protection, applied to me, by the Office I would be paid, or else withhold my information, and that I would not be paid by the patient if he wished it.

Before writing to the agent I saw my patient, and explained to him how matters stood. He at once, as a man of business, saw the justice of my demand on the Office, and promised to back me in it. I further told him that the office in which my own life was assured would pay me, and that it was an office of equal respectability with the Standard.

I must further say that, in going to call on my patient on the occasion mentioned, I met the principal clerk of the attorney (a highly respectable practitioner), who is the agent here of my office. I said to him, "Mr. —, does your office pay the medical attendant of the persons assuring?" He replied, "We pay our own referee." "But do you pay the medical attendant of the assurer?"—"No!" "Oh, well, then, I must recommend my patients to assure in an office that does, for I am determined to fight this battle with the assurance offices." He replied, "Well, Mr. H., we can arrange that." I told him how I was placed, and that if the Standard refused my demand I would bring my patient to their office. To cut a long story short, after some little delay the agent of the Standard called and said they would pay me my fee, which they

did; and, after all was settled, the agent said that they should be glad to act in the same way again if I would recommend their office.

Thus you see that the best offices will pay when "*proper pressure*" is put on them; and that the *profession have themselves to blame if they do not obtain their just demands*. I am also, from pretty considerable experience, justified in saying that the public generally are fully prepared to back the just demands of their professional advisers when the matter is explained to them.

I was much pleased to see the list you lately published of the offices which pay; and I think much good would be done if you and your cotemporaries were to publish occasionally—say in the first number of every quarter—a list of the offices which do recognise the claims of the profession. We should then be able at once, at any time, to recommend our patients to these offices when called on to do so, and at the same time a salutary stimulus would be given to the non-paying offices. My suggestion, then, is, in all cases of assurance stand firm, and you will obtain your demand.—Your obedient servant,

J. H. HOUGHTON.

Dudley, Oct. 18, 1830.

N.B. A brother practitioner has informed me that he has obtained a like result from the same office by the same proceedings.

Medical Intelligence.

THE CHOLERA AT ALEXANDRIA.

INTELLIGENCE from Alexandria, dated October 9th, states that the cholera, after having entirely disappeared from the country for many days, had again broken out on the 8th instant, and four cases were reported. It is attributed, however, to the excessively oppressive weather, and the first shower of rain will, it is expected, restore the town to its usual good health. No rain has fallen there since March, and the first shower is anxiously looked for to cool the air.

THE CHOLERA AT HAVANNAH.

PRIVATE letters from Havannah dated the 22d ult. state that the cholera had broken out on the south side of the island, where it had not appeared before, and was attended with considerable mortality amongst the slave population. It had also made its appearance in the centre of Cuba, where the negro population was numerous.

EFFECT OF HEAT IN EXPANDING METALS.

THE Britannia tubular bridge has afforded by its great length some curious results in reference to the expansion of iron by increase of temperature. An increase of 26°, namely from 32° to 58°, gives an increase of length of *three and a quarter inches* in the whole bridge, which at 32° is 1510 feet one inch and a half long. The expansion is thus one-eighth of an inch for each degree, or 1-14,500th part of the whole length. The daily expansion and contraction of the tube varies from half an inch to three inches, or 1-14,500th part of the whole length. When the sun is shining on one side and the other side is cool, the heated portions expand, and thereby warp or bend the tube towards the heated side, the motion being as much as two and a-half inches vertically, and two and a half inches laterally. Clouds passing before the sun affect the rate of expansion by rapid cooling, and singular curves of heat are thus described by self-acting clock work. Even in the dullest and most rainy weather the tube rises slightly, showing that heat as well as light is radiated through the clouds. On very hot sunny days the lateral motion has been as much as *three inches*, which is considerable, when it is remembered that the deflection of the tube produced by a heavy train is only three-tenths of an inch, and that a violent gale does not bend it more than a *quarter* of an inch.

A LUNATIC BALL AT ST. LUKE'S HOSPITAL.

ON Friday last, the 18th inst., being St. Luke's day, the entertainment of dancing and singing was afforded to a large number of the unhappy objects of this charity under circumstances of peculiar interest. It was the first occasion of this kind which had taken place at St. Luke's, and from the successful issue of the undertaking it may be expected to be repeated. It appears that under the judicious management of the resident officers, acting with the sanction of the house committee, and upon the suggestion of the physicians, between 60 and 70 of the patients, men and women, were assembled at 6 o'clock in the evening in the great hall in the centre of the building, and music being provided, they were quickly arranged in their places for an old English country dance. This, with appropriate changes, was kept up with great spirit until 9 o'clock, when all retired peaceably and cheerfully to their respective apartments. The officers of the establishment, with such nurses and attendants as could be spared from the galleries, joined merrily in the dance. By this means the confidence of the patients was secured, and they showed themselves grateful for the enjoyment afforded them, by entering into the amusement

without levity or boisterous conduct of any kind. The vigilant eye of the matron rested anxiously upon the women, many of whom had been but recently admitted in an apparently hopeless state of lunacy, and the excitement was upon the whole the more important in the case of this hospital, as it is the practice to receive within its walls the most violent cases of acute mania, and to retain them only until cured; or until, after a twelvemonth's trial, the disease appears beyond the relief of human skill. There are at present 196 patients in the hospital—91 men and 105 women. The average of cures during the last year was 60½ per cent, being a considerable improvement upon former years.

REMUNERATING AND NON-REMUNERATING INSURANCE OFFICES.

WE had intended to publish a list of the Metropolitan Insurance Offices which do not remunerate Medical Referees for their certificates, but we find the number of non-remunerating offices so large, that we have no room for the list.

The metropolitan offices will be found at page 89 of the British Almanack, for 1850. Out of a list of 125, there are only 32 which profess to pay medical referees for their certificates. The 93 which do not pay, but (when they can get them) procure medical certificates and act upon them without a fee, comprise, unfortunately, the oldest, largest, and most influential offices, such as the Sun, the Book, Eagle, Atlas, and the London Assurance Corporations. Medical men, however, have one clear and distinct rule to follow—i. e., to sign no certificate without a fee, and to inform their patients that this rule has been now generally adopted by the profession.

UNIVERSITY OF CAMBRIDGE. LECTURES ON COMPARATIVE ANATOMY AND PHYSIOLOGY.

THE Professor of Anatomy commenced a course of lectures on comparative anatomy and physiology in the Anatomical School on Wednesday, October 28, at one o'clock. The lectures, to be delivered on Mondays, Wednesdays, and Fridays, at one o'clock, will be twenty-four for the present term. The course will commence with a review, illustrated by diagrams and preparations, of the anatomy and physiology of man, as the term of comparison, and will then proceed to the anatomy and physiology of animals, according to the ascending series. Gentlemen who purpose to attend these lectures with the intention of obtaining the Professor's certificate, which, by grace of the Senate (October 31, 1848), is now required, under certain conditions, for the degrees of B.A., honorary M.A., and B.C.L., must be provided with the professorial

lecture ticket, for which they are requested to apply to the registrar, or to their college tutor.

HARVEIAN SOCIETY.

THE following gentlemen have been elected office-bearers for the present session.

Presidents.—Dr. Chowne, and H. Burford Norman, Esq., F.R.C.S.

Council.—Dr. Nathaniel Grant; Dr. Joseph Ridge; Edgar Barker, Esq., F.R.C.S.

Dr. Stroud, Treasurer, 20, Great Coram Street, Brunswick Square; and Dr. R. Hutchinson Powell, Honorary Secretary, 21, Edward Street, Portman Square, *ex officio* members of Council.

APOTHECARIES' HALL.

NAMES of gentlemen who passed their examination in the science and practice of medicine, and received certificates to practise, on Thursday, 17th October, 1850:—Henry Vincent Garman, Bow Road—George Peat Dunn, Ledbury, Herefordshire—William Hewitt, Reading, Berks—Joshua Hartley, Malton, Yorkshire—Chas. Caulfield Moore, Brimsfield, Gloucestershire.

OBITUARY.

On the 14th inst., aged 58, Mr. William Bates, Surgeon, of George Street, Blackfriars Road.

DR. FOUQUIER.

THE Faculty of Medicine of Paris has just suffered a loss, by the death of Dr. Fouquier, Professor of Clinical Medicine to the Faculty. He had reached the age of seventy-four, and was universally esteemed and respected. Dr. Fouquier had been the favourite physician of Louis Philippe for a considerable period, and commanded a very large practice. It is generally allowed that he fully deserved the distinguished rank he held in the profession, and he was particularly beloved by his patients. He has not left any medical works of note: a few therapeutical essays, and a translation of Brown's book, as well as a share in one of Celsus's, comprise the whole of his literary remains.

Selections from Journals.

ON THE CAUSES OF DEATH IN NEW-BORN INFANTS.

DR. CASPER, in his series of "Inspections of the Dead for Judicial Purposes," a former portion of which will be found at p. 218 of the eighth volume of the present series of the MEDICAL GAZETTE, and of which a succeeding portion appeared

in Casper's *Wochenschrift*, Nos. 47 and 48, 1849;—has recently given the results of forty-six post-mortem examinations made for judicial purposes on the bodies of infants.

These notices are prefaced by a few preliminary observations in reference, 1st, to the indications afforded as to the maturity of a fetus: 2nd, on the hydrostatic test; 3rd, on stelectasis.

1st. Dr. Casper states that from the observations which he has made, he would lay down a rule, which he is not aware has been stated before, that the mass of the body of fetuses and new-born children furnishes a surer indication of the degree of maturity, than does their absolute weight.

2nd. With reference to the hydrostatic test, Dr. Casper combats Henke's objections, which, he considers, have only tended to introduce uncertainty on this part of the subject. The hydrostatic test Dr. Casper considers as affording in every degree trustworthy conclusions, if performed by careful and competent hands. With regard to the objection that emphysema of the lungs would cause them to float, Dr. Casper fairly inquires whether it has ever been met with in new-born infants. In answer, also, to the statement that artificial inflation of the lungs might prove a source of fallacy, Dr. Casper also asks, Whether a mother anxious to conceal a birth would have recourse to the means of resuscitation? and doubts that this mode, which requires an anatomical and physiological education, would be known to, or could be practised by, a woman under the circumstances. Emphysema from putrefaction Dr. Casper excludes from all medico-legal considerations, by the fact that the lungs are among the last to undergo putrefaction, or that, when it has occurred, it is so evident that no mistake can occur. Dr. Casper mentions a case in which the heart being putrid, the lungs nevertheless sank.

3rd. Dr. Casper considers that pneumonia has often been mistaken for the so-called stelectasis, and gives a case in illustration, in which pneumonia was found in the lungs of a child eight days old, and those organs presented all the characters of stelectasis.

The present series of inspections illustrates chiefly the value of the hydrostatic test, and its application where death in new-born infants was shown to have occurred from apoplexy, pneumonia, &c.

Dr. Casper then proceeds with the description of his cases.

A Case in which Death occurred to an Infant from falling on its Head in the act of Birth.

This case possessed a twofold interest.

An unmarried female having arrived at the full period of her gestation, attended her master to market on Christmas-eve, and carrying a heavy basket, was unexpectedly taken in labour. The child, she said, was suddenly expelled when she had only felt the pains for half an hour, and it fell on its head on the foot-path. The umbilical cord was in this way torn asunder, and its ruptured extremities confirmed her story. The mother declared that she then fainted, and on coming to herself found the child lying dead beside her.

The hydrostatic test, and the autopsy, proved that the child had lived, and had died of effusion of blood within the cranium: about a drachm of blood was found extravasated at the base of the brain.

A deficiency in the ossification of the right parietal bone was found, in which was a portion, the size of a half florin, thin and transparent; and this presented an indented fissure one line in width. This was attributed by a physician who was present to the fall, or some other force applied at the part: but Dr. Casper points out that there was no corresponding sugillation, besides that the fissure itself, and the thinned bone around it, presented no appearance of injury.

It was considered that the child was born at the full time; that it had died of extravasation of blood on the brain; and that this extravasation was attributable to the accident attendant on its birth.

Apoplexy without Evident Cause.

A woman was delivered in a state of unconsciousness: on coming to herself, she found the child lying dead between her thighs. She divided the cord with a pair of scissors. The state of the cord bore out her assertion. Apoplectic extravasation was found within the cranium. But in such cases the statements of the accused must be received with much caution; for although it is admitted that a woman may give birth to a child under the circumstances indicated, this may be alleged to hide crime or carelessness.

Death from Pneumonia.

An infant, four days old, died without having manifested any signs of, or without any apparent cause of the inflammation. Red hepatization was found in one portion; this sank in water. The remainder of the lung floated.

Anencephalus.

The cerebellum hung in a bag of dura mater in the situation of the occipital bone. A portion of cerebral substance was contained in an abnormal cavity formed by an expansion of the two first vertebrae. The imperfect head was placed on the shoulders,

The integuments of the chin were united to those of the breast. Spina bifida of the entire vertebral canal existed.

Infanticide?

— K. was charged with having concealed the birth of a child on a certain day. This she denied when questioned by an experienced midwife, notwithstanding the discovery of a fresh afterbirth on a board in her room, and behind her bed, rolled up in an apron, and smeared with blood and dirt, the body of a child, warm, but lifeless. It was clear that she had given birth to the child at the foot of the bed, where a quantity of blood was found. A pair of scissors, smeared with blood, were found on the window seat, and under the bed a bloody cotton cord.

The girl afterwards declared that she had become insensible from the severity of the pains, and that she was delivered of the child in that state. With regard to the division of the umbilical cord she professed utter ignorance.

On examining the body of the infant, there was found a mark round the neck about two lines in width, of a lighter colour than the surrounding integuments. Besides this, the important points which were observed were congestion of the liver, emptiness of the bladder, fulness of the large intestines, the abdominal veins loaded with dark red fluid blood. The lungs, without the heart, weighed about two ounces and a half; they floated readily in water; when cut, crepitated, and on pressure yielded a sanguineous frothy mucus. The cavities on the right side of the heart were empty; on the left side they were filled with blood. The cranial bones were loaded with blood. The membranes and vessels of the brain were also greatly congested. Thence it was concluded that the child was born alive at the full time, and had died of apoplexy, and that the cotton cord was tied round its neck after death. The diagnostic marks of strangulation were wanting.

Two other cases afforded evidence in reference to the value of the hydrostatic test. The first was a female infant found in the water. The body was already partially decomposed. The lungs were of a dark brown colour, but sank in water.

In the second, a male child found in water, the body was putrefied and emphysematous. The diaphragm rose to the fourth ribs; the pericardium was not covered by the lungs, which were of a dark brown colour, and of the consistence of liver, and sank readily in water. In both cases it was concluded that the children had not been born alive.

A female new-born infant was found

in the water. It was mature, had lived, was apoplectic, and had not died by suffocation. Other circumstances attending the finding of the bodies confirmed this opinion.

The body of a male infant was found on a dunghill. The rats had partly devoured it, and only a portion of lung remained. From this it was possible to determine that the child had lived. The lung was crepitant, and floated. The brain showed the appearance of apoplexy. The abdomen had been pierced by a dung fork.

The body of an infant was found in the street. It had never breathed. The bladder was empty. What is the value of the latter circumstance, Dr. Casper inquires?

A child was delivered footling; the head was arrested in the pelvis. When assistance arrived the child was already dead. The hydrostatic test, showed it to have been still-born. Dissection discovered, upwards, of an ounce of extravasated blood at the base of the brain.

The body of a female foetus was found in the water. The head and neck severed from the trunk. The body and limbs presented marks of immaturity. From the mutilation to which the body had been subjected it could not be determined whether the child had been born alive.

Two other cases showed early decomposition in the lungs. One, a female child that must have lived, was found in the water, and had died of apoplexy. There were no signs of suffocation. The tongue protruded beyond the teeth; but Dr. Casper has so often found this under other circumstances than suffocation, that he does not regard it as conclusive. In the other also there was every sign that the child had lived. In both putrefaction was but little advanced, but the lungs presented vesicles on their surfaces.—*Casper's Wochenschrift.* x

MECHANICAL OBSTRUCTION IN THE COLON OF RATHER A SINGULAR NATURE. BY DR. GRIEBS, OF BERKS COUNTY, PENNSYLVANIA.

In July 1833, I was called, two miles from my home, to see R. L., a little girl about eight years of age. She had been sick several days, and under family treatment. I found her suffering severely from pain in her abdomen, frequent efforts to vomit, and a good deal of fever. Her bowels had not been moved for several days. I administered an enema, used means to appease her stomach and to subdue pain and fever. Afterwards I gave her several doses of calomel, but when I followed it up with other purgatives, the vomiting and pain returned. I used frequent mild enemata. At first some little faeces came, but soon nothing but the injection. Upon close examination of the abdomen, and from other attending

signs, I felt convinced that there was mechanical obstruction, which the peristaltic action of the bowels could not overcome. I accordingly threw into her bowels, at last, three quarts of warm water, with a little soap, in a continued but gentle stream, by means of Maw's stomach-pump; she screamed out that we were "bursting her." Immediately on evacuating her bowels she passed large masses of dry or hard feces, in which were entangled nine large worms. I then hoped that the obstruction was overcome, and gave a few small doses of cathartic medicine to be taken till next day: but, on my visit, finding the medicine had no effect, I repeated the enema, in the same quantity, which brought away again a mass of hard feces and seven large worms. After this I had no difficulty with the case, except that she was slightly paralytic in her left leg. I have not seen her for many years; but have lately heard she is married, and has several children; also that she has since had disease of the hip-joint, and is quite lame. I think that it is not out of place here to mention a circumstance in another case of obstinate constipation. After being completely foiled in all my efforts, I determined to use tobacco injections, and upon due reflection I concluded that the smoke might suit best. I accordingly put a lighted cigar into the lateral tube of Maw's stomach-pump, and found it, thus prepared, the very best apparatus to generate and apply the smoke that could be possibly imagined. It had the desired effect.—*American Journal of the Medical Sciences*, July 1850.

BOOKS & PERIODICALS RECEIVED

DURING THE WEEK.

Remarks on Insanity: its Nature and Treatment. By Henry Monro, M.B. Oxon. Part I.

Observations on the Treatment of Phthisis Pulmonalis. By J. H. Bennett, M.D. F.R.S.E. &c.

Lectures on Clinical Midwifery. By J. H. Bennett, M.D. F.R.S.E. &c. Nos. 1, 2.

General Pathology. By John Simon, F.R.S.L.

On the Identity or Non-Identity of the Specific Causes of Typhoid, Typhus, and Relapsing Fever. By William Jenner, M.D. &c. (From the *Med.-Chirurgical Transactions*, vol. 38.)

Instruction in Chemical Analysis. Qualitative. By J. Lloyd Bullock, Fellow of the Chemical Society, &c. 8d Edition.

Casper's *Wochenschrift für die gesammte*

Heilkunde. Nos. 35 to 38; 31 August to 21 September.

Henke's *Zeitschrift für die Staatsarzneikunde*. 3tes Vierteljahrheft, 1850.

BIRTHS & DEATHS IN THE METROPOLIS

During the Week ending Saturday, Oct. 19.

BIRTHS.		DEATHS.	
Males....	748	Males....	480
Females..	764	Females..	431
1802		860	

CAUSES OF DEATH.

ALL CAUSES	860
SPECIFIED CAUSES	846
1. Zymotic (or Epidemic, Endemic, Contagious) Diseases....	198
<i>Sporadic Diseases, viz.—</i>	
1. Dropsy, Cancer, &c.	31
2. Brain, Spinal Marrow, Nerves, and Senses	99
4. Heart and Bloodvessels.	34
5. Lungs and organs of Respiration ..	189
6. Stomach, Liver, &c.	53
7. Diseases of the Kidneys, &c.	3
8. Childbirth, Diseases of Uterus, &c. ..	7
9. Rheumatism, Diseases of Bones, Joints, &c.	9
10. Skin	0
11. Premature Birth	18
12. Old Age	57
13. Sudden Deaths	7
14. Violence, Privation, Cold, &c.	35

The following is a selection of the numbers of Deaths from the most important special causes:

Small-pox	5	Convulsions	28
Measles	16	Bronchitis	4
Scarlatina	38	Pneumonia	58
Whooping-cough	28	Phthisis	130
Diarrhoea	55	Lungs	9
Cholera	4	Teething	5
Typhus	84	Stomach	3
Dropsy	19	Liver	11
Hydrocephalus	13	Childbirth	4
Apoplexy	31	Uterus	8
Paralysis	15		

REMARKS.—The total number of deaths was 48 below the average mortality of the 42d week of ten previous years.

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer 29.94
 " " Thermometer* 49°
 Self-registering do. b Max. 60° Min. 27°
 * From 12 observations daily. ° Sun.

RAINF, in inches, 0.0.—Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was about the mean of the month.

NOTICES TO CORRESPONDENTS.

The *Carlisle Journal* has reached us. The article referred to shall receive our attention. The communications of Mr. Barlow, Dr. Snow, Mr. Baiman, and Mr. Oke Clark, will be inserted in the following number.

Seco.—We know of no place in London where our correspondent can be accommodated with that which he desires.

Lectures.

CLINICAL LECTURE

ON

A CASE OF DISEASE OF THE
BRAIN,

(Delivered at King's College Hospital),

By R. B. TODD, M.D. F.R.S.

Physician to the Hospital.

(Reported by H. H. SALTER, B.A., Dem. Anat.
K.C.L.)

LECTURE XII.

I PROPOSE to call your attention, gentlemen, to two cases of brain disease that have recently terminated fatally in the hospital, which fatal termination gives us an opportunity of ascertaining positively what were the diseased conditions before death. A fatal termination to cases of this kind, although casting opprobrium on our art, yet affords us this consolation, that the post-mortem inspection clears away that amount of uncertainty which overhangs most cases of brain disease in consequence of the various complicating sympathies that accompany disease of the central organ of the nervous system. In many cases of cerebral affection, while it is sufficiently easy to determine the nature of the disease, it is very difficult to decide upon its locality. It is only, therefore, by a careful observation of cases during life, and an unprejudiced comparison of the post-mortem changes with the symptoms, that we can arrive at accurate conclusions respecting the precise value of certain symptoms, or the exact interpretation of them. A clot in one hemisphere of the brain, encroaching more or less upon the corpus striatum, will produce symptoms exactly the same as those of a similar clot deep in the substance of the corresponding cerebellar hemisphere; and it is only by the observation and careful collation of numerous cases, made complete by their post-mortem examinations, that we can expect to arrive at such conclusions as may hereafter enable us to distinguish a paralyzing clot situated in the cerebrum from a similar one situated in the cerebellum. It is curious that in heart disease the main difficulty is not as to the situation of the lesion, but as to its precise nature. We can easily enough tell whether this or that valve is diseased, or this or that ventricle dilated; but we cannot always be so exact as regards the precise nature of the lesion. On the

whole, however, we have attained in heart cases a certainty of diagnosis far exceeding that in brain diseases. This we owe to post-mortem investigation, and, perhaps, also to a more intimate knowledge of the mechanism of the organ. In a large majority of heart cases, then, we find that post-mortem examinations are *corroborative*; but in brain cases they often give us the only clear information—we possess, certainly as regards the site of the lesion—and sometimes entirely contradict our anticipations.

The first case, that of William Ware (vol. xxvi.), a ploughman from Kent, who has been in the hospital some time. He unfortunately caught erysipelas, and died of it; and it is owing to that circumstance that we have been able to see what were the causes of his symptoms. He was 30 years of age, and of temperate habits. About fifteen weeks before his admission, he was suddenly seized with paralysis of the right side of his body and face. He does not seem to have lost his consciousness at first, but awoke out of his sleep in a state of paralysis. After this, according to his own account, he lost his consciousness, and continued in that state for three weeks; but it is not probable that this unconscious condition was complete coma, as three weeks' coma is very unusual. He was probably in a heavy sort of stupor, unable, as he says, to speak distinctly, or to comprehend perfectly what was said to him, or to feed himself, and deprived totally of the power of the right side.

It is always important to see what state the patient was in before the attack: in most cases you will find something wrong. Our patient complained of a numbness of the extremities of the affected side three or four days before the attack, no doubt dependent on some morbid change going on in the brain. After the recovery of his consciousness, he continued for three weeks in a state of complete paralysis: he used to gape very much, and frequently cried in a childish manner, as persons thus afflicted are very apt to do. At the end of the three weeks he began to recover the palsy, the leg first regaining power, then the arm; but as they had been stationary for some time, he was sent up to town.

On admission, our patient exhibited the following symptoms:—There was hemiplegic paralysis of the right side, no longer, however, complete; for he could use the leg very well, and the arm slightly. He could elevate the arm to a right angle with the trunk, and bend the elbow-joint and flex the fingers so as to grasp feebly. In protruding the tongue it deviated, as is usual, to the paralysed side; but he could move it from side to side. The face was

still slightly distorted, the cheek hanging on the right side: none of this distortion, however, was due to paralysis of the facial nerve; for all the movements of the superficial muscles were perfect: he could wink, and frown, and whistle; and when he laughed, the distortion of his face was not nearly so great as you ordinarily find it when the portio dura nerve is affected. The facial palsy was due to paralysis of the buccal nerve of the fifth pair affecting the buccinator muscle. The sensibility of the affected side was but slightly impaired. The muscles of the palsied limbs were as usual somewhat wasted: they were, however, evidently gradually recovering their state of tone, and they were free from rigidity. Galvanism had been used several times, more experimentally than curatively, and he was encouraged to take as much exercise as he could without fatigue. With the view of reducing any inflammatory process which might be going on at the seat of cerebral lesion, we gave him mercury; and while he was in a state of salivation he was exposed to the contagion of erysipelas, and was seized with that disease in a very severe form, and died.

The symptoms justify a diagnosis with regard to locality. As the paralysis was very complete, and as the motor power only was affected, it seemed in the highest degree probable that the lesion of the brain was situated in some part which exercises an important influence on voluntary motion. Now there are the best reasons for believing that no part is so intimately connected with this function as the corpus striatum. In a case, then, in which the paralysis of motion is complete, without there being any considerable paralysis of sensation, the lesion would most probably be seated in the corpus striatum, or in that part of the hemisphere which is most intimately connected with it; and of the two corpora striata we would fix on that of the left side, since the paralysis is shown on the opposite side to that in which the cerebral disease exists; and here the paralysis was on the right side.

Taking, then, this *locality* as granted, what was the nature of the disease? It might either be occasioned by a clot of blood from a sudden rupture of a blood-vessel or apoplexy, or a more slow disease, which, however, was of such a nature as to be capable of producing sudden paralysis. The suddenness of the attack, and the succeeding loss of consciousness, favour the former supposition. But it is wrong to suppose that apoplexy can alone produce this sudden paralysis (by apoplexy I mean the rupture of a blood-vessel, and the consequent escape of blood into the brain)—there may be other causes. A portion of

the brain may be undergoing a gradual process of softening for some time, and yet its continuity may be preserved, and its constitution may not be so altered as to prevent its carrying on its function as a conductor—in an impaired way, perhaps, but not sufficiently so to call attention to it. Suddenly, from some temporary stress, perhaps, put upon the parts, the fibres give way, and all power of voluntary motion is lost, as suddenly as the galvanic current ceases on breaking the circuit. This is one cause of paralysis in cases where there is no apoplexy.

But it is very seldom that such a lesion exists alone. It is, however, very frequently—I incline, indeed, to believe almost always—the precursor of apoplexy; and therefore we frequently find in these patches of white softening one or more clots of blood of various sizes. The artery or arteries leading to the part are diseased; that portion of the brain fails in its nutrition; it passes into the state of white softening; and the minute vessels, losing the support which they must receive from the firm brain-texture, and being themselves perhaps more or less diseased, give way, and allow the blood to escape into the tissue of the brain.

There is another way in which paralysis may take place—viz., by an inflammatory state of the brain-substance, which produces softening, although of a different kind to that which I have just described, in which there may or may not be rupture of fibres; but there may be deposit as the result of the inflammation, which, by its pressure, may interfere with the conducting power of the fibres.

Now I have mentioned the opposite conditions of *white* and *red* softening. These are apt to come on under opposite circumstances. *White* softening may be caused by anything that diminishes or cuts off the supply of blood to the head. In some cases, in which the carotid artery has been tied for aneurism, and the supply of blood to the brain on that side thus stopped, the patient may go on very well for a short time, and there may be no impairment of function; but during that time the process of softening has been gradually going on on that side of the brain which is supplied by that carotid; and in a day or two the softened fibres cease to maintain their continuity, and give way, and paralysis ensues.

Some years ago I attended, along with my friend Mr. Street, of Norwood, a very remarkable case of this sort. The supply of blood had been cut off by a dissecting aneurism, which had plugged up the common carotid artery on the right side, and paralysis of the left side of the body took

place. We were much puzzled to account for the paralysis till after death, when the post-mortem examination cleared it up. The account of this case was published in one of the volumes of the *Medico-Chirurgical Transactions*.

There is abundant evidence to show that under ordinary circumstances white softening is *atrophic*—i. e., dependent on imperfect nourishment of the brain, and non-inflammatory. But it may exist on the confines of genuine inflammation, the nutrient fluid being diverted from it to the inflamed portion, or it may exist around an effusion of liquid in the ventricles, that effusion being possibly inflammatory in its nature.

The second sort, the *red softening*, is inflammatory. I show you here a very good representation of it, which was made from a case of mine some time since. The portion of the brain affected was of considerable extent, there was paralysis, of course, on the opposite side, there was a stupid, comatose condition, not complete, vomas, and loss of sensibility, which continued till the patient died. But the paralysis came on gradually, and not till after these comatose symptoms had existed some time.

Now of these two conditions I think we may very justly attribute the symptoms in our patient Ware's case to a white softening, followed by a rupture of bloodvessels, and an effusion of blood.

The numbness, which he described as having occurred before the paralysis, indicated that morbid changes were slowly creeping on, and after a time rupture of the bloodvessels, and disruption of fibres, took place, when followed the paralysis and the three weeks' stupor. The former being very complete, denoted a very complete solution of continuity on some part of the centre of volition; the sudden stupor implied that pressure existed within the cranium, but only to a moderate extent, for a large effusion, capable of producing such a complete paralysis, would undoubtedly have caused profound coma.

This, I say, seems the *most probable* train of morbid processes, at the commencement of the case; but we cannot speak on this subject with great certainty, for we cannot fully depend on the accuracy of the patient's history of himself. It tallies, however, very well with what we observed of his subsequent history, and of the post-mortem examination.

A train of phenomena, however, very similar, but differing as regards the *sudden* supervention of the paralytic state, might have occurred, when the primary lesion was of the inflammatory kind.

Upon examination of the brain after

death, it was found that a considerable portion of the corpus striatum of the left side was completely destroyed and excavated, and that the cavity was filled with a creamy fluid, having somewhat the appearance of pus: the anterior and inner part of the corpus striatum was healthy, but the whole of the posterior and outer part was thus disorganized: a few bands of fibres, easily broken down, passed from one side of the cavity to the other. The creamy matter contained in the cavity was found on microscopical examination to contain great numbers of large cells, containing oily matter in large globules, and also in a state of extremely minute subdivision. These curious organic globules might suggest the idea, that some active reparative process was going on during life. What their precise signification is, I do not pretend to determine; but I think I may affirm that they are characteristic of a state of white softening, as I have found them in other cases, in which no doubt could exist of the atrophic nature of the lesion.

It seems almost certain, then, that this excavation of a portion of the corpus striatum must have been due to a white softening, followed by an effusion of blood, and which was the immediate cause of the breaking down of the fibres, and of the pressure which gave rise to the paralysis. No traces of blood remained on the diseased part, as no doubt there had been ample time for the disintegration of its particles, and for its absorption.

At first the paralysis was very complete, but afterwards the patient recovered a certain amount of power, especially in the lower extremities. If the paralysis had been accompanied with rigidity, I should have been led to the conclusion that the cerebral lesion was of an irritating nature. This rigid state of the paralysed limb (*when it comes on at the same time as, or very soon after, the paralysis*) is generally seen when some superficial part is affected, as the meninges, or the surface of the brain, or when there is a growth from the skull, or a tumor in the hemispheres, in some cases of inflammatory softening, or in some conditions keeping up a constant irritation; but when there is simple rupture of the fibres of a deep-seated part of the brain, as the corpus striatum, with or without pressure, there is no irritation: the paralysed muscles are quite lax.

There was no appearance whatever of rigidity in the muscles of the paralysed limbs. Now this is just the sort of paralysis (accompanied as it was by comatose symptoms) which would arise from a solution of continuity of fibres in the first instance, and from compression. A portion of the corpus striatum is destroyed, and the

healthy part is compressed by the effused blood. On the absorption of the latter a certain amount of power had returned in the limbs; and it depended on the possibility of repairing the broken-down portion, whether a complete restoration would take place. That parts so much diseased would have ever been completely repaired seems extremely unlikely for two reasons—first, because the arteries of the part did not seem in a perfectly healthy condition; and, secondly, because of the well-known tardiness of all reparative processes in the brain, where actual solution of continuity has taken place.

You remember that on several occasions we passed the galvanic current through the paralytic and the sound limbs in this case. This was done, as I explained to you at the time, for the purpose of ascertaining whether any irritant disease existed within the cranium at the seat of the paralyzing lesion.

If, on passing the galvanic current, you produce less contraction in the paralysed than in the sound side, then you may judge the cause to be of a *depressing* kind: if, on the other hand, the contractions in the diseased limb are the strongest, then you may conclude that the condition of the centre which causes the paralysis is *irritative*. But then you may bear in mind that that irritation is not always inflammatory.

The most important points in this case may be thus summed up:—

First, through some diseased state of the assimilative process, the arteries of the brain become diseased, and an insidious gradual process of softening takes place; rupture of one or more blood-vessels follows upon this, with solution of continuity of fibres, and compression of the neighbouring healthy brain-structure; after this we have absorption of the clot, and more or less attempt at reparation: it may be that some inflammation may take place around the clots, which may retard the process of recovery.

If you were called upon to treat a case of this kind from the beginning, what course would you adopt? If you can clearly make out that the lesion is not inflammatory, but, on the contrary, due to defective nutrition, the less you interfere the better. Keep the patient in the horizontal position, with the head a little raised; let the head be kept cool; unload the bowels in such a way as will involve the least effort on the part of the patient: a large turpentine and castor-oil clyster is generally more expeditious than purgatives given by the mouth. Bleeding is generally not admissible in such cases as this, as it

tends to increase the atrophic condition of the brain, and would, under such circumstances, favour rather than repress hæmorrhage.

When this patient came under treatment in the hospital, we did not at first administer any medicinal agent: he was kept quiet and nourished moderately. After one or two trials with the galvanism we observed, on one occasion, a little more excitability in the palsied limbs than in the sound under the influence of the inverse current. Finding that he had made no progress for some days, I was led, from this effect of the galvanism, to fear that some inflammation might have set up around the lesion; and, accordingly, I was induced to give him drachm doses of the solution of the bichloride of mercury—that is, as you know, the sixteenth of a grain of that salt three times a day. No good effect followed this treatment: on the contrary, I fear the salivation which took place sooner than might have been expected must have made him more susceptible of the poison of erysipelas.

After all, I must acknowledge that in this part of the treatment somewhat of the *nimia medici diligentia* was exhibited. It is a lesson hard to learn, and more difficult to act upon, that nature can do more than the physician; but it is a lesson which each succeeding year of increasing experience will impress upon you, and in no cases more than in those of chronic affections of the brain.

I had intended to have brought under your notice to-day another case of the same nature as Ware's, in which apoplectic effusions took place on both sides; but, as I fear I could not do so without making this lecture too long, I shall make this case the subject of my next lecture.

THE AFFAIRS OF THE MEDICAL PROTECTION OFFICE.

At a Meeting of the Leeds Members of the Medical Protection Office, London, Oct. 21st, 1850, George Wilson, Esq., in the chair, it was moved and seconded, and unanimously resolved, "that Mr. F. W. Brearey be requested by the agent to furnish the Leeds members with a full statement of the affairs of the Medical Protection Office to the close of his management, and that in the meantime no further accounts be supplied by the agent here, and that all monies collected by him since the 14th of Sept., on account of the Leeds members, be retained in his hands; and that this resolution be inserted in the Medical Gazette, the Lancet, and the Medical Times. The meeting adjourned until the accounts required be furnished.

Original Communications.

A SUPPLEMENT TO
SOME OBSERVATIONS ON THE
MUSCULAR CONTRACTIONSWHICH OCCASIONALLY HAPPEN AFTER
DEATH FROM CHOLERA.*Containing a notice of a case of Apoplexy,
after which similar Motions were re-
marked.*By WM. FREDERICK BARLOW, M.R.C.S.
Resident Medical Officer to the Westminster
Hospital.

[Continued from p. 600.]

It would, as I have hinted on a former occasion, be obviously of great interest to inquire into the condition of the muscles after death in cases of cholera by means of galvanism, for though the plan of percussion has been known to succeed in producing action, it would be absurd to use it for any precise or delicate purposes, in the place of an agent so subtle, so powerful, so sure, and effecting the mildest or most violent results in strictest obedience to the wishes of the experimenter. It would be desirable to know whether the muscles of those who manifest contractions when life is over, respond to this stimulus with an unusual readiness, and whether there be any ascertainable difference of irritability in favour of those muscles, to take some given instance, wherein they most frequently and energetically operate. It would not be useless to contrast, moreover, the muscular state of those dying in the cold stage and those perishing subsequent to reaction. And the opportunity might be taken of determining the effect which violent and repeated galvanism *might* have, through its exhausting the irritability of the muscles, on the period of the approach of rigor mortis.

I know not whether any observations have been made, by the assistance of galvanism, on the irritability of the muscles during life in cholera. It would probably be found, judging both from the various conditions of the muscular system of the subjects affected by it, and from the great dissimilitude prevailing as to the amount of cramp, to be very different in different cases. The

cause of the cramp may be in greater intensity in one case than another, and so irritate the muscles more, but the state of the fibres which manifest the phenomenon is, doubtless, dissimilar, and so extremely modifies and influences the action of that cause, that it would be preposterous to attempt to measure, as a general rule, the severity of the disorder by the degree of spasm. And, indeed, we know how many have sunk, unresistingly as it were, and as if overwhelmed by some mortal shock, without much suffering from muscular disorder. We must, if possible, so view the state of the muscles after death as connected with that which immediately preceded, as to make one continued history. The most violent cramps have affected the living where the most remarkable movements have stirred the dead; and I am not aware that muscular contractions have ever been noted, save in those who perished in the cold stage of cholera, the decline of which is the decline also of the spasmodic seizures.

I have said, that were galvanism employed to test the irritability of the living in this disease, that most likely the degree would be found very unequal. The amount of a property must, of course, not be judged of by its abnormal manifestation; the healthy who have no cramp, no convulsion, may obviously have more of that peculiar force, whereupon such phenomena depend *immediately*, than those whom they affect. In the former there is nothing to provoke such actions.

On one occasion I passed a very slight galvanic current through the lower lip of a man who lay fast sinking from cholera. The muscular fibres responded readily. Upon that I tried the lowest effectual force, and then applied the same power to the lip of a friend of mine who was by. It had, as far as the experiment could decide, the like amount of irritability. My own lip, on being similarly galvanised, was found more irritable than that of either.

There is, I think, no question more intricate, delicate, and difficult of investigation, than that of muscular irritability; and it is partly made so by its very opposite states in such as really are, or are at least supposed to be, without traces of disease. Modified extremely by the processes of digestion, respiration, circulation, all the delicate and in great part inscrutable operations of nutrition, sex, age, occupation, fa-

tigue, repose, and passions of the mind, is it wonderful that when we come to measure its relations to different individuals, and to the same individual at different times, that we should find our task one of great difficulty? And yet I doubt not that the subject of irritability would be found full of very many new applications by any able inquirer who would be prepared to contemplate the matter largely, and to make experiments with skill and patience.

I have, in the course of my observations, always spoken of the movements which took place after the cessation of the breathing and circulation, as occurring *after death*. Some, perhaps, following those who have written elaborately on what death is, may say that I have assumed the question. It was necessary to employ the word, not so much in a sense of refined philosophy, as in a wide and general meaning. I have used it, not only as people in general, but as the best writers on surgery and medicine are wont to employ it. I may be told that putrefaction has been pronounced as the only sure sign of death, but is not every one who dies, or almost every one, pronounced dead without such testimony? Would a physician, by the side of the dying, on being asked at some given moment if he were dead or no, say that he cannot tell, that he will not be positive until some signs of decay are unmistakably visible? No: he satisfies himself that the so-called "vital functions" are extinguished, and speaks accordingly. I happen to have lying before me Corvisart's work on diseases of the heart. I have made mention of death in the sense wherein he uses it in the following passage:—"I shall freely call the heart the main spring of the human machine; for let its action be suspended but for a moment, and there is apparent death; let it cease altogether, and there is real and sudden death."* Of course we must always have great care to distinguish between suspended animation and dissolution; and in this the stethoscope will be of importance sometimes, as has been strongly insisted on by M. Bouchut. But it will occasionally be impossible for a brief space to distinguish,

let us be as cautious as we will; the real state will be proved by the issue. In such a case we must act as though the dissolution were apparent only, since the nature of that issue may turn upon our own efforts.*

Haller speaks of the *vis insita* of muscles being "proper to life and the first hours *after death*."

Dr. Dowler, in his observations on post-mortem muscular contractions, has drawn attention to a subject of very considerable moment; and I trust it may not be thought superfluous to allude to it. He says,—“Post-mortem contractility, in its legal applications, is not without interest, showing as it does the absurdity of some grave *judicial decisions*, based solely on muscular motion, which latter has been held to be a sufficient proof of life in a new-born child, enabling the husband to inherit the estate of his wife during his life-time.”

It appears to me that lawyers have been placed in difficulty on this point by medical witnesses, and that it cannot be disposed of in the summary style of the American physician. He affirms, speaking of a particular case, that “the Court of Exchequer and the jury decided that the child was born alive, because, when it was immersed in a warm bath, a twitching and tremulous motion of the lips appeared twice.” But in criticising this conclusion, it is only fair to inquire what was the kind of medical testimony which the Court had to guide it. Was it placed in the dilemma of having to choose between contrary opinions? The matter is so weighty that I shall refer to the trial in question, as I find it cited by Dr. Taylor. It was the object of the plaintiff to show that the child was born alive. “Dr. Lyon, the accoucheur who attended the plaintiff's wife, had died some time before the trial; but it was proved that he had declared the child to have been living an hour before it was born,—that he had directed a warm bath to be prepared, and when the child was born gave it to the nurse to place in the bath. The child neither cried nor moved after its birth, nor did

* He adds the following note:—"It will not, surely, be objected to me, that, in death, a capillary circulation still exists for some time; that the hair, the beard, &c., grow in a dead subject; I hope I shall not be thought ridiculous, if I say, a man be dead, although his beard sprout."

* Lord Bacon says, in his "*Historia Vitæ et Mortis*,"—"Ad resuscitandum eos, qui deliquit animi aut catalepes subitas patiuntur, (quorum haud pauci, absque ope, etiam expiraturi fuissent,) hæc sunt in usu." And then, amongst the measures to be employed, he mentions the most generally useful of all, "*Subita inspersio aquæ frigidae in faciem*."

it manifest any sign of active existence; but the two women who placed the child in the bath swore, that while it was immersed, there appeared twice a twitching and tremulous motion of the lips. They informed the accoucheur of this, and he directed them to blow into its throat; but it did not exhibit any further evidence of life. The principal question on the trial was,—“Whether the tremulous motion of the lips was sufficient evidence of the child having been born alive?” The medical witnesses differed. Dr. Babington and Dr. Haighton gave their opinion, that had the vital principle been extinct, there could have been no muscular motion in any part of the body; therefore the child had, in their opinion, been born alive, or manifested life after its entire birth. Dr. Denman gave a contrary opinion: he contended that the child had not been born alive, and attempted to draw a distinction between uterine and extra-uterine life. He attributed the motions of the lips after birth to the remains of uterine life. The jury, however, under the direction of the court, did not adopt this view of the case: they pronounced the child to have been living; and by their verdict the plaintiff recovered the property of which he had been for ten years deprived.*

* See Medical Jurisprudence, p. 610. It appears from this report that, if Dr. Denman's evidence had been admitted, the husband could not have been made a tenant by the curtesy of England. The law draws a clear distinction between the life of the *unborn* and the *born* child. It seems also necessary that the child be born *during the life of the mother*; so that an infant brought quite alive into the world by means of the Caesarean section, would not enable the father, *unless the mother were living* at the time of the operation, to become tenant by curtesy. Blackstone says, “The issue also must be born during the life of the mother; for if the mother dies in labour, and the Caesarean operation is performed, the husband in this case shall not be tenant by the curtesy—because, at the instant of the mother's death, he was clearly not entitled, as having had no issue born, but the land descended to the child while he was yet in his mother's womb; and the estate, being once so vested, shall not afterwards be taken from him.” (See Commentaries, 12th edit., book ii., p. 127.) By the law of Scotland it seems “that curiality or curtesy only takes place where the issue has been heard to cry.” Even the act of *breathing*, the raising of one eyelid, and subsequent convulsions, have not been held enough. (See Dyer's Reports, vol. i., 25, a.) It must be proved that the child *cried*. But according to the English law, “though crying is the *strongest* evidence of its being born alive, it is not the *only* evidence.” “Some have had a notion that it must be heard to cry; but that is a mistake”—(see Blackstone, *opt. et loc. cit.*)—a point very well shown by the case of *Rish v. Palmer*, referred to above, in which, as has been stated, the circumstance of two muscular

The few observations which I shall venture on this history will be made with deference, because of their being directly opposed to deservedly esteemed authority. It plainly appears that the transfer of property, despite long possession, was dependent on the affirmed occurrence of one or two muscular quiverings. The decision rested, be it observed, not on what the medical witnesses *saw themselves*, but on what “two women” reported that they beheld very long back, so that it is impossible to say what was the exact appearance of the movements. There is no reason to suppose that they were voluntary, or in any way connected with consciousness. They were, in all probability, similar to those motions which occasionally happen in the completely dead, speaking of death as we generally view it. They were rather, as I think, evidences of *past* than of *present* life, looking at that complex, perfect life, the life of the body taken as a whole, and as distinguished from mere *local* vitality. Shall we consider *such* quiverings as signs of life, and not go one step beyond, and consider as manifestations of life also those movements of the muscular fibre which may be produced by galvanism long after the latest pulsation of the heart? So long as there is the faintest respiratory movement, or the least perceptible pulsation of an artery; so long as a beat at the heart can be felt, or the weakest sound of it be detected by the stethoscope, a person must be called alive, though just on the verge of death: but is he to be called living because a muscular fibre may chance to quiver? Are we to say of those cholera subjects who manifested such protracted contractions that they did not die till the last was over?

twitches happening upon immersion in a warm bath, and *ten* years previously to the time of trial, and sworn to, not by any medical witness, but by two females, “the nurse and the cook” (as one report declares), were held sufficient evidence of “living,” and to justify a decision in the plaintiff's favour. It may be well to add a passage from Blackstone on Tenancy by curtesy, in order to save the curious reader the trouble of making reference. “Tenant by the *curtesy of England*, is where a man marries a woman seised of an estate of inheritance, that is, of lands and tenements in fee-simple, or fee-tail; and has by her issue, born alive, which was capable of inheriting her estate. In this case, he shall, on the death of his wife, hold the lands for his life, as tenant by the curtesy of England.” See Stephen's Commentaries, Vol. i., second edition, p. 251. See also Beck's Medical Jurisprudence, seventh edition, p. 114. I had not seen the observations in this work, at the time of making the remarks which I have ventured.

To me the distinction which Dr. Denman drew between uterine and extra-uterine life seems a good one, and that which should have guided the Court.* Had the child performed but one act of inspiration the case would have been different; it would have given manifestation of a vital function, as we commonly express it, and have made, moreover, a movement *peculiar to extra-uterine life*. Two twitchings of the lips, which *might, perhaps, have never happened but for the impression of the heat*,† were held sufficient to decide the Court. The law of Scotland would, I am informed, have required proof of crying as evidence of respiration. It may be well questioned whether the question of life or death ought to turn upon the existence or absence of a muscular tremor. We must not, in fairness, call a quivering of this kind a sign of life sometimes, and then reject it at other times as an indication of "living," when it may square with our theories, or suit our convenience so to do. Whatever may be the view adopted by the reader, he will, I am sure, feel the necessity of considering the whole subject of what have been termed post-mortem contractions before positively concluding. He has not simply to ask himself what is life in a scientific and abstract point of view, but to consider what it is as the word is generally employed.‡

The action of the involuntary muscles after the annihilation of consciousness, and the cessation of the breathing, has now been very long observed. It is necessary to distinguish between the action of the heart, or intestines, or uterus, and that mere quivering of fibres, which in nowise reminds one of the special and perfect action of these different structures.

The state of the heart, like that of the voluntary muscles, is modified not by death simply, but by its kind: abrupt dissolution and slow decay leave it, no doubt, most oppositely irritable.§ The physiologist must be cautious of applying

unguardedly the observations made by him on one form of death to another form of it which is completely different.

It is to be reasonably expected, looking to the results of experiments on animals, that the heart in the human subject must quiver, more or less, after some violent and sudden deaths, when neither the touch can discover, nor the ear detect, how attentive soever, the least proof of action; and that the intestines must still persist in their vermicular contractions. Dr. Feldmann observed the heart of a decapitated criminal; the right auricle showed marked contractions on the pericardium being removed. Vesalius, as every one knows, is reported to have remarked a similar phenomenon in the dead body of a Spanish nobleman, and to have been, despite his rare and ardent devotion to anatomy, absurdly and ignominiously banished from a country which should have held him punctiliously in selectest honour.

Intussusception, of which there was no evidence during life, has been again and again discovered after death. Rokitsansky is of opinion that it "occurs during the last moments;" but I think it not unlikely that it may also happen at a later period than that phrase implies. It results, according to him, from unequal irritability of the intestine—an ingenious idea! for it is easy to suppose that one part of it may be lax and passive, whilst another, directly above, is in a state of active contraction. Probably the different states of irritability in different parts of the intestinal canal, which we cannot suppose to be without exception uniformly irritable throughout, may occasionally give rise to intussusception in life-time also, and explain, moreover, certain inequalities or irregularities of rigor mortis. As to the latter point, it may be inferred that the most irritable portions, being the latest to contract, and remaining contracted the longest, may now and then be seen by the pathologist in the form of those partial constrictions which seem more satisfactorily explained by a reference to the effects of rigor mortis than they can be otherwise.*

* Yet how could the Court have decided otherwise than they did, in the face of two such able and distinguished witnesses as Dr. Haighton and the late Dr. Babington?

† I have seen muscles contract on the direct application of heat to their fibres.

‡ The reader will find some definitions of life in a note in the 8d volume of Mr. Palmer's edition of Hunter's Works, p. 126.

§ This has been forcibly insisted on by Mr. Travers in his "Inquiry."

* The reader may compare the observations of Rokitsansky on the production of intussusception with those which have been stated by John Hunter.

Rokitsansky says:—"We naturally ask how the intussusception is brought about, and how its enlargement is effected?"

"The cause is to be found either in the contrac-

The time at which rigor mortis of the heart comes and goes no doubt varies extremely; and the organ is most likely to be found rigid in those cases in which it approaches tardily and lingers long. In some cases it may be presumed to have come and gone before the time of the examination of the body.

The *peristaltic* action of the uterus may very likely endure, in some instances, after respiration has ceased, and so of itself complete a delivery.* I know not whether any observations have been made on rigor mortis in relation to the uterus; but certainly it will not be proper to infer that this organ was contracted before death simply because it is beheld contracted after it. This point must be borne in mind in the examination of women who untimely perish in the parturient state.

Interesting as is the condition of the involuntary muscles after death, and instructive as it would be to consider it more fully, I must leave the subject now. It could not have been properly

tion or moveability of a piece of the intestine, on which account it passes into the adjoining or more capacious tube; or in the extreme expansion or relaxation of a segment of intestine, which gives rise to an inversion of the adjoining narrower or more innervated portion."—Rokitansky's *Pathological Anatomy*, printed for the Sydenham Society, p. 56.

Hunter observes:—"The manner in which it may take place is, by one portion of a loose intestine being contracted, and the part immediately below relaxed and dilated, under which circumstances it might very readily happen, by the contracted portion slipping a little way into that which is dilated; not primary action in either portion of intestine, but from some additional weight in the gut above. How far the peristaltic motion, by pushing the contents on to the contracted parts, may force them into the relaxed, I will not determine, but should rather suppose it would not."—See Hunter's *Works*, vol. iii., p. 587.

Rokitansky would seem to imply, by the phrase "more innervated portion," that the narrowing of the portions of gut which become involved is owing to its being more contractile than that part of the bowel which it enters. How far intussusception may be explained by the unequal irritability of different portions of the intestinal tube will be disputed no doubt; but, unlike what may be said of some assigned causes, it seems capable of explaining the effect in question. It may be imagined that opium could not be given without risk for intestinal spasm occupying some fixed and limited spot, if it had the effect of exclusively paralyzing the affected part without in any way influencing the contractile power of that immediately above it: a condition would in this way be produced very favourable, it may be presumed, to intussusception.

* I have lately, in an experiment, seen the most lively action in the uterus of an animal after it was removed from the body. It is quite clear that the organ can act independently of the spinal cord; but it is thrown into action, as many observations prove, through the reflex function of that organ.

omitted altogether, because the state of the involuntary muscular fibre is as much modified as that of the voluntary by the various modes of dying. A very good illustration of the opposite conditions of the muscles, left by unlike kinds of dissolution, is given by Sir Gilbert Blane, in the Croonian Lecture on Muscular Motion which he delivered before the Royal Society in 1788:—"It is a curious and well ascertained fact, that if a fish, immediately upon being taken out of the water, be stunned by a violent blow on the head, or by having the head crushed, the irritability and sweetness of the muscles will be preserved longer than if it had been allowed to die with the organs of sense entire. This is so well known to fishermen, that they put it in practice in order to make them longer susceptible of the operation called *crimping*. A salmon is one of the fish least tenacious of life, inasmuch that it will lose all signs of life in less than half an hour after it is taken out of the water if suffered to die without any further injury; but if, immediately on being caught, it receives a violent blow on the head, the muscles will show visible irritability for more than twelve hours afterwards."

It may be stated that, in all cases of sudden death, unless they are such as surprise persons either debilitated by long disease, or extremely weak from some cause or other, that the muscular fibre is extremely irritable, although it may not manifest irritability without some stimulant being applied. Abundant evidence of this fact has long been given by physiological experiment. The only wonder is, that the spontaneous mobility of the fibre after certain cases of quick destruction has been so tardy in attracting notice.

I have in a former paper alluded to the twitches of the muscular fibres seen in slaughtered animals. They are matters of popular, loose observation. I have observed them in mice which I have destroyed by carbonic acid and hæmorrhage. I have seen them in frogs many times. They do not appear to be any the more powerful, judging from what I have observed in the latter, from strychnine being administered previous to decapitation. This tetanus-producing poison never, as it seems, acts upon the muscular fibre, unless through the medium of the spinal cord.

As the state of nutrition of the fibre

at the period of experiment, and the way wherein it may have been circumstanced as to rest and exercise, make every difference in regard to the occurrence, extent, force, and duration of the quiverings of the fibres, the latter may, in some sense, indicate in what condition the muscles were, in relation to those matters, at the time of dying. I will sketch an experiment, with the view of illustrating the prevention of any manifestation of irritability by the impoverished state of the muscles. I took a frog which seemed extremely languid, and removed the head quickly; on stripping off the skin, no muscular quiverings were seen anywhere—no, not the faintest: the muscles were extremely pale, flabby, evidently ill-nourished, and some spots were seen here and there upon the fibres, looking not unlike dots of fading purpura.

In observing the state of animals killed by division of large blood-vessels, it is obviously necessary to well distinguish between those *immediate* convulsive motions which depend apparently, as Dr. Marshall Hall has pointed out, on centric irritation of the spinal cord, and those remoter quiverings, very slight and very restricted often, which seem referable to the state of muscular fibre. And here let it be said that there cannot by possibility be a greater contrast of appearance than that between those quick and universal motions which I have seen in animals destroyed by hæmorrhage, and those peculiarly wayward, partial, shifting, often extremely slow movements—quite alike, perhaps, in no two cases—that I have beheld after cholera and apoplexy. No one *could* confound them, unless by actual purpose: they are unlike in form, in rate—unlike, in short, in almost everything in period of occurrence often, being manifested through the muscular fibre.

It may, in the present state of our knowledge, be disputed whether motions of the limbs—such, I mean, as *actually change their position*—do really occur in sequence of death by hæmorrhage, after those convulsive motions are fairly over which directly follow the fatal loss of blood. It may, perhaps, be anticipated, supposing keen inquiry be made, that distinct motions of this kind will, though perchance rarely, be discovered in such a case; but, in watching for such, the greatest care must be taken not to mistake any reflex actions externally ex-

cited for, and confound them with these. I would here refer to some remarks of M. Bonchat, which he makes in speaking of the immobility of the body as an *immediate* sign of death.*

"L'immobilité du corps, c'est-à-dire la cessation des mouvements dans les membres, est un résultat incontestable de l'abolition des fonctions du système cérébro-spinal. Toutefois ce phénomène ne saurait être considéré comme un indice certain de la mort de l'homme, puisque dans un assez grand nombre de maladies nerveuses, l'épilepsie en particulier, il peut y avoir un instant cessation des mouvements musculaires, qui reparaissent lors du retour à la santé. De plus, si les muscles sont immobiles dans leur totalité, dans leur ensemble, de manière à empêcher tout mouvement des membres, leurs fibres se contractent encore partiellement plusieurs heures après la mort, surtout dans les cas de mort violente. C'est ce que Nysten a parfaitement bien établi par de nombreuses observations faites chez les suppliciés. On pourra les vérifier en assistant au travail des bouchers, qui, dans leurs abattoirs, sont tout surpris de voir les fibres musculaires se contracter encore chez un bœuf entièrement dépecé, coupé en deux, et *moné*, comme ils disent, environ près d'une heure après la mort. Ces contractions, qui seraient incapables d'imprimer aucun mouvement aux membres, ont cependant lieu dans les muscles de la vie de relation, c'est-à-dire dans les muscles volontaires, et sont utiles à indiquer, lorsqu'on parle de l'immobilité du corps après la cessation de la vie."

It is quite clear that the observations of Nysten on the great irritability which so long remains to the muscular fibres after death, and the abiding and pertinacious quiverings in slaughtered animals, have not yet been sufficiently applied to pathology. Looking to the principle on which they may depend, it would be most likely erroneous to separate the slight quiverings which merely oscillate on the surface of the muscles, without stirring the limbs, from those strong and striking contractions of them by which the legs and

* See *Traité des Signes de la Mort*, p. 145. It is quite plain that there may be death without immobility, and immobility without death, as cholera and syncope, to go no farther, prove absolutely.

arms are again and again variously moved in different directions, and, occasionally, in a manner which resembles, at first sight, that of the movements which spring from volition. One law, it is probable, includes the two classes of instances: and we may well believe—for there is much evidence towards this conclusion—that in cases of sudden and speedy dissolution, subcutaneous muscular quiverings are infinitely more common than is generally supposed, though, from their feebleness, or the thick covering of integument which invests them, they are invisible. We do not see the muscular twitchings of destroyed animals before we strip their skins from off them; and it is plain enough occasionally, that when we do see them, that we should have no chance of discerning them, unless we first laid the muscles bare. Lately, in watching the twitching of the muscles of an amputated leg, I failed to detect any save in those fibres which were exposed: these were most lively, were renewed at intervals, just as the after-death contractions of cholera are, and were readily excited by pricking or percussion, just as these may sometimes be. It may, indeed, be contended that the exposure to the air excites these quiverings, and that there is no proof whatever of their existence prior to their being laid open to its influence; but it must be remembered that twitches have been seen after mortal asphyxia (in which, be it observed, there must have been loss of oxygen in action upon the fibre than there generally is) without the skin being interfered with, and the most distinct contractions have, as has been amply shown, repeatedly affected uncovered muscles. More, too, is often needed to produce motion than exposure to the air, even where the muscles are very irritable; for frequently all the exposed fibres do not contract: one or two muscles only may be affected, and very restricted portions of these even. Why are these quiverings sometimes so limited, so partial? Are the particular fibres which are active more irritable than the rest, or are they influenced by some stimulus confined to them alone? In speaking thus of the asserted influence of the air, I am very far from saying that it has no power of any kind to cause or to maintain contractions. I would only go the length of asserting that it does not seem at all necessary to bring about

the activity of the fibre in all cases, though in some it may rouse the still muscle, and make the active keep disquiet longer than it would otherwise have done.

Although we cannot, in considering the whole subject, draw an impassable line between the slight quiverings and the remarkable motions, it still remains difficult to explain the reason of the extreme degree of contraction which prevails in some cases. Some aid, as has already been insisted, is undoubtedly afforded by making proper allowance for the various states of the muscular fibre, as to its amount of irritability at the time of death.

Those states, however, will not account for everything. The question has been asked, in previous observations, whether the muscular fibres, or terminal motor nerves supplying them, may not for some time after death be still irritated, in cases of cholera, by the poison of that disease, or the changes of blood it causes, whatever it or they may be? It seems the more reasonable to ask it, looking to what has been remarked in some poisoned animals. Yet we must be careful of attributing any peculiar power to the supposed poison of malignant cholera; seeing that remarkable motions have also followed yellow fever, and keeping in mind the state of the muscles in the case of apoplexy which I have described. This case is, indeed, of especial interest, when we come to argue on the morbid stimuli which, after the cessation of respiration and circulation, may be still supposed to influence the excitable fibre.

As to the slight undulatory motions which play about the muscles of animals just killed by loss of blood, it is quite clear that these cannot possibly be imputed to any morbid material. Does the sudden withdrawal of the impression of the circulation on the muscular fibres, or their minute motor nerves, ever lead to quiverings, just as the sudden loss of blood circulating through the spinal marrow has been held to cause general convulsion?

In our consideration of the property of muscular irritability, we must never forget how variously it may be excited, and how inexpressibly difficult it really is, not so much to affirm what its excitants are, as to assign to them their due and relative power. Its manifestations after death, as during life, may

depend clearly on different causes: now powerful though unseen stimuli may occasion them, though the measure of irritability be not extreme; now slight stimuli may provoke them, because of its amount being excessive; and again they may exist in unusual force, both on account of the power of the exciting cause and the rarely responsive condition of the fibre whereupon it has to play. And though I incline to the belief that the striking contractions which now and then happen after fatal cholera do really no more depend upon any excitement of the spinal marrow, centric or eccentric, than do the quiverings of an amputated limb—which of course cannot be, by any one, referred to it—I confess it far from certain that movements after death in the human subject are *never* referable to impressions made, directly or indirectly, on the spinal cord.

It would save a vast deal of trouble, and appear, at first sight, very consistent, to affirm at once and broadly some simple general principle, styling it a law, and then support it by very unceremoniously twisting some facts, and alone resting on those phenomena, with a cautious partiality, which point in unison towards a particular direction; but this mode of proceeding is not even to be thought of, and the inquirer must rather confess his difficulties, and wait patiently for further evidence, ere he decide on the power and relative power of supposed morbid excitants of the muscular fibre after extinguished life. Meanwhile, however, let him try to explain as much as he is able; more facts will be found in time; and what are now uncertain glimmerings may in the end expand, and brighten into a full, clear light.

The main circumstance in relation to these post-mortem movements is, so far as we can see at present, undoubtedly the *suddenness* or *rapidity* of death: yet, no sooner do we try to account for their occurrence (I speak now of *actual* movements of the limbs), by quick dissolution, than we are met at once by the observation, that as yet they have been seen only after a *few* instances of swift loss of life. This is certainly true when we come to consider how very seldom, except, indeed, in cases of cholera, they have been observed. As yet, so far as I know, they have been noted in one case of apoplexy

only, although this is a disease in which one might think they would have been more commonly beheld. At present there is a dearth of instances.*

The subject is rendered much easier by viewing the slight quiverings, and the changes of position, merely as phenomena which differ in *degree*: still that difference of degree is so extremely marked, that it is not a little difficult to clear up, even on making all those allowances for variations in the condition of the muscles which may be presumed to exist at the last moment. It is one thing—let the remark be repeated—to say that a muscle is so irritable that the least stimulant will act, and another to state clearly the cause of its action. Yet it is certainly a great step to see plainly some conditions which favour, and some circumstances which preclude, any visible manifestations of post-mortem irritability; and no little thing to be able to disabuse our minds effectually of the notion that such as the more remarkable are peculiar to cholera. It may be that they have been so much more frequently observed in this disease from the sad circumstance of its ruthless mortality, which, unhappily, affords us such a multitude of opportunities of examining bodies robbed speedily of life.

It may be said, that if muscular tremors are not seen after death in cases of chronic maladies, wherein the vitality of all the tissues is impaired exceedingly before dissolution, that they are at times beheld very frequent and palpable on the verge of its event, as—to take a good and familiar illustration—in typhus fever. True: but in typhus fever, and some other cases wherein such tremors occur, I apprehend that their presence may be satisfactorily accounted for by

* I find the following passage, by Dr. Hamett, in the substance of the Official Medical Reports on Cholera as it prevailed among the Poor at Dantzick between the end of May and the first part of September, 1831. Alluding to the "morbidity effects," he says:—"The next is the invariable contraction of the bladder; and another, which, although not apparently constant after death from this disease, is seldom or never to be met with after death from others—namely, slight spasmodic contractions or movements, if they may be so called, in the muscular fibres here and there in the body, and more especially in the face and extremities, not only immediately, but some time after dissolution." From the use here made of the word *slight*, and the expression, "if they may be so called," applied to movements, it would appear that Dr. Hamett did not observe the most remarkable kind of contractions which are known to happen. (See the above substance of Reports, p. 57).

supposing the muscles to be agitated by mental influences on the one hand, whilst imperfectly controlled by volition on the other.*

The quivering of the fibres of an amputated limb is as good an instance as can well be given of the action of muscles independently of any influence of the nervous centres; for it takes place under circumstances of separation from them. It may be regarded as an elementary fact ever to be borne in mind in the consideration of certain obscure cases of muscular contraction which we are utterly at a loss to explain. There are instances occurring in which no one, let his acumen and his knowledge of nervous and muscular phenomena be what they may, who could say for certain of some forms of spasmodic action, whether they be centric, eccentric, or referable to some irritation of the muscle itself.

There is, if I mistake not, as much difficulty in accounting satisfactorily for the state of the muscles in cholera during life-time, as in explaining conclusively the various phenomena presented by them after death.

1. Are the cramps produced reflexly by irritation of the mucous membrane of the intestine?

2. Or are they occasioned, centrally, by the blood circulating through the spinal cord?

3. Or are they brought about by changes of the blood-current in the muscles themselves, affecting their fibres or motor nerves?

These are questions which, in my humble opinion, are far more easily asked than answered. They may be as yet pronounced open questions.

I much hesitate to subscribe to the view which has been entertained of their being reflex, because of some points attaching to their form and course, and because, also, of the remarkable rarity of general convulsion in that stage of cholera wherein they exclusively occur.

On the same grounds, too, I think it very doubtful whether they be of centric origin. It is, however, known well

enough that irritation of the cord will produce cramps; and the way in which they not unfrequently forerun, and, viewed properly, foretell incipient diseases of the spinal marrow, is an interesting illustration of the fact. If the altered circulation of the cord itself did really provoke them, is it not inexplicable why the glottis should not be sometimes closed, and the patient either perish from, or be imminently endangered by, passed asphyxia?

The third question cannot, viewed closely, be replied to with a ready and unconditional affirmative, though I think there are facts which make it very probable that the seizures of the muscles in cholera depend upon either direct irritation of its fibres or the terminal motor nerves supplying them.*

One sees a particular muscle in rigid and long maintained action, when—not seldom—scarcely any other is affected at the time. If this contraction be from centric irritation of the cord, it must obviously be applied most partially. When altered states of the blood produce spasms, they generally produce such as are general,—such, in brief, as the poison of scarlatina, or of measles, or variola, brings severally about. No doubt centric irritation will occasionally give rise to most limited movements; but when it does so, it is by influencing a particular spot originating fibres which are distributed to the muscles, or muscle, or even portion of muscle, which may be affected. Reflected irritation may be held more reasonably to explain these cramps; but this mostly displays itself in a different form to that which marks the cramps of cholera, and is so prone to produce general convulsion, that one, as I have said above, can scarcely wonder that it does not do so in cholera at times, if really it be the cause of the muscular disturbances. But why, as the blood, poisoned throughout, pervades all the muscular fibres of the body, are not the cramps universal? Because, it may be, those fibres are variously irritable. Galvanism will not affect, in some cases, all the muscles of the body equally. Cramp has an extraordinary partiality for the muscles of the calf of the leg. Certainly there are none, as a general rule, more

* See an Essay by the writer on Some of the Relations of Volition to the Physiology and Pathology of the Spinal Cord, MEDICAL GAZETTE, vol. for 1846.

† I have never seen what could be properly called a general convulsion in the cold period of the disease. Once in the subsequent feverish stage, I saw a patient who had some time been comatose die in a true fit of epilepsy.

* Dr. Wilson has very especially insisted on the action of the blood on the muscle itself as a cause of spasm. See his work on "Spasm, Lamguor, and Palsy."

often used, more fully nourished, than these are; and there are none which may be supposed in a condition, therefore, to respond more lively to any morbid impression.

Why, again, if the muscles be so influenced by the blood as to contract, and even become pertinaciously rigid, is the *heart*, the principal and most irritable of all, exempted from a seizure which would soon destroy? It is in vain for me to essay an altogether conclusive answer to this hard inquiry; but let it be here well remembered that a stimulus—galvanism to wit—which may most powerfully affect the voluntary muscles will very inferiorly influence the involuntary.*

We have been hitherto far from fully appreciating, and still farther from fully applying, the facts which we know relative to the excitement of muscular action in demonstrable independency of the nervous centres: I say not of the terminal motor nerves.

The effect of altered states of the blood in producing general convulsion is clear enough, though by no means at present adequately investigated; but far less explored has been its influence in the production of cramps and certain partial spasmodic maladies. "A fit of the gout," says Heberden, "has been judged to suspend the power of cramps; but I am much more strongly convinced that the gout is apt to breed and foster them."†

One of the most remarkable circumstances in the history of cholera is the different degrees of cramp. Some, during last year's epidemic, fell unresistingly, and experienced hardly a pang: they died as people overwhelmed by violent, irrecoverable shock. Others were racked most piteously: no trait was wanting to make more perfect their finished picture of severest agony. The muscles, defying all rule completely, were gathered here and there into hard, conspicuous knots; the limbs were strangely distorted, and effectually fixed; the features, just, perhaps, as the will would have directed, or emotion influenced them, were locked rudely in terrible grimace; and the enfeebled irrita-

bility of the seats of spasm gave the only respite.

The whole state of the nervous system also varied greatly. Let us for a moment bring to memory the condition of the brain. Some lay in a silent prostration, which might, perchance, be mistaken for coma at first sight,—lay, not moving a muscle, save when some pain distressed, some want was urgent. Their case, not without parallel, was comparable to that which we see almost daily in fatal burns, wherein the mind and body seem prostrated together and alike.* But in other instances there was, on the contrary, an extreme restlessness, which gave way to nothing, save one—mortal anodyne,—a fruitless, unrestrainable exertion to be remarked often in the sinking state, and making it the fleetest. I was much struck during the past epidemic by many instances in point; amongst others, by that of a young Irishwoman, who, in the prime of life, and, to all appearance, most desirous of living, rapidly perished. Hers was one of the too numerous cases which reminded us, "*remedia tardiora quam mala, et ut corpora lente augescunt citò extinguuntur.*"† Her lively temperament, contrasting with the palpable, fatal collapse, was manifested from time to time by a singular and marked vivacity of manner, by "an inquietude and restlessness which aggravated the ineffable prostration of her strength."‡ It was, indeed, impossible to help closely contemplating the mental condition of some patients, with their circulation so shocked; their respiration so imperfect; their blood so damaged! Wonderful to see the signal difference between the mind and body! Wonderful to see thought, will, emotion, thus active, thus enduring! Could we imagine some livid corpse to speak, move, "glare with unspeculative eye," we should hardly picture a more marvellous spectacle than the mind unsubdued by the destroying pestilence,—when the keen anatomy of the features dismally shrunken as by long disease; the body shrivelled, livid, earthy cold; the hoarse, almost inaudible voice, now whispering of the quenchless thirst, now of the tor-

* See Commentaries, fourth edition, p. 342.

† L'irritabilité est la même partout; elle ne varie qu'en intensité dans les différents muscles, mais elle n'obéit pas aux mêmes stimules dans tous les muscles.—See Œuvres de Legallois, tome i. p. 222.

* See An Inquiry, &c., by Benjamin Travers, F.R.S., p. 107, and Leçons Orales de Clinique Chirurgicale, par M. le Baron Dupuytren, tome i. p. 428.

† Tacitus, Vita Agricola.

‡ Tallmen as cited by Dr. Chambers.

turing cramp; the sighing, irregular, long-pausing breathing, ever threatening to fail; the languid heart; the pulseless wrist; and all the symptoms of the disease combined to prognosticate swift dissolution.

Let me now for the present conclude these observations, which have reached already to an unexpected length. Whatever errors of reasoning may be found in the few comments I have ventured, will, I hope, in consideration of the manifest difficulties of the subject, be viewed indulgently. Every phenomenon which manifests or illustrates that force whereby the muscles immediately contract is of considerable interest: and the (so-called) spontaneous manifestation of it after death is of unusual attraction, owing to the circumstances under which it occurs. Unexpected by the generality of observers, it is apt to take them at a disadvantage, and, perchance, to hurry some of them into erroneous inferences.

But the whole subject of that power whereby the muscles act directly, deserves, though it may obtain not, the most general study. No wonder that it so occupied the severe attention of the great intellect of Haller! Imagine the innumerable fibres, voluntary and involuntary, which, through changes so infinite, and a time so protracted, it never refuses to supply with energy! Imagine the sources of its renewal, the modes of its disturbance, the causes of its exhaustion, and all the purposes whereunto it is devoted and adapted—some vital, others essential to life's noblest ends! Mark it exemplified in the contractions of the heart, the expression of the features, the working of the hand; now note it in the tardiest thing that crawls; now in the swiftest that flies through viewless air! Think only of the manifold uses to which it is converted throughout the life of animals in its every form! Can we ever behold with incurious eye any demonstration of this pervading, indispensable force, wherever or whenever we may perceive it? Shall we not rather diligently contemplate it under every circumstance, and try to class truly the minutest movement which it possibly can cause, no matter how unmeaning it may seem at first sight, with those of its own kind?

Westminster Hospital, Oct. 9, 1830.

ON NARCOTISM BY THE INHALATION OF VAPOURS.

By JOHN SNOW, M.D.

[Continued from page 327.]

PART XV.

Detection of ether in the expired air after inhalation—Detection of alcohol in the expired air after it had been taken into the stomach—The effects of chloroform and ether prolonged by causing the exhaled vapour to be re-inspired.

In my last communication it was shown that the vapour of chloroform can be detected by chemical tests, as it exhales from the blood in the expired air. The strong odour of ether, which continues to be perceived for hours in the breath of persons who have inhaled it, is a pretty good indication that this medicine is exhaled from the blood in a similar manner. I thought it desirable, however, to have a more material proof of the fact, than that afforded by the odour, and therefore contrived and performed the following experiments:—

Exp. 58.—As a preliminary measure I passed the expired air for twenty minutes through strong sulphuric acid, inspiring by the nostrils, and expiring by the mouth, through a spiral tube immersed in cold water; a continuation of this tube afterwards dipping into half an ounce of sulphuric acid contained in a bottle. The acid was afterwards boiled in a small retort, the beak of which communicated with a gas receiver under water. No gas was obtained beyond the air expelled from the retort by the heat, and the acid was not changed in colour.

Exp. 59.—On the following day—August 1st, I inhaled three fluid drachms of ether gradually, in the course of four minutes, and was rendered almost unconscious. After waiting for a minute, in order that the lungs might be entirely emptied of the vapour remaining at the conclusion of the inhalation, I commenced to pass the expired air through sulphuric acid, the air first passing through a spiral tube immersed in iced water, to condense the watery vapour, as in the last experiment. This process was continued for twenty minutes. A

few hours afterwards the sulphuric acid was placed in a small retort, the beak of which communicated with a receiver under water, and was heated with the flame of a spirit lamp. It was gradually rendered quite black by the heat, and 11·3 cubic inches of gas were obtained in the jar. The jar being transferred to the mercurial trough, and solution of caustic potash being introduced, the contents, after standing for an hour or two, and being agitated occasionally, till no further reduction of bulk would take place, were diminished to 8·9 cubic inches, showing an absorption of 7·4 cubic inches of carbonic acid gas. The jar being reversed, and a lighted taper being applied to its mouth, its remaining contents took fire, and burnt with a bluish flame. As 2·6 cubic inches of air were contained in the retort at the commencement of the process, the quantity of inflammable gas was probably 1·8 cubic inch.

Exp. 60.—On August 2nd, I again inhaled three fluid drachms of ether, and proceeded exactly as in the last experiment. The sulphuric acid was rendered black as before, and 7·6 cubic inches of gas were collected in the receiver. Potash absorbed 3·2 cubic inches of this, and the jar being reversed, and a lighted taper applied to its mouth, the remaining contents burnt with a flame which gradually descended in the jar to the surface of the mercury. Allowing for the air expelled from the retort, the quantity of combustible gas was 1·6 cubic inch.

Exp. 61.—In order to ascertain the nature of the inflammable gas produced, another experiment was performed, on a subsequent day. The same quantity of ether was inhaled, and the expired air was passed through sulphuric acid in the same manner. The acid was boiled in the retort, until 7·1 cubic inches of gas were obtained in the receiver, when the process was stopped. Solution of potassa being agitated in the gas absorbed 3·5 cubic inches. Two cubic inches of oxygen gas were added to the remaining 3·6 cubic inches, and a portion of the mixed gases was transferred to Dr. Ure's eudiometer. As it did not explode with the spark from a small electric machine, a small quantity of pure hydrogen gas was added, when explosion took place with the following result. The quantities are in hundredths of a cubic inch:—

Hydrogen	3·0
Oxygen, &c. . . .	21·0
Total	24·0
After explosion . .	16·5
Loss of volume . .	7·5

being a diminution of three parts more than the hydrogen would occasion. The remaining 16·5 parts were agitated with a little solution of potassa, when a further diminution of about six parts took place; a little more than ten parts being left. This result shows that the inflammable gas under examination was carbonic oxide, which, in becoming converted into an equal volume of carbonic acid, consumes half its own volume of oxygen. The beak and upper part of the small retort contained 1·9 cubic inch of air, which would be necessarily expelled into the gas receiver, and when this and the oxygen afterwards added are subtracted, the remainder is in the same proportion, very nearly, as the carbonic acid produced by the explosion; consequently the gases obtained by heating the sulphuric acid were carbonic acid gas, and carbonic oxide.

In these experiments, the ether passing off in the expired air is in part absorbed by the sulphuric acid, and on the application of heat is decomposed into various products; the above gases being given off, and free carbon remaining in the acid, and rendering it black. Sulphurous acid gas is evolved, but is absorbed by the water. On adding a few minims of ether to half an ounce of sulphuric acid, and operating in the same way as in the above experiments, the same products were obtained. Alcohol, when heated with a large excess of sulphuric acid, yields the same products as ether; but as I had taken no kind of fermented liquor before inhaling the ether in the above experiments, these products must have resulted from the sulphuric ether.

From the general resemblance between the action of alcohol, ether, and chloroform, and from these substances being governed in their action by some of the same general laws, as previously shown in the experiments on frogs and fishes,* it might be expected that since chloroform and ether can be shown to pass off in the expired air, alcohol would also

* MED. GAZ., last vol., p. 622.

be exhaled in the same manner. Common experience, so far as the sense of smell is concerned, is in accordance with this view. Leibig, however, says,* "according to all the observations hitherto made, neither the expired air, nor the urine, contains any trace of alcohol, after indulgence in spirituous liquors." This, so far as I know, was true as regards the human subject, but Dr. Percy† had obtained alcohol by distilling the urine of a dog, to which he had given a fatal dose of it.

Feeling a strong conviction that alcohol must pass off in the breath, I have made many experiments during the last twelvemonths, with a view to detect it. At first, I caused the expired air, after spirit had been drunk, to pass, for an hour or longer, through a spiral tube, immersed in ice and salt, but did not succeed in detecting alcohol in the condensed water. A little reflection, however, made it evident that alcohol could only exist there in extremely minute quantities; for the spirit which had been taken, being equivalent only to two ounces of absolute alcohol, the inspired air would only be able to take up about a two-hundredth part as much vapour of alcohol as would saturate it, at the heat of the body; and it would be in vain to attempt to reduce the air to such a low temperature as would cause it to deposit any part of so relatively small an amount of vapour; in other words, the alcoholic dew-point of the air must be lower than the temperature of the ice and salt, and, consequently, all the spirit that could be arrested would be that which might be attracted by the small quantity of condensed water. By collecting together the water condensed from the breath in six different experiments, I succeeded, however, in obtaining spirit in a pure state, as will be detailed further on.

In the following experiments the same method was employed, as detailed above, for the detection of ether.

EXP. 62.—August 6th, 1850. Two ounces and a half of rectified spirit of wine, of 80 per cent., were diluted with rather less than a pint of water, and taken, with bread and butter, at supper-time. A slight feeling of inebriation was occasioned by it, but not sufficient to interfere, in the least, with the proper

performance of the experiment. The air was afterwards taken in by the nostrils and breathed out by the mouth, through a wide tube communicating with a metal box containing a spiral arrangement, by which the air was obliged to pass round several times. This box was surrounded with ice. The air was conducted next, by a glass tube half an inch wide, to the bottom of a bottle containing half a fluid ounce of sulphuric acid. The object of condensing the moisture of the breath, in the metal box, was to prevent its diluting the sulphuric acid beyond the point at which it ceases to decompose alcohol when heated. The expired air was, in this manner, passed through the sulphuric acid for thirty-five minutes. Care was taken that no air coming from the stomach by eructation should pass into the apparatus. Two and a half fluid drachms of clear water were condensed in the metal box. The following morning, the sulphuric acid was put into a small retort, communicating with a gas receiver over water, and heated with the flame of a spirit lamp. The acid was rendered quite black, and 5·1 cubic inches of gas were obtained, of which 2·6 cubic inches consisted of air from the retort. The receiver being transferred to the mercurial trough, and a little solution of potassa introduced, 1·65 cubic inches were absorbed. The jar being inverted, and a light applied to its mouth, the remaining contents took fire, the flame gradually descending in the jar to the surface of the mercury. The quantity of inflammable gas was 0·85 cubic inch.

EXP. 63.—Another night the same quantity of rectified spirit was taken, in the same manner, and the expired air passed through the spiral box and the sulphuric acid as before. Six fluid drachms of acid were employed this time, and the process of breathing through it was continued for an hour. Two and a half drachms of water were again condensed in the metal box, and the acid was increased in bulk by rather more than half a drachm. The sulphuric acid was next morning placed in a retort and heated. It was turned black, and six cubic inches of gas were obtained, two of which consisted of air from the retort. Solution of potassa absorbed 3·45 cubic inches of carbonic acid gas, and the remaining contents of the receiver burnt with a slight explosion, on a light being

* *Animal Chemistry*, p. 239.

† *Prize Thesis* "On the Presence of Alcohol in the Brain," &c.

applied. The inflammable gas did not amount to more than 0.55 cubic inch.

Exp. 64.—The same quantity of rectified spirit was taken at night on another occasion, and the expired air passed for an hour through sulphuric acid in the same way as before. The quantity of acid employed this time was a fluid ounce. On the following morning six drachms of the acid were heated in a small retort: they were rendered quite black, and somewhat viscid. 4.85 cubic inches of gas were obtained in the receiver, of which 1.8 cubic inch consisted of air from the retort; potash absorbed 0.6 cubic inch; 0.85 cubic inch of the remainder was transferred into a small jar, to the mouth of which a taper was applied, when the contents burnt for a little time with a bluish flame. To the residue in the receiver 3.8 cubic inches of oxygen were added, and a portion of the mixture was introduced into the eudiometer. As it did not explode with the electric spark, a small quantity of pure hydrogen gas was added, when an explosion was effected with the following result:—

Hydrogen	3.0
Oxygen, &c. . . .	32.0
Total	35.0
After explosion . .	27.0
Diminution	8.0

being a loss of 3.5 more than occasioned by the hydrogen.

Solution of potassa being agitated in the remaining 27 parts, they were diminished to 10; showing an absorption of 8 parts of carbonic acid. The loss of volume was consequently very nearly half as great as the quantity of carbonic acid gas produced by the explosion; and therefore the inflammable gas under examination was carbonic oxide, the amount of which was just one-fourth of the mixed gas introduced into the eudiometer. It is evident on calculation that nearly 1.8 cubic inch of carbonic oxide must have been expelled from the retort, and that this and the carbonic acid were the only gases evolved by the sulphuric acid.

The decomposition which the alcohol, absorbed from the expired air, undergoes in the sulphuric acid is the same as that undergone by the ether in the experiments previously detailed.

Exp. 65.—The water condensed in

the metal box, surrounded with ice in the above three experiments, and in three others not related, amounted together to two ounces. It was placed in a retort, and about three drachms were distilled. This product was placed in a smaller retort, and about twenty minims were distilled into a small test tube. Dry carbonate of potassa was added to this till it would dissolve no more. In a little time, a layer of clear spirit, about the tenth of an inch in thickness, floated on the top of the solution of potash. A piece of asbestos being dipped in this, it burnt with a blue flame. A very little powdered camphor was dropped into a small tube, drawn at one end to a capillary point. This point being brought in contact with the liquid floating on the solution of potash, a little of it rose by capillary attraction, and was observed to dissolve the camphor within. On blowing at the other end of the tube, a minute drop of solution of camphor was forced out, and received on a piece of glass, when the spirit immediately evaporated, leaving a coating of camphor. These tests leave no doubt of the presence of alcohol. The process used in this experiment is similar to that employed by Dr. Percy for the detection of alcohol in the brain and other organs.

Exp. 66.—Two and a half fluid ounces of rectified spirit, of 80 per cent., were diluted with water, and taken at supper-time. The air was afterwards inspired: for fifty minutes by the nostrils, and expired by the mouth, through a glass tube which dipped into three ounces of water contained in a bottle. Next morning the water was put into a retort, and about three drachms were distilled, which were put into a smaller retort, and about twenty minims were distilled into a small test tube. On carbonate of potassa being added in excess, a thin layer of clear liquid floated on the surface. This was proved to be alcohol; for a little bit of asbestos being moistened in it, burnt with a blue flame, and it dissolved camphor in the way described in the former experiment.

Whilst the above experiments show that alcohol is exhaled in the breath after it has been taken into the stomach, a little consideration will prove that only a small part of it can be excreted in this manner. When there are two ounces of alcohol in the blood, the air which reaches the lungs can only take

up, as stated before, about a two-hundredth part as much as would saturate it at the temperature of the blood. At this rate, a person breathing the usual amount of air would only exhale about twelve minims of alcohol in an hour; consequently, if it had to pass off entirely in the expired air, its effects would continue for a very much longer period than they do; and, since alcohol can hardly be detected in the other excretions, it must be decomposed in the system into fresh products.

I have assumed from the first that the speedy subsidence of the narcotism caused by chloroform and ether, in comparison with that from alcohol and other narcotics, depends on the volatility of the former substances, which allows of their ready exit by the expired air. Indeed, the effects of these medicines usually subside in the period which a calculation founded on this view would assign to them. It was previously estimated, for instance, that twenty-four minims of chloroform are contained in the blood of an adult of average size in a state of very complete insensibility; this being about one-twenty-eighth part as much as the blood would dissolve. The inhalation being now discontinued, the fresh air which reaches the air cells will abstract from the blood nearly one-twenty-eighth part as much as it can hold in suspension at the temperature of 100° ; and as each hundred cubic inches of air, when saturated at 100° , contains 43.8 cubic inches of vapour of chloroform, $43.8 \div 28 = 1.54$ cubic inches, or 1.48 minims, will be the quantity removed by the first hundred cubic inches of air which reaches the air-cells. It has been shown that about half the inspired air gets as far as the air-cells; and, supposing the patient to be breathing 400 cubic inches in the minute, 200 cubic inches would act in the removal of the vapour. In this manner it would take two minutes and a half to reduce the quantity of chloroform from 24 to 18 minims, and the narcotism from the fourth to the third degree; after which the effects would diminish more slowly, and in three and a half minutes longer the narcotism would have diminished to the second degree. Then, as the air would only take up about one-fifty-sixth part as much as it would hold, in about five minutes longer we might expect the

return of consciousness; and the slight dizziness or confusion which might remain would subside still more gradually. The above statement expresses pretty well what usually occurs when the inhalation has been kept up for a little time. Children recover from the effects of chloroform more rapidly, on account of their quicker circulation and respiration. Old people, on the other hand, more slowly, for the opposite reason. When insensibility is produced in the course of two minutes for a short operation, and the inhalation is not repeated, the effects of the vapour subside more quickly than stated above; because, at the same time that the chloroform is passing off by the lungs, it is also escaping from the main current of the circulation, by permeating the coats of the small vessels, and diffusing itself in the tissues, and thus allowing the brain to resume its functions.

Ether is more volatile than chloroform; but being also much more soluble, the relative quantity absorbed into the system is so much greater, as to more than compensate for the superior volatility; and consequently the effects of ether subside somewhat more slowly than those of chloroform, the ether taking rather longer to pass off in the expired air.

It follows as a necessary consequence of this mode of excretion of a vapour, that, if its exhalation by the breath could in any way be stopped, its narcotic effects ought to be much prolonged. The following experiments show that such is the case:—

EXP. 67.—About 750 cubic inches of oxygen gas were introduced into a balloon of thin membrane, varnished with solution of Indian rubber in turpentine. The balloon was attached to one of the apertures of the spiral box which forms part of the ether inhaler I employ, and which was used for condensing the moisture in the experiments on alcohol previously detailed. Four ounces of solution of potassa were put into the inhaler, and to its other opening was attached a tube, connected with a face-piece without valves.* After inhaling as much chloroform as I could without

* I used the same arrangement in giving oxygen gas last year, at the request of Dr. Wilson, to a cholera patient in St. George's Hospital. The patient, who was in a state of collapse, was not saved or relieved by it.

being rendered unconscious, I immediately began to breathe the oxygen from and to the balloon, and over the solution of potassa. In this way the vapour exhaled in the breath had, the greater part of it, to be re-inspired. This process was continued for ten minutes, during which time the feeling of narcotism subsided very little, and it passed off very slowly afterwards, about half an hour elapsing before it was quite gone.

The oxygen was used, in this and the following experiments, to allow of respiration being continued for some time from the balloon without employing such an amount of air as would take up a great deal of the vapour. As there was air both in the lungs and inhaler at the beginning of the experiment, the oxygen was not breathed unmixed with nitrogen. The solution of caustic potash was employed for the purpose of absorbing the carbonic acid gas generated by respiration as the air passed to and fro over a large extent of its surface.

Exp. 68.—On another day the same quantity of oxygen and solution of potassa were employed, and fifteen minims of chloroform were placed in the spiral inhaler, in a small glass vessel, which prevented its mixing with the solution of potassa. I then began to breathe as in the former experiment, and continued to do so for fifteen minutes. The effects of the chloroform were gradually induced during the first three minutes, causing a considerable feeling of narcotism, but not producing unconsciousness. After the end of three minutes, the feeling of narcotism remained stationary till twelve minutes had elapsed, and during the last three minutes it very slightly diminished. The experiment was discontinued on account of a feeling of want of breath. It was half an hour longer before the effects of the chloroform were altogether removed.

Exp. 69.—The oxygen and solution of potassa were employed as before, and two and a half fluid drachms of ether were put into the inhaler, with the potash. The oxygen was breathed to and fro over the potash for twenty minutes. The effects of the ether were rapidly developed during the first three minutes, but not amounting to loss of consciousness. From this time, the influence of the ether remained nearly the same to

the end of the experiment, and afterwards subsided very gradually.

The effects of the small quantity of chloroform and ether inhaled in these experiments would have passed off in three or four minutes, if the exhaled vapour had been allowed to diffuse itself in the air in the usual way.

The amount of carbonic acid absorbed by the potassa was determined, and will be given in the next communication, as it forms a separate branch of the inquiry into the action of narcotic vapours.

[To be continued.]

ANALYSIS OF THE FLUID OF HYDROCELE.

DR. DALTON, of Boston, states that he has had an opportunity of examining the fluid of hydrocele in ten cases. The age of the patients varied from twenty-four to sixty years. The amount of fluid varied from three to seventeen and a half ounces. The average amount of fluid was a little over eleven ounces. It was generally clear, and of a yellow, greenish-yellow, or decidedly greenish colour. It was neutral in three cases, alkaline in seven. Its specific gravity varied from 1017 to 1035; the average being 1024.5. In every instance the fluid was strongly albuminous, often becoming quite solid and opaque by the action of heat. In one instance it also contained an abundance of cholesterine, in the form of small crystalline plates, which, floating about in the fluid, and glittering in the light, had to the naked eye the appearance of oil-globules. The fluid of this hydrocele was alkaline, rather large in quantity (seventeen ounces and a half), and of unusually high density (1035); but it presented no other peculiarity. The subject was a large, heavy man, with a dark complexion, rather muscular than fat, about thirty years of age, and in good health. The fluid had been accumulating for four years.

Simon has given the analysis of the fluid of a hydrocele which also contained cholesterine. It was of a yellow colour, without odour, alkaline, and sparkled when shaken, in the same manner as the above. It was also, like the above, remarkable for its density, containing fourteen per cent. of solid ingredient, which, Simon says, is a larger proportion than he has ever observed in any other serous fluid of a similar nature. He reports, also, five other analyses, extracted from various authors. In all these cases, the fluid was albuminous, and, in most of them, alkaline.—*American Journal of Medical Sciences* for July, 1850.

MEDICAL GAZETTE.

FRIDAY, NOVEMBER 1, 1850.

A SUBJECT of considerable interest in relation to the public health has lately undergone some discussion in the daily papers. It has attracted the attention of Government, and has given rise to a Parliamentary paper now before us.* We allude to the new process of *sugar-refining* patented by Dr. SCOFFERN.

We are induced to advert to this subject, because it involves questions of a purely scientific bearing, and on which those only who have studied medicine as physiologists, pathologists, and chemists, are competent to pronounce an opinion. When we assert that the public health is deeply involved in the issue, we think that we have stated enough to justify a notice of this subject in the pages of a medical periodical.

It has been long known that the ordinary process of sugar-refining is attended with considerable waste; and it has, we believe, been established that the new process patented by Dr. Scoffern, which consists in the refining of the syrups by a solution of *subacetate of lead*, yields a much larger product, and at the same time the process of purification is greatly expedited. The subacetate of lead acts in this, as in other cases, as a most perfect defecator. The surplus salt of lead is subsequently decomposed in the syrup, and rendered insoluble by the action of sulphurous acid, which sets free the acetic acid, and, as it is assumed, converts all the lead to sulphite. The sulphite, it is asserted, is entirely separated by filtration, and the filtered liquid gives no indication of the presence of lead on the

addition of the most delicate tests for that metal.

So far the theory is perfect; and we learn from the Report of Messrs. Thomson, Graham, and Hofmann, that in practice on a small scale, brown sugar may be thus refined, and no trace of lead remain in the saccharine liquid.

It became, however, a material question, whether, in the process of extracting or refining sugar on the large scale, and when the means and attention which the process could receive in a chemical laboratory were not at hand, some portion of lead-salt might not remain in the sugar, and affect the health of the consumers? In order to throw some light upon this question, we subjoin an extract from a letter, published in the Parliamentary Paper, addressed by the Governor of British Guiana to EARL GREY, in the early part of the present year. This extract refers to the Reports of Dr. Shier, Colonial Agricultural Chemist, and Dr. Blair, the Surgeon-General, respecting the results of experiments tried by Mr. Lambert, the agent of the patentees:—

"2. The Reports from the Agricultural Chemist prove conclusively that, however beautiful the clarification produced by the subacetate of lead, the subsequent separation of that pernicious ingredient from the cane juice, by means of sulphurous acid gas, cannot be effected thoroughly with the aid which science has as yet brought to bear upon the subject; and, in corroboration of the opinion which Dr. Shier consequently expresses as to the necessity of prohibiting the use of lead salts in the colonies, I further enclose an extract from a letter published in the newspapers by Dr. Wilton Turner, a chemist of eminence, who has for some years been employed in investigating improvements in sugar making, at the cost of individual proprietors in British Guiana, wherein he arrives at very similar conclusions.

"3. In bringing this strong corroboration of the anticipations entertained by the Board of Trade under your Lordship's notice, I am influenced by no

* SUGAR-REFINING: Return to an Order of the House of Commons, dated August 14th, 1850, &c.

desire of impugning the merits of a process which has not only been declared a valuable one by distinguished chemical authorities in Great Britain, but actually adopted in some of the refineries. My object is solely to enforce the necessity of every precaution being taken to prevent disasters from the ignorance or inadvertence of those who might be induced to work under the patent in the colonies.

"4. The properties of cane-juice, in the state in which it is expressed from the cane, are well known to all who have had the opportunity of comparison to be so very different from those of the solutions of sugar operated upon at home, that the fact of experiments proving successful in laboratories there is easily reconcilable with their failure when tried on a working scale on an estate in this colony.

"5. The agent, indeed, alleges, in a letter of which I think it right to annex an extract, that 'the presence of lead in the particular sample of sugar analysed by Dr. Shier might be traced to causes entirely extraneous and accidental; but if a gentleman who has devoted his time and attention to the process is liable to encounter such obstacles, it is frightful to conceive the consequences which might ensue, were it left in the hands of the class of persons usually employed in the manufacture of sugar in tropical countries.'

Dr. Shier furnishes in his report the following plain statement respecting the mode in which the experiments were conducted:—

"On the occasion of my visit to plantation 'Hope,' where Mr. Lambert had facilities which but few of the estates of the colony could afford, I saw Scoffern's process applied on the large scale. Mr. Lambert conducted the process with great ability, and strictly in accordance with the requirements of the Specification. Excess of lead salt was employed, and sulphurous acid gas was transmitted through the clarified and filtered juice for the removal of the excess of lead, till the test and counter test recommended in the Specification warranted its discontinuance. Carbonate of lime was then added, and the juice, after a certain amount of concentration on the copper's wall, was filtered a second time, and the evaporation was

finished in the vacuum-pan. The product was struck into the pneumatic washing apparatus in use on the estate; and, after the first exhaustion, the sugar was washed with less than the usual amount of water.

I was furnished with specimens both of the sugar and molasses, and have subjected them to analysis in the colonial laboratory. I have found lead both in the sugar and molasses, but in larger quantity in the latter.

It appears to us, that there could not be in this case any unfairness in the selection of specimens for chemical examination. They were prepared by the agent of the patentees according to the terms of the specification. It does not appear that the agent made any objection at the time to the chemical analysis of these specimens by Dr. Shier, but fifteen days after the date of Dr. Shier's reports, announcing the detection of lead, we find the agent attempting to assign the presence of this metal to causes entirely *extraneous* and *accidental*. Had no lead been discovered, the results of Dr. Shier's investigation would have been taken as a strong proof of the satisfactory nature of the process, but when it was clearly found by the experimentalist, we have *other* causes assigned, and a partially defective apparatus blamed as the source of the adulteration! Now we are sure our readers will agree with us that it was the agent's duty, either not to allow a chemical examination to be made, of specimens liable to contain lead, from "accidental causes" "or defective apparatus"—or if he did allow them to be taken for the purpose of analysis, he cannot withdraw from the conclusion that the process is defective, and that even when conducted *by himself*, "with great ability, and strictly in accordance with the requirements of the specification," the products are liable to contain lead.

The conclusion arrived at by Dr. Shier, the colonial chemist, is, "that

the use of lead-salts in clarifying cane-juice is *unsafe*, and that they would be doubly so in the hands of such workmen as are usually employed in the colony."

Dr. Blair says:—

"It is therefore my opinion, that the admixture of an aliment so variously, extensively, and continuously used as sugar with any preparation of lead, must be followed by disastrous consequences to the health of the consumers."

Dr. Wilton Turner, another colonial scientific authority, takes the following common-sense view of the matter:—

"The beautiful clarification produced by the sub-acetate of lead has long been known. It not only separates the proteine compounds, but at the same time throws down the acids of the juice and the colouring matter, so as to leave the juice of good canes little inferior to a solution of sugar in water. This juice is, however, contaminated with lead; and the object of Dr. Scoffern's process is to effect its complete separation. If this cannot be ensured, however we may regret abandoning a method producing so splendid a clarification, it must be done.

"The separation of the lead from the poisonous juice (I can use no milder term) is effected by sulphurous acid gas. Although far from granting that the chemistry of this process be correct to the extent claimed by the patentee, still, by way of argument, let it be allowed that the whole of the lead is rendered insoluble by this re-agent. To complete the separation of the lead, a perfect filtration must now be effected. This is a mechanical process, and, to be perfect, requires the greatest care and attention, even on a small scale. To suppose it could be conducted with the same nicety in the sugar-house, and without frequent failure, would be contrary to all our experience. It would be wrong to say such a thing was impossible, but its realization is highly improbable. The evidence offered by Dr. Scoffern, that such has been effected, though strong, and perhaps worthy of credit, must not be taken as proof that such would be the general working result in this colony. The reverse is so likely to be the case, and the effect of the most minute doses of lead so terrible, when

taken habitually, that all estates using this process should be compelled to brand their casks with the word 'lead.' If, on examination by a properly qualified person, the slightest trace of lead be found, the public safety requires the immediate destruction of such sugar. If the process be used, some such step will be imperatively called for on the part of Government."

It has been objected to the report of Dr. Shier, by those interested in supporting the patent, that he does not assign the *quantity* of lead detected by him. In answer to this he very justly observes:—

"I attached more importance to the establishment of the fact that lead is present, than to determine with exactness the quantity; for that will differ in every different making of sugar, according to the nature of the juice, the skill of the operator, and the suitability of the apparatus employed."

This objection, so far as the use of such sugar by the public is concerned, is unsound. If *any* lead be found, the process is obviously defective, and the quantity then becomes a matter of pure accident, depending on the care used in filtration and the experience of the operator. To put this in a stronger light, we may perhaps state that, in most of the cases of poisoning by water impregnated with lead, the quantity of the metallic salt in a gallon has been too small to allow the proportions to be determined with any accuracy.* It is rather the fact whether the lead be or be not entirely separated that it is desirable to ascertain; and on this point Dr. Shier's results are entirely in accordance with those more recently arrived at in England by Professors Thomson, Graham, and Hofmann.

These reports from the British Colonies induced the Home Government to institute an inquiry into the process, with a view of ascertaining "whether

* See letter by Mr. Herspath, *MED. GAZ.*, Sept. 20th, 1850, p. 518.

the separation of the lead be so effectual that the patent may be used with safety to the public."

The inquiry appears to us to have been conducted with the greatest fairness to all parties. Three professors of chemistry—i. e., Professor Thomson, of Glasgow; Professor Graham, of London; and Professor Hofmann, of the Royal College of Chemistry—were selected to make the chemical investigation: and three other gentlemen—Drs. Pereira, Taylor, and Carpenter—were requested to report medically on the results arrived at by those chemists.

The Parliamentary document now before us contains the reports of the Commissioners; and, in another article, we propose to lay the substance of them before our readers. We shall only remark at present that lead was discovered in the sugar and treacle prepared by the patent process in a quantity sufficient, as it is alleged, to endanger public health. The writers of the city articles in some of the daily journals, resting their case upon a letter published by Mr. R. WARINGTON, the pharmaceutical chemist of Apothecaries' Hall—a gentleman employed by the solicitors to the patentees—allege that the process is perfectly *safe*; that no lead exists in the sugar when *properly* made; that if any lead be by accident left in the sugar, it is as harmless as chalk, &c. Upon the authority of Mr. R. WARINGTON, of Apothecaries' Hall, and Dr. Gregory, of Edinburgh—whose curious experiments on the fattening properties of *sulphate of lead*, when given to rabbits, we lately noticed*—some of the newspaper writers have decided that the Government reporters have committed a series of mistakes both as to their facts and their inferences. In short, they contend that the Government should allow the experiment of producing lead-colic on a large scale in

this country, by the public sale of "*lead sugar*," to be fairly tried. There is a vulgar prejudice against knowingly taking even homœopathic doses of lead in any article of food; and, having the weakness to share in this prejudice, we propose at another time to consider whether the Government has not acted with wisdom and prudence in putting a check on this mode of refining sugar, by trusting to the opinions of persons who can have no prejudice to mislead them, in preference to acting on the loose statements of individuals interested in supporting only *one* view of this question, and that view, as we believe, fraught with great danger to the public health.

We elsewhere* publish a case in which a deliberate attempt was recently made to perpetrate robbery by the use of chloroform. We quite agree with our intelligent correspondent, Dr. Snow,† that many of the accounts published of attempted street robberies by the aid of chloroform, are to be regarded as ingenious inventions on the part of the complainants. In this instance, however, there can be no doubt that chloroform vapour was used; and the miscreant would have succeeded, perhaps in taking the life of the prosecutor, but for the alarm which, after a violent struggle, he was able to make.

This case suggests one or two points for reflection:—1. The placing of some reasonable restrictions on the sale of chloroform; and 2. An alteration in the law so as to secure a proper amount of punishment to the offender. With regard to the sale of this drug to the public, we have reason to believe that in the proposed Sale of Poisons Restriction Bill, chloroform will be included.

It appears that the prisoner in this case procured two bottles of the liquid

* See MEDICAL GAZETTE, August 20, p. 272.

* See page 772.

† MED. GAZ., vol. xlv. page 227.

at the shop of a druggist at Carlisle. It never occurred to the druggist who sold it, that a request made by a stranger to be supplied with such a large quantity of this drug, was of itself a most suspicious circumstance. We are informed that he procured it "without difficulty;" and any robber or assassin who requires chloroform in aid of lawless violence may equally procure it at any time. Considering the insidious use which may be made of chloroform in the perpetration of robbery, rape, and murder, this part of the liberty of the British subject ought, in our judgment, to be immediately restricted by some legal penalty. The prosecutor in the Kendal case nearly lost his life: a few minutes more might have put it out of his power to struggle, or to raise an alarm. An inquest might have been held, and, from the absence of the visible effects of poison, or marks of violence on his person, and from the presence of congestion in the bloodvessels, a jury might have come to the conclusion that the reverend gentleman had died from a sudden attack of apoplexy,—from fright, or by the visitation of God.

The present state of our law appears to us to be wholly inadequate to repress this form of Thuggism. It has been long known that narcotic drugs have been used by criminals for the purpose of more effectually perpetrating rape or robbery: it is a new and fearful mark of crime to employ a poisonous *vapour* which is likely to lead to death as certainly as the application of a ligature round the neck. When attacked during sleep, no person can be prepared for resistance; and although the stimulating effects of the *vapour* may arouse the individual, it will, perhaps, be too late to enable him to contend successfully against one or more criminals who have him so completely in their power. In what respect this mode of assailing a person differs from attempted strangulation or drowning, we do not know,

except that it indicates a more artful and ruthless method of disposing of a victim, and at the same time it leaves upon the body no marks of murderous violence.

According to English law, it is not a felony to administer poison to another, or to attempt to drown, suffocate, or strangle a person, unless the *intent* be to commit *murder*. A criminal may administer opium, cocculus indicus or chloroform-vapour, to another, with what amounts to comparative impunity, provided his *intent* be to *rob* or *ravish*, and not to murder. Thus the Kendal assassin *de facto*, if not in legal intent, has escaped at the Appleby sessions with *eighteen months imprisonment*! His offence was not felony; and we presume that he could not have been tried under the statute of poisoning, simply because he intended to *rob* and not to *murder* the prosecutor. This is surely a great defect in the law, if the Cumberland magistrates be correct in their view of it, and one which, for the security of the public, should be speedily remedied. If criminals wilfully employ deadly drugs to aid them in perpetrating rape and robbery, and thus inevitably place life in danger,—often as great as if a knife or a pistol had been used,—the punishment for a mere assault or misdemeanour is wholly inadequate to the offence. As our correspondent suggests, had the prisoner been caught in the act of sulphurising Sir James Graham's pheasants in Netherby Woods, instead of coolly chloroforming a venerable dissenting parson at an inn in Kendal, he would probably have had a much more severe sentence passed on him! We cannot distinguish this act from bow-stringing, throat-cutting, or any other nocturnal form of Thuggism where the intent is to murder, and which the law visits with death or transportation.

Some years since an assault was committed, at

England, on a commercial traveller while asleep. The intent was to rob, and to conceal the act of robbery by murder. The prosecutor escaped with his life after a desperate struggle; but the prisoner was convicted and executed. He used a knife or a razor instead of chloroform; but this is only a variation in the means of death; for the vapour of the drug thus forcibly and secretly administered during sleep may as seriously endanger life as the use of a weapon.

In the *Times* of Tuesday last we find that a man was tried and convicted, at the Central Criminal Court, for assaulting a person in the open street with an offensive weapon—a kind of elastic band, calculated to produce strangulation. The object was clearly to rob the prosecutor, and not to murder him, except as an aid to robbery. The prisoner was convicted, and sentenced to transportation for twenty years. How is such a sentence to be reconciled with the eighteen months imprisonment in the Kendal case? Such is the inconsistency in the administration of the criminal law in the north and south of England. One man escapes with a short imprisonment; another is transported for twenty years; and morally, medically, or legally, it would be impossible to point out any substantial difference in the two crimes. Chloroform vapour may place a man's life in jeopardy as much as a rope or a ligature applied directly to his neck. In the case of attempted strangulation, the punishment was not too severe; while in the attempted poisoning by chloroform there has been, we consider, a complete failure of justice. It is felony to administer to a woman drugs with intent to procure abortion, whether the abortion take place or not: and if we wish to suppress English Thuggism it should be made equally felony to administer noxious drugs with intent to commit rape or robbery.

LECTURES
ON THE
MEDICAL JURISPRUDENCE OF
INSANITY.

*Delivered in the Medical School of King's
College, Aberdeen.*

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LECTURE VII.

When is mental aberration such as to justify its being deemed insanity?—Discrimination of acute and maniacal delirium—Directions for the examination of the Insane—special difficulties—Lunacy certificates—Scottish Lunacy Law, Asylums, and form of certificate—Proceedings under the English Lunacy Act—form of certificate—When is mental unsoundness such as to justify interdiction?—interdiction defined—Scottish and English mode of procedure—grounds of interdiction—Testamentary capacity of the Insane.

YOU have now in the course we have followed seen the condition called Insanity compared with the similar and allied states which most resemble it; we have reviewed the legal doctrines and practice in regard to it—its nosological divisions with their leading characteristics; we have glanced at some particulars in its statistics, and dwelt at length on the physical, emotional, intellectual, and impulsive features of the disease, embracing with various other matters of forensic application, the medico-legal specialities of suicide, homicide, theft, fire-raising, drunkenness, and feigned insanity; let me request your attention farther to the discussion of certain judicial questions connected with the subject, and to various matters that are best considered in relation to these.

When is mental aberration such as to justify its being deemed Insanity?—Although it may have an essential character, insanity is not one uniform manifestation of psychical or physical inefficiency. Sometimes it is intellectual defect (imbecility—amentia), sometimes intellectual disorder (delirium—mania), sometimes destruction or loss of faculties (dementia), sometimes permanent but particular incapacity of judgment (delusion—monomania), and lastly, it may be merely diseased passion and loss of control (moral insanity). It has therefore to be distinguished from intellectual dul-

ness and ignorance, from febrile and temporary sympathetic delirium, from illusion and hallucination, from eccentricity and depravity. It is often associated with physical disorders, and on this ground bears a resemblance to all diseases which have sympathetic mental disturbance combined with them, some of which, as for example hysteria and hypochondriasis, are transitional to insanity, and not very readily to be distinguished from it. Intellectual deficiency is not insanity, unless it be the consequence of congenital defect of the faculties natural to humanity (amentia), or destruction of them by cerebral disease (dementia); the individual must through his whole life have been under the mental capacity of his fellows, or display deficiencies which are unnatural to his former self. Excitement, delirium, incoherence, are insanity (mania—dementia) only when persistent, associated with delusion, and concurrent with physical disorder. Illusion and hallucination indicate the insane condition (mania—monomania), when they cannot, as formerly described, be made subservient to the operation of comparison, when they defy the judgment, and govern the conduct despite of reason and free will. Exalted passion, erratic propensities, and tyrannous impulses, cannot be deemed mental disease (moral insanity), unless they be foreign to the character of humanity, or to the natural disposition of the individual, and the apparent consequences of diseased incapacity of self-control. I shall not recapitulate, however, what I have already said of these matters, but restrict myself to the marks which distinguish acute from maniacal delirium. I have repeatedly known the latter to be mistaken for the former.

In febrile delirium the ravings of the patient are less rapid, less intelligible, less distinctly enunciated, and less related to surrounding circumstances. The mental symptoms have not been the first to attract attention, and they are not the most urgent; there is serious bodily disorder, great disturbance of the circulation, a high temperature, evidence of uneasiness and pain,—probably tremor, feebleness of body, and loss of appetite,—symptoms very unusual in maniacal delirium, in which, on the contrary, at the period of greatest excitement, you find the patient perfectly fit to be out of bed, complaining of no pain, seeming to feel no uneasiness, and manifesting perhaps little thought of food, but neither irritability of the stomach nor loss of appetite. Maniacal delirium partakes more of the idiopathic character, continues an indefinite time, persists after the removal of its supposed causes or associated physical symptoms, and rarely

occurs before the age of puberty. Taking the history of the attack into account, you will not confound febrile or acute delirium with mania, if you satisfy yourself of its sympathetic origin, the great involution of all the powers of the mind, and the evident derangement of the physical as well as the mental functions of the nervous system. Delirium is the superaddition of a mental to a corporeal disorder. The best diagnostics are physical:—psychically the two conditions are much the same. The voluntary control over thought is lost in both, but voluntary motion is, so to speak, *plus* in mania, *minus* in delirium. Emotion is more perverted in the one, more obscured in the other. Perception and conception are confounded in both. In maniacal delirium there is a greater capacity of memory (the retention of impressions), and after recovery, accordingly, a greater power of recollection; a fact not available as a diagnostic, but very useful to be kept in remembrance by those who come in contact with the insane. A person after his recovery from maniacal delirium recollects all or most of the circumstances of his illness, the wanderings of his imagination, the absurdity of his actions, and the treatment to which he was subjected; whereas, he who has been suffering from febrile delirium finds that memory has been through much of the time incapacitated; something he may recollect vividly, something indifferently, but he has an incorrect notion of the time which has elapsed, and much of his illness is a blank in his mind.

Delirium being a state of unsoundness of mind in the fullest sense, is a condition which for the time annuls civil and criminal responsibility.

The Examination of Lunatics.—The testimony given by a medical man regarding the state of mind of one who is suspected to be insane, must be derived from a personal examination. Before the interview you should take care to be furnished with a full and particular history of the case, comprehending the age, social relations, profession, and physical character of the individual; the existence or not of hereditary predisposition, or of a previous insanity, his natural cast of mind, habits, acquirements, and character; his state of bodily health, the date of the appearance of derangement, and the presumed causes of the malady; the first indications of disturbance, the order and succession of symptoms bodily and mental, the difference between his present and his former habits and character, the type of the emotional disorder manifested, and the delusions, if any, by which his conduct is governed; the actions which are most characteristic of his insanity and the mode by which he is

likely or has been prepared to understand the nature of your visit. A complete history as above will save you much trouble, and possibly the lunatic much unnecessary questioning and irritation; and in some instances be of value in estimating the worth of the opinions and observations of relatives, in cases in which corroborative evidence is desirable.

Your introduction to the lunatic should be managed in such a way as to lead him to form no prejudice against you. As a general rule it may be well that he should not suspect your object, but there will be cases undoubtedly in which it would not be improper that he should know the intention of the visit: at all events it is desirable that you should not be an object of his ill-will. In the latter case, in which he is aware that his sanity is canvassed, you should feel, and he should be led to understand this, that soundness of mind is the judgment which you are anxious to have the grounds of forming. You should conduct yourself towards him, so far as the circumstances of his case permit, exactly as you would towards one sound in mind. In all cases where it is possible and proper, your interview with him should be private, without the presence of even his nearest or most interested relative. You should not exhibit timidity, nor hide the feeling under a blustering manner; rather have the presence of some one to protect you. You are to be sympathizing, not patronising. You are to treat him as an invalid, and not as a child, or as inferior in intellect. I am in these directions presuming that the case is one of some doubt and difficulty, and not, as is more usual, one in which the examination is chiefly a formal necessity, and requiring little tact and discrimination.

You may perhaps be able to discover pathognomonic symptoms of mental disease in the aspect and behaviour of the person, to lead you to be satisfied of his condition before he opens his mouth, more especially if he is not an entire stranger to you; but however high you may rate your power of discrimination in this way, you are never to venture a certificate solely on such grounds; you may be misled by stimulation, extreme and unwonted although natural emotion, a state of excitement from intoxication, depression from physical diseases or the influence of drugs and remedies, and the physiognomy of other diseases, as for example, epilepsy, and hysterical or other nervous affections; besides, the grounds of your opinion should be such as others can comprehend and rightly appreciate. Neither are you to be guided solely by the authority and statements of others; these may direct you in your

investigation, and confirm you in your conclusions, and may be cited as forming partly the foundation of your opinion, but you are always to satisfy your judgment by your own observations and examinations.

In most cases, as you are introduced to the individual who has to be examined in your character as a physician employed by his friends who are anxious respecting his bodily condition, you will find it generally most suitable to commence with an investigation of physical symptoms. Notice the conformation of the head, the expression of the countenance; ascertain the condition of the pulse, the state of the digestive function, the feelings in the head, the temperature of the scalp, the disposition in regard to sleep. Encourage him to represent his symptoms of ill health himself, the notions which he attaches to them, and the causes to which he supposes they are owing. These will frequently be obviously of an insane kind. In some cases the physical examination will afford nearly conclusive evidence, independent of the patient's own exposition of his maladies; there may be a pathognomonic countenance—shall we say also, in deference to Dr. Burrows, a pathognomonic odour not requiring to be substantiated? There may be evidence of physical ailment implying mental disorder. Had it been suitable to the objects of this class to speak of the diseases of lunatics, I should have alluded very expressly to a peculiar sort of paralysis, which even in its slight beginnings scarcely ever, or if I speak from my own experience never, exists but in combination with diseased minds. I allude to the paralysis of the insane, which you will find described in all the later writers on mental pathology, and most graphically portrayed in the Croonian Lectures of 1849, by Dr. Conolly.

Your investigation of the mental disorder should consist in a search for those characteristics of unsoundness which have been described. You are to seek for such changes of moral character as nothing but disease can explain; for such instances of imbecility, incoherence, and delusion, as attest imperfect mental power, disordered understanding, and incapacitated judgment, and for instances of conduct incompatible with a healthy faculty of self-control. If the patient labour under acute mania or chronic delirium you will meet with no difficulty, he will save you all necessity of searching, and furnish in a single interview more proofs of insanity than the memory can retain. If imbecility is the type of the disease, you are to look for its indications in the physiognomy, habits, tastes, and general conduct; in the powers of attention, efforts of judgment, and capacity of dealing with thoughts. You must take the

education and the means of acquiring information into account: shrewd examiners have before now mistaken ignorance for imbecility. A person may be more sound in mind than Malvolio was, without knowing what opinion Pythagoras held concerning wild-fowl. If some governing delusion is sought to be exposed, you may perhaps find occasion for all your acumen; for, as you are aware, monomaniacs, if their easily awakened suspiciousness be once aroused, occasionally exert no slight power of cunning and secretiveness. You must, in the first place, do all you can to obtain his confidence; if once you succeed in getting him to converse freely with you, he will gradually become communicative, and come nearer and nearer to the delusion engrossing his mind. If he should fail to do so, try to make some of his singularities and eccentricities a subject of discussion, express an interest in them, and seek for an explanation of them. If he should still fail to expose his ruling delusion, you should, from possessing the necessary key, take a judicious opportunity of introducing the subject yourself, and question him of his belief regarding it. Few monomaniacs are proof against this, however much they may wish to conceal their insane notion. I have never known one directly and persistently to repudiate his delusion; he tries, but does not succeed. When pushed to the point he displays his morbid fancy, or he manifests confusion of thought and incoherence of language in relation to it, or he expresses himself mysteriously and with reserve, or he gets angry, declines to speak on that matter, or sullenly remains silent,—a great effort to him. In all cases advantage should be taken of a lunatic's willingness to write, for even when no characteristics of insanity have been observed in his conversation, you will frequently in his letter or statement find traces of delusion, incoherence, or emotional aberration. By sympathizing with the patient you will usually succeed much better than by the most ingenious attempts to compel a display of his delusion; but sometimes, when other expedients have been unsuccessful, it may be advantageous to accuse him, so to speak, of delusion and insanity, so as to lead him to an explanation and defence of conduct which appears extravagant and irrational. Do not be over-desirous to elicit the fact of insanity according to some pre-conceived plan which you have laid down for the purpose. Allow the lunatic to take his own way of exposing his state of mind; the more the interview assumes the form of monologue on his part, and the nearer his discourse approaches to the form of soliloquy, the better.

Time will not permit me to allude to the

special difficulties that may occur in the examination of lunatics. Here is a common one. You are required to give an opinion on the state of mind of one of whom you know nothing, of whom you can be told little or nothing; that preliminary history is wanting which I have said is of so much convenience. The individual has possibly been found vagabondising, and has been laid hold of by the authorities for some slight offence. There is some reason to think that he was once under confinement as a lunatic, and on this account you are requested to investigate his case in a police cell or the ward of a prison. He is reserved and sullen, replies curtly to questions, but manifests no delusion. You can make little out of him, save that his disposition is by no means engaging, or his intellect very brilliant. In such a case you must endeavour to accumulate some knowledge of him; have him watched and conversed with, and from time to time secretly observed. If you find that he mostly occupies one place and position, that though sullen and dejected he occasionally smiles by himself, avoids his food, that at night he sleeps badly, talks and laughs to himself, stands on the floor, or wanders about his apartment, you have ample grounds for suspecting that he is the subject of some morbid delusion; but as you possess no hint of it, you may have difficulty in getting it evolved. You cannot sound his opinions on all matters of sensation and speculation, but you may chance to jangle the loose string by a judicious venture. I have had occasion to notice, as being more potent than ordinary in conjuring forth a delusion in such cases, questions relative to kindred, bodily health, sexual ideas, poisoning, unseen agency, and nocturnal feelings. There is much room for the exercise of tact, ingenuity, and sagacity, in these cases.

I have known an examiner annoyed and nonplused by the persistent and absolute silence of the person under examination. This conduct may arise from idiocy, insane abstraction, and peculiar delusion, in which instances insanity will be well marked in the aspect and behaviour of the patient. It sometimes is the consequence of mere obstinacy, or of a determination to thwart all attempts at investigation, but in every case it forms, in combination with the testimony presented, a good ground for the belief of the existence of an unsound mind.

The forms of imbecility of mind and moral insanity are perhaps the most difficult of all to investigate satisfactorily; but as we have yet to consider the subjects of civil and criminal responsibility, I shall at present delay no longer on the examination of the insane.

Lunacy Certificates.—The ordinary purpose of the examination of lunatics is to certify, that the condition of the mind is such as to require interference with the patient's liberty for the sake of fitting treatment, his personal safety, and the protection of the community. In Scotland, before an individual can be sent to a lunatic asylum, the fact of mental derangement, together with the necessity of confining the patient, must be certified by an authorized practitioner in medicine. The certificate must be given "upon soul and conscience," and all who are concerned in lodging one who is sound of mind in an asylum, or in compassing the confinement of an insane person in an irregular way, are liable to heavy penalties. Upon such a certificate the nearest relation or recognized guardian of the patient petitions the sheriff of the county to grant a warrant for removing him to a licensed lunatic house. While under confinement he is seen at will by the sheriff or his deputy, accompanied usually by a medical assistant and the procurator fiscal of the district, and he has the power of ordering the liberation of the patient whenever he may deem it unjustly delayed. The minister of the parish has also a right to visit the lunatic asylum, but no power in the way of supervision or inspection. The machinery is therefore simple, economical, and as regards the protection of those under restraint in licensed asylums, not to be charged with being ineffective; but the Scottish lunacy law is very far from being perfect, although from the successful resistance which all the recent attempts at improving it have met, it might be deemed a much more faultless institution than it actually is. Lunatics that are not paupers may be kept anywhere, and treated in any way, by any one. The public hospitals are public in a limited sense, and neither freely available for educational purposes to the profession, nor even for the care and treatment of all classes of the insane. I have known instances of disagreeable or difficult cases being rejected, and a lunatic who has committed a criminal action, and whose safe custody has become a matter of much consequence to the community, is by most of these misnamed public asylums unhesitatingly refused. They are not subjected to adequate government control, but are liable to be under the management of a body of self-elected, irresponsible, ill-chosen directors, nearly as numerous as the inmates. In some cases the hospital is but an appendage to some other institution, instead of being separate and independent, having no governing authority but that connected with the parent establishment, and therefore subject to be ruled as if it were a

prison, an infirmary, or a poor's-house. Patients of various grades are admitted into houses, which in some instances have been originally designed for and are chiefly occupied by insane poor, and are even sometimes rather heterogeneously mixed. It is this proportion of private patients, however small, that constitutes the barrier to the admission of students for the purposes of instruction. The attempt is constantly being made to ape the style of a private asylum, and to increase the number of the better class, to the injury of the full efficiency of the hospital as a public institution, and to the prevention of the establishment of such admirable houses for the better rank of lunatics as are common enough throughout England, but almost unknown in this part of the island.

There is an uncertainty about legal details in reference to lunacy matters in Scotland, which it is desirable should be amended. There is a want of uniformity in the constitution, charges, and arrangements of the hospitals, and if I am not mistaken, in regard to modes of procedure and the forms of warrants and certificates, much is left to the discretionary power of local officials.

The following may be received as an adequate form of a lunacy certificate in Scotland.

(Place and date—the latter in words).
I, A. B., M.D. (or surgeon), hereby certify on Soul and Conscience, that I have this day visited and examined C. D., at present residing at ———, and am of opinion that the said C. D. is in such a state of mental derangement as to require confinement in a lunatic asylum.

(Signed) A. B., M.D. (or surgeon).

In England there must be two medical certificates, unless in the case of a pauper lunatic, when the testimony of one medical examiner is sufficient. These certificates must be signed by individuals not partners together, not relatives of the patient, and having no professional connection with, or other interest in the asylum to which the patient is to be consigned. These examiners have to investigate the case not together, but separately, and must each in his certificate mention the facts upon which his opinion is founded. These documents must be accompanied by a printed order, filled up and signed by the relative or guardian who authorizes the patient's detention as a lunatic. When the lunatic has been received into the hospital he has to be examined anew, and reported upon to the Board of Commissioners in Lunacy. The most important difference betwixt a Scottish and an English lunacy certificate, is the specification of some of the facts on which the opinion is based, in the case of every

lunatic not a pauper, a thing which seems to derogate from the professional character of the document. To give a certificate means, to render a fact sure by the testimony of adequate knowledge; the *ex cathedra* deliverance is by this mode, however, converted into the mere expression of a proposition, and the opinion propounded is not so much a judgment as a condescendence or submission of professional or presumed adequate knowledge to some other, which if surer ought to be at once applied. The medical examiner would appear not to say—this is lunacy; but—is not this lunacy? a function which might be quite as well, and rather more fittingly, discharged by others than “a physician, or surgeon, or apothecary, duly authorized to practise as such.” Much latitude is allowed in regard to the statement of facts: these may be any characteristic symptoms of lunacy, as incoherence in conversation, ungoverned excitement, special delusion, noisy or indecent conduct, alteration of the natural physiognomy, or even facts which have not been observed by the examiner himself, but have been merely represented to him by others, provided this circumstance in regard to them be stated in the certificate. The following is an example of the form adopted.

I, A. B. being a physician (surgeon or apothecary), duly authorized to practise as such, hereby certify that I have this day, separately from any other medical practitioner, visited and personally examined C. D., the person named in the accompanying statement and order, and that the said C. D. is a lunatic, and a proper person to be confined, and that I have formed this opinion from the following facts, the latter being from information derived from his relatives; viz., that he labours under a great despondency of mind, the result of the delusion that he is the victim of a widely extended conspiracy to injure him, and that he manifests a propensity to self-destruction.

Signed A. B.,
(Residence.)

(Date in words.)

In Scotland, where no grounds are required to be assigned for the opinion given that one is insane, and where the community has not the protective supervision of lunacy commissioners, it would be well either that instruction in medical pathology were made a necessary branch of medical education, or that the privilege of granting lunacy certificates should be restricted to those who could declare that they had had the means of studying the disease¹ in some hospital for insanity.

When is mental unsoundness

justified by interdiction?—Interdiction is defined by Briskine, as a legal restraint upon those who, either through profuseness, or extreme facility of temper, are too easily induced to make hurtful conveyances, by which they are disabled from signing any deed to their prejudice without the consent of curators, who are called interdictors, and who are the nearest male relatives on the father's side, if they be of lawful age and otherwise capacitated.

When a lunatic is interdicted he is no longer free to contract agreements, execute legal deeds, or conduct his own affairs. In England, the individual having been certified to be insane by qualified medical examiners, the Court of Chancery is petitioned on this ground to appoint a special commission, to determine whether he be *compos* or *non compos mentis*. This is termed appointing a commission *de lunatico inquirendo*. This commission examines the evidence pro and con, and gives verdict accordingly. In Scotland, the lunacy certificate having been obtained, application is made to Chancery for what is termed a Brieve of Idiocy, or a Brieve of Furiosity, (according as the person is held to be either fatuous or raving mad.) This document is addressed to the judge-ordinary of the district (the sheriff,) and instructs him to summon a jury to determine upon the individual's state of mind, and upon the male relative to be appointed interdictor. The proceedings are instituted by the nearest male relative of proper age, and with the knowledge of the person whose state of mind is the subject of doubt, and are termed “a cognition,” or “cognoscing a lunatic.” The lunatic must be personally examined by the jury previous to their verdict.

Interdiction is justifiable and necessary in all cases of mania, dementia, and imbecility. Cases of difficulty arise in the lesser degrees of insanity, as the earlier stage of mania, the person being not raving mad, but only in a state of more than natural excitement; cases of imbecility, the person being not idiotic, but simply of weak mind, and many instances of monomania. In moral insanity interdiction is presumed to be unnecessary.

In cases of asserted imbecility, take care not to mistake ignorance, from want of education and opportunities of information, for mental defect; in cases of excitement and delusion, be satisfied that the former really interferes with voluntary control, and that the latter is not merely of a speculative and harmless character, but is practically exhibited in insane and ruinous conduct. The mode of investigation must be adapted to each particular case. In imbecility, the individual on whom the question is raised, must have had sufficient education to be capable of understanding the nature of the question.

tion and opportunity to know, comparing his intellect with one cultivated in a similar degree, and if the imbecility be adventitious, the present condition of his understanding with what you can learn of its previous capacity. Test his faculty of memory, his powers of notation, his capacity of being instructed, his knowledge of money matters, of the condition of his affairs, and of the proceedings in progress. Take his moral character into account; see whether or not he have self-reliance and "a mind of his own," or be so facile as to be capable of being led in any direction by the self-interested and evil-designing. Though the capacity of a monomaniac to manage his own affairs is never increased by his delusion, still he may be perfectly competent to manage them, not in an ordinary way merely, but much better than any one else could do for him. Ascertain how his faculties and emotions are exhibited in writing, his ideas regarding matters of religion, morality, property, money, duties, and obligations. Conduct is a much better test than capacity in such cases. Before you can testify that interdiction is necessitated from delusions, be satisfied that they lead into insane and ruinous courses; that they so engross the mind as to render efforts of attention inefficient when directed to other necessary objects; that they lead to neglect of natural duties; that they occasion apathy, indifference, and neglect of worldly affairs, to the lunatic's own loss, and the injury of those naturally dependent upon him, or legally interested in him; or that they are combined with loss of memory, general mental deficiency, or morbid feelings, which lead him to pervert the application of his worldly means. There might be incapacity in business, without there being as a consequence sufficient grounds for interdiction from the management of affairs; mere incapacity to make money, and thereby a livelihood, owing to diseased mind, would by no means form a sufficient plea, although incapacity of keeping, or spending, or using it rationally, might.

Testamentary capacity of the Insane.—For a valid testament there must exist a disposing mind; that is, a mind not oppressed and governed by delusions, in which the judgment is capable of discerning, the memory not clearly impaired, and the emotions not unnatural. No one labouring under mania, amnesia, or dementia, can be deemed to have a disposing mind; but it is presumed that one who is merely imbecile, or labouring under a form of partial insanity, may. Any lunatic may execute a valid testament during a lucid interval of his disease. He is incapable of disposing of his property, if his imbecility of mind is such as to interfere with a right under-

standing of the act, if his delusion obscures his knowledge of surrounding circumstances, and if he labours under any delusion incompatible with right feelings and rational ideas in regard to the disposal of his property. A lunatic who has been interdicted may yet be capable of making a sufficient will, because he may have enough of capacity for doing this, although he have not the degree of mental strength and activity of intellect requisite for this uncontrolled management of all his affairs. Although of feeble mind, the lunatic's imbecility would not be such as on that score to invalidate his will, if he fully and accurately comprehended the extent and nature of his property, if the dispositions in the deed were not only natural but of his own devising, and if he rightly understood the consequences of the document. Nor although the subject of delusion would the lunatic be incapacitated to execute a will, if the provisions in it were rational, in conformity with previous intentions when sane in mind, and independent of the delusion. The character of the deed itself forms usually the best evidence of testamentary capacity. Should a will be written by the patient, without incoherence, irrational, unnatural, or inconsistent provisions, it will be perfectly valid, even although he was at the time undoubtedly unsound in mind, and the inmate of a lunatic asylum. It is not the insanity of the devisour, but the illegality, falsity, or insanity of the deed, that will overthrow it:

EPIDEMIOLOGICAL SOCIETY.

THE arrangements for the working of this Society are proceeding as rapidly as is compatible with the magnitude and extent of its objects. At a meeting of the Council, held at the residence of Dr. Babington, the President, on Friday, Oct. 25, the laws were finally determined upon, and ordered to be printed. The report of the "Objects Committee" (a committee appointed to specify and arrange the objects proposed to be accomplished) was received, adopted, and ordered to be published. A Committee was appointed for general purposes, among which are to be included that of appointing, arranging, and directing sub-committees of special inquiry on various subjects; and it was resolved—"That a Sub-committee be forthwith appointed to investigate the highly important phenomena of small-pox and vaccination." The meeting was well attended, about twenty members of the Council being present, among whom were some of the most distinguished members of the profession.

Actus.

*Oratio Harveiana in Aëthrus Collegii
Regalis Medicorum habita, die Junii
xxix., MDCCCL. A JACOBO ARTURO
WILSON, M.D. Londini, MDCCCL.*

DR. WILSON thus introduces the object of the Harveian Oration:—

"Attendite igitur, socii, hospites, et quicumque huic festo favetis, fratres medici! Nec vereamini, pro hæc saltem vice, aures adhibere in verba, non mei Londinensis, linguæ non mee balbutientis, et Latino pede claudicantis, sed Anglica, Harveiana, ex ore suo ipsius, octogenarii, et, quasi e sepulchro, pro se et suis in omnes ævum loquentis."

"To maintain friendship, there shall be at every meeting, once a month, a small collation, as the president shall think fit, for the entertainment of such as come; and, once in every year, a general feast for all the fellows; and on the day of such feast shall be an oration, in Latin, by some member in commemoration of the benefactors by name, and what in particular they have done for the benefit of the College, with an exhortation to others to imitate, and an exhortation to the members to study and search out the secrets of nature, by way of experiment; and for the honour of the profession, to continue mutually in love."

On which injunction Dr. Wilson remarks, we doubt not with the full concurrence of his audience:—

"Id unum tibi objicio, quod, nata Anglus, in hæc Anglorum concione, pro te, stirpe, moribus, indole (si quis unquam), Anglico, de medicina et medicis Anglicis hæc orationem aggressurus, lingue alienæ, nec mee, nec tuæ, compedibus obstrictus sum."

Without in the slightest degree detracting from the acquirements of the learned members of the College of Physicians, we cannot but express our opinion that it is a real farce that they should, in the present day, be compelled to deliver and listen to topics of present importance in a dead language that has long ceased to be the vehicle for the interchange of thought among learned and scientific men, even of different countries, and it is still more absurd from the mouth of an English orator addressing English physicians!

Dr. Wilson, after alluding to his Har-

veian predecessors bearing his own name, and having paid a tribute of filial respect to his distinguished father, proceeds to discourse—"De rebus hoc proximo anno gestis et factis, equibus aliquid lucri vel damni, honoris vel incommodi, communitati nostro acciderit,—de statu, progressu, et futuris artis medicæ fortunis—de morum inter nosmetipsos disciplinâ et consuetudine."

The first topic to which Dr. Wilson thus refers is one of paramount importance, involving the discussion of conduct on the part of the Executive which has in all probability greatly extended the fatal spread of a fearful pestilence:

"Respice, igitur, in annum vix præteritum, vos, Præses et Socii! qui mecum huic solenni orationi inservitis! In trivis—per vias—per aperta rura—apud ipsas Bænas, qui pover! quantus luctus! quæ frequentia funera! Quæ hæc est immensis morbi species! hæc mortis imago nova? Annus nefastus! Collegio nostro, præ cæteris, heu!—sine gloriâ memorabilia. Annus, in quo, huic urbi insolita et ævis-sima peste diu ingruente, tum poetas universum Angliæ regnum vastante, Collegium hoc Regium Medicorum Londinensium, neque a senatu, neque a consiliariis regniæ intimis, neque a prætoris regio ad res domesticas administrandas designato, ne unâ quidem vice vel voce, in auxilium salutis publicæ vocatum est! Neque in curiâ, neque in fæco, prevalente morbo, vox nostra audita est. A nullis nostri periculum factum est consilii!

"Per orem sanguinem Harveii! Quid est, si hoc non contumelia est!"

We have ourselves on several occasions expressed the strongest disapprobation of the usurpation of the functions of the College of Physicians by a "Board of Works," whose self-sufficiency did not permit them to seek the advice of the body the best qualified to give opinions on matters on which they showed themselves ignorant. We therefore now cordially welcome Dr. Wilson's indignant remonstrance upon the general neglect and ingratitude with which the labours of the profession in the treatment of cholera, have been rewarded.

Dr. Wilson pays a just tribute to the memory of Dr. Burton, of St. Thomas's Hospital, who was one of the earliest and most lamented victims of cholera. Dr. Burton's investigations on the action of lead on the system are duly noticed by the orator.

The loss which the body medical and the world at large (if it would but learn to know the true value of legitimate medicine) has suffered in the death of PNOX, is strongly and eloquently enforced. Dr. Wilson has put the debt of gratitude due to this father of pathological chemistry in as forcible language as he could command; but, in our estimate, words cannot express it.

The evils of quackery are ably exposed by the orator. The gross and insulting treatment of naval surgeons does not pass unscathed.

In recording the merits of Harvey and Lumley, the orator does not forget the labours of living investigators who, following the example of Harvey, devote their energies to the investigation of the blood in health and disease. The character of Harvey's researches is compared and contrasted with the medical inquiries of the present day. The present state and prospects of the College also receive notice. Dr. Wilson concludes in the following words:

"In nostra republica, quid verum est, sit pro re—pro imperio, religio—pro lege suprema, populi salus! Sit unitas in communitate—in communitate unitas!"

"Ad hæc tuenda et conservanda sit nullum inter nos iurandum!—si ullum, in verba hæc:—Experientia!—Amor!—Amicitia!—verba ipsius Harvey—verba omnium ætatum, temporum, laborum! Leges, mores, etiam diplomata regis, humana omnia mutantur. Amor et amicitia a Deo sunt, et manent."

"Veniam date!"

To praise the latinity of this oration is superfluous; its merits were acknowledged by its hearers, and will in no way be disputed by its readers.

Remarks on the Comparative Value of the different Anæsthetic Agents. By GEORGE HAYWARD, M.D., one of the Surgeons of the Massachusetts General Hospital. Pamphlet, 8vo. pp. 11. Boston, U.S. 1860.

THE agents examined by Dr. Hayward are ether, chloroform, and chloric ether. The author decides in favour of ether. "The advantages, then (he observes), of sulphuric ether, as an anæsthetic agent, are its entire safety, the ease with which it is administered, and the slight inconvenience which follows its administration." Dr. Hayward further

states that, after a careful examination of some of the leading medical journals of Europe and America, he has found only one case of death by its inhalation, and that, took place at Auxerre, in France, soon after its discovery, and when the best mode of administering it had not been adopted.

Of chloroform the author observes that its "only advantages are that it is more agreeable to inhale than ether, and that a smaller quantity answers the purpose." On the other hand, he points out that its injurious and fatal effects have been by no means unfrequent.

Of the truth of this assertion our British, as well as the American and Continental journals, can bear lamentable testimony. "With regard to chloric ether," Dr. Hayward very justly observes "that it derives its power of producing insensibility from the chloroform, it contains, and it is difficult to understand how the addition of alcohol can deprive it of its dangerous properties, when it is well known that the mixture of this substance with sulphuric ether, renders it in a great measure unfit for inhalation." Under this impression the author concludes that chloric ether is an unsafe anæsthetic agent.

This short essay deserves an attentive perusal by all who have recourse to anæsthetic agents.

The Anatomical Remembrancer. 4th edition. 12mo. pp. 240. London: Higley. 1860.

FOR the sake of those gentlemen now pursuing their anatomical studies at the medical schools, we lose no time in calling attention to the publication of a fourth edition of this useful little pocket Remembrancer. There is no reason why it should not be always the companion of the student. A glance at it during any leisure half hour,—if such an event really occurs amidst the hurry of lectures and practice,—will not only aid recollection, but impress facts more strongly on the mind, and suggest new subjects of study. The little book is published anonymously; but the fact that it has reached a fourth edition is a very strong point in its favour. It is written with conciseness and clearness, and, as far as we have examined it, with correctness.

Annual Report of the Progress of Chemistry and the Allied Sciences. By JUSTUS LIEBIG, M.D., and H. KOPF. Edited by A. W. HOFMANN, M.D., and WARREN DE LA RUE. Parts IV and V, 1847-8. London: Taylor and Walton.

We are glad to welcome two more numbers of this valuable retrospect of chemical science. No fewer than ten men of scientific eminence are concerned in the preparation of this Report; and from this division of labour, it is easy to perceive that all branches of chemistry have received due attention. We have looked through the two parts now before us, and we can recommend them to all lovers of chemical science as essential to the acquisition of a due knowledge of the progress made during the last two years.

Pathology of the Human Eye. By JOHN DALRYMPLE, F.R.C.S. Fasciculi V. and VI. London: Churchill.

It is some time since we have had an opportunity of noticing this valuable publication, in the production of which artistic skill and practical information are admirably combined. The two fasciculi before us comprise no fewer than forty-seven illustrations of various diseased conditions of the eye. The drawings are as well executed and as faithfully coloured as any which have been published in the preceding fasciculi. In plate 19 there is a coloured drawing, representing the effects of Iritis on the globules of the eyes, which is so skilfully treated that it has quite a stereoscopic appearance. The pathological conditions represented in these two parts are, *Aquo-Capsulitis*, *Iritis*, *Congestion of the Choroid*, and *Glaucoma*. We entertain no doubt that they will keep up the character of the work, and give satisfaction to the subscribers.

Contributions to the Physiology of the Alimentary Canal. By WILLIAM Brinton, M.D. London. Pamphlet, 8vo. pp. 27. London. 1849.

THE contents of this pamphlet have already appeared in our pages. The author has increased the utility of his "Contributions" by their republication in a separate form. They will be found to afford many useful practical hints to medical practitioners, by whom, we trust, they will be generally read.

Surgical Anatomy. By JOSEPH MACLISE, F.R.C.S.E. Fasciculi VI. and VII. London: Churchill.

IN these two parts Mr. MacLise presents us with coloured lithographic drawings—1. Of congenital and infantile inguinal hernia and hydrocele. 2. Of inguinal hernia in general. 3. The dissection of femoral hernia, its origin and progress, the seat of stricture, &c. 4. The surgical dissection of the femoral region. 5. The relative anatomy of the male pelvic organs. And, 6. the surgical dissection of the perineum. All these drawings are executed with anatomical correctness, and are well calculated to convey to the student of surgery a large amount of valuable instruction. The descriptive letterpress is appropriate. Another recommendation is, that it is the cheapest illustrated publication on surgical anatomy which has ever appeared.

The Metropolitan Interments Act; with Explanatory Notes, an Appendix, and Index. By H. H. OLIPHANT, of Trin. Col. Cam., B.A., and of the Inner Temple, Barrister-at-law. 8vo. pp. 100. Longmans. 1850.

THIS is a very convenient and useful publication at the present time. We commend it to all who wish to make themselves acquainted with an Act of Parliament which introduces one of the most important sanitary improvements of the age.

Deafness Practically Illustrated; being an Exposition of Original Views as to the Nature, Causes, and Treatment of Diseases of the Ear. By JAMES YEARSLEY, M.R.C.S.E., &c. &c. 3d edit. 8vo. pp. 244. London: Churchill; Highley. 1850.

WE need scarcely on the present occasion do more than reiterate the opinions which we expressed when reviewing the former edition of this work in the year 1847.

We may state that the author attributes nearly all diseases of the ear to morbid conditions of the mucous membranes of the throat, nose, and ear. Mr. Yearsley "holds all works on diseases of the ear to be of little value wherein reference to the mucous membrane is not imprinted on almost every page."

The work comprises many valuable

practical suggestions. It is addressed to the public as well as to the profession: the latter will doubtless prove the better judge of its merits.

A Plea of Humanity in Behalf of Medical Education. The Annual Address delivered before the New York State Medical Society, and the Members of the Legislature, at the Capitol, February 6th, 1849. By ALEXANDER H. STEVENS, M.D., President of the Society. Pamphlet. 8vo. pp. 47. New York.

In this eloquent and energetic address we have brought before us the claims of the profession of medicine in return for what it has done for the benefit of man in all aspects and states of society, and the high and honourable spirit in which it has been practised by its brightest ornaments, is ably set forth. On these considerations is grounded a powerful appeal for the encouragement of the best and most liberal education of its members. Though its illustrations belong for the most part to American history, this appeal may be read with interest in the parent country.

The Eleventh Annual Announcement of the Baltimore College of Dental Surgery. 1850.

DENTAL SURGERY would seem to be in a far more flourishing condition in the state of Baltimore than in England; for here we find a college with a provost, and professors of principles and practice of dental surgery; of special pathology and therapeutics; of anatomy and physiology, or of operative and mechanical dentistry; and a lecturer on dental chemistry!

It will excite no great surprise to find that the "faculty" of the Baltimore College confess that their classes are by no means so large as they had reason to expect.

Appended to the Report are two excellent Addresses, delivered to the "Gentlemen Graduates of the Baltimore College of Dental Surgery" by two of their professors: they contain very good injunctions on the regulation of conduct, and place before the "graduates" the highest motives for their guidance in practice.

Proceedings of Societies.

MEDICAL SOCIETY OF LONDON.

Oct. 19, 1850.

DR. J. R. BENNETT, PRESIDENT.

Valvular Disease of the Heart.

DR. R. H. SIMPLE, the author of this paper, commenced by observing that the diagnosis and pathology of diseases of the heart, although formerly wrapped in great obscurity, were now rendered remarkably exact and definite by the labours and researches of modern investigators. The healthy sounds were now referred to their true causes, and the morbid ones were accurately connected with their true pathological phenomena. But he considered that it might be a question, whether too much stress were not laid at present upon the stethoscopic indications in forming a prognosis, and whether or not too little regard were paid to the attendant circumstances and general health of the patient. He by no means denied the importance of cardiac diseases, especially of the diseases of the valves; but it was a curious circumstance, that notwithstanding both theory and experience taught us the extreme danger of such diseases, yet clinical investigation showed that very extensive disease of the cardiac valves *might* exist without seriously impairing the health or greatly abridging the usual term of human existence.

The author read the history of three cases which had occurred in his own practice: the first was that of a man who died at the age of seventy, without manifesting any of the usual severe symptoms of cardiac disease, although the stethoscopic indications showed very clearly extensive disease of the mitral and aortic valves; the second case was that of a lady, with whom the author was acquainted for more than ten years before her death, and who enjoyed during that time almost uninterrupted good health, and was actively and daily engaged in the superintendence of a large family: she died suddenly, at the age of seventy. In the third case, which was that of an infirmity patient, the author was acquainted with the existence of serious valvular disease for ten years before death: yet she suffered very few of the symptoms of cardiac disease, and died of pleuro-pneumonia, at the age of eighty-three. In all these cases post-mortem examinations were made, and most exten-

sive and serious cardiac lesions were discovered, consisting of an ossified condition of the aortic valves, rigidity of the mitral valves, and a dilated, ossified, and rigid condition of the great arteries. Dr. Semple considered, that whatever explanations might be offered to account for the comparative immunity of these patients from severe symptoms during life, yet the facts themselves were curious and instructive; and he thought he might appeal to practical observation to prove, that however important might be the diseases of the valves, and however valuable might be the information derived from the use of the stethoscope, these diseases were not so rapidly and invariably fatal as they were generally supposed to be. In forming a prognosis, however cautious and guarded it might be, facts such as he had brought before the notice of the Society should be held in view in forming an opinion.

LIVERPOOL MEDICAL AND PATHOLOGICAL SOCIETY.

Abscess of the Liver.

MR. PATERSON, after describing the general symptoms of acute hepatitis, proceeded to illustrate its various stages by the following cases:—

The danger in any way of deviating from proper diet during the progress and treatment of acute hepatitis need not be better exemplified than in the following case:—

The patient, though tolerably submissive to medical treatment in other points, was totally intractable on the subject of diet. Colonel P— had taken his passage to the Cape of Good Hope on board my ship. He was advised by his medical attendant to live on board from the time he came down from the country, where he had suffered from gastro-enteritis and hepatitis. On coming on board he was much exhausted and very pale, pulse weak, tongue white, belly tense, liver very hard, and he had a troublesome cough: the bowels were costive.

He was washed with hot water, while he remained in the recumbent position, and had an enema. In the course of the day he took ℥j. of calomel, with ʒss. of compound extract of colocynth, followed by ʒi. of compound powder of jalap, which produced only two stools. After a few hours' rest his pulse had risen slightly; and, though there was but little morbid heat, it was considered requisite to take 20 ounces of blood from the arm; and he had calomel.—Ext. Coloc. Ca., Pulv. Scam.

Ca., aa. ʒss., which were given in syrup, as he refused to take any more pills.

Oct. 23d.—He was purged freely, and felt better; the cough was moderated; the belly was generally softer; but the right lobe of the liver was hard, large, and tender if pressed. Pulse soft and natural.—℞ Calomel., Extract Colocynth. Comp., aa. ʒss. at 7 A.M.; Repet. V.S., ad ʒi. in the morning; 12 leeches to be applied to the liver at noon. At 4 P.M. he had only one scanty and light-coloured stool; after which he was seized with spasm in the belly, and some symptoms of cholera, attended with great anxiety, a soft pulse, and the tongue cool. He took calomel, ℥j.; Opii, gr. ii.; after which he became easy, and slept well all night. The leech-bites were bleeding freely next morning. The patient's tongue was warm, moist, and white. There was no pyrexia, and the apprehension of cholera had subsided. Leeches were applied to the belly daily for a week; and active purgatives with calomel were taken daily.

Nov. 13.—The belly had become less tumid, and softer; the tumour of the liver more distinct and circumscribed; the mouth was sore, and he felt weaker. The pain of the right side was subdued (at least, he did not acknowledge any pain on pressure); the enlargement of the liver remained, but it was softer.

After this a blister was applied to the right side, which was kept discharging by the savine oerate; a small number of leeches was applied daily to its surface; and liniment hydrarg. was rubbed over the belly and chest where the blister had not reached. The patient now became low-spirited, and said he was determined not to die of debility, and that, unless an unrestricted quantity of food were allowed, he had resolved to procure it. In this he was as good as his word; for, by the aid of his servants, he was well fed, and had no wish to conceal that fact. The first unfavourable symptom which arose from improper diet was a return of cough, then gradually increasing emaciation, and a sharpness of visage, followed by dysenteric symptoms, with blood in the stools. He lost all hope, and refused to take any remedial; the evacuations assumed the appearance of dark brown water, mixed with blood, and had a putrid odour: he was unable to rise to stool. Still the pulse did not exceed 86 when he was in the recumbent posture. He died on the 8th of December, 1842.

Dissection, nine hours after death.—Subject emaciated; universal adhesions of the right lung, apparently not recent. The liver was much enlarged, and of a pale drab colour; its structure was indurated; the convex surface of the right lobe was

extensively adherent at the space between the 7th and 8th ribs, and at that part contained a large abscess very near the surface; the margin of the right lobe adhered to the colon. There were numerous small ulcers in the colon and rectum. The mesenteric glands were enlarged, and the cellular texture at the root of the mesentery was consolidated. It is not common to find abscesses of the liver, as in this case, combined with the pale degeneration. Up to the 18th of November there appeared room to hope that the disease might have been cured, the rules of diet being attended to, and persistence in a correct treatment, so as to promote absorption.

In the only case where I have seen a superficial abscess of the liver exactly under the ligamentum latum, the patient was unable to lie on either side, but was obliged to rest on his back, with the shoulders very little raised. This patient was an emaciated European, a captain in the Engineers. He had resided about twenty years in India. He embarked on board my ship on our homeward passage, on the 9th of January, 1844. He was brought on board in his bed in the lowest state of misery and distress, having been ill with a purging for nine weeks, and voided much blood, part of which was in coagula. His pulse was 104, and weak: he was very feeble, and unable to rest on either side. There was a fulness of the epigastrium, inclining to the right side. Fluctuation was very distinctly marked. From the time he came on board, he urgently begged me to puncture the liver, which I at last reluctantly consented to do, owing to the weak state of the patient: his appetite kept very good. We had been nine days at sea, and he thought himself better; so I introduced a trocar into the most projecting part of the liver. There was a great discharge of purulent matter, fully two pints. The wound continued to discharge a considerable quantity for some time, my patient improving in health and strength daily. He was soon able to be carried upon deck; and what with nourishing diet, the sea breeze, and careful nursing, he improved rapidly; and in six weeks from the operation was well, and the wound closed. He continued well during the remainder of the voyage; had no pain in the liver; and died about two years after this time from inflammation of the brain.

A lady, of rather sallow and unhealthy aspect, after residing seven years in India, during which time she had suffered from dysentery, continued fever, and at last acute hepatitis, was recommended to England for change of climate. When she came on board she was pretty well, but slightly thinner than usual, she ate reasonably; but

soon after coming on board had some difficulty and oppression in breathing, and pain in the right side, which increased slowly. A few leeches were applied once, and mercury given until the mouth was sore. Eighteen days after embarking a troublesome cough came on, and on the 25th the pain in the right side grew worse, a slight degree of bloody expectoration appeared. On the 26th day, she suddenly experienced a sense of suffocation and extreme faintness, and coughed up about sixteen ounces of rather thick, viscid, puriform, grey-coloured matter. Hectic symptoms came on, and she became low and weak, but the pain at the right side ceased. Her appetite was pretty good, and she in a short time improved in health, and made a very slow and protracted recovery.

This lady remained three years in England, and quite recovered her health, and went to India to join her husband. I heard from her two months ago, and she was then well, and has had one fine healthy child since she returned to Bengal.

ACADEMY OF MEDICINE, PARIS.

Oct. 15, 1850.

Nephritic Amaurosis.

M. LIANDOUX transmitted a second memoir on this subject, with the following conclusions:—

1. Disorders of the sight are an almost constant symptom of Bright's disease of the kidney.
2. Nephritic amaurosis cannot be attributed to loss of power (*deterioration des forces*).
3. It often appears before other pathognomonic signs of the disease.
4. It appears, disappears, and returns, without following exactly the phases of the presence of albumen in the urine, and of the cedema.
5. Nephritic amaurosis must be regarded as a deranged state of the ganglionic nervous system.

Inhalation of Carbonic Acid in Phthisis, and a new Auscultatory Sign of the presence of latent Tubercles.

M. GRISOLLE read a report on a memoir by M. Le Goin, in which the author states his opinion that carbonic acid inhaled, freely mixed with air, produces a beneficial excitement in the lungs. The reporters do not concur in the views on the subject expressed by Dr. Le Goin.

The new auscultatory sign of latent tubercles which the author had observed was the occurrence of a crepitation heard with

the first inspiration immediately on waking, in the morning. M. Grisolle observed on this point that the phenomenon was purely physiological, the result of the less expansion of the air-cells during sleep, and that it could not be regarded as an indication of disease.

Etiology of Tuberculization.

M. PIERRE read an essay by M. Wanner, in which the author attributes the production of tubercle to the presence of lime in the soil of the district in which the patients reside, and stated that at Sologne, where the soil to a great depth consists entirely of silica and alumina, he had never met with a case of the disease.

SURGICAL SOCIETY OF PARIS.

Oct. 16, 1850.

Remarkable Gun-shot Wound.

M. HUGUIER was called to a patient who had received a gun-shot wound in the axilla under the following circumstances:—A man, 40 years of age, of a good constitution, had been shooting with a badly-made double-barrelled flint-lock fowling-piece. One of the barrels having been discharged, the imprudent sportsman proceeded to reload it, having puf in the powder, and rammed it down with a wooden rod; leaving the rod in the barrel, he was feeling in his pocket for the shot, when by some accident the same barrel was discharged into his axilla. The ram-rod traversed the axilla, shattering itself against the humerus into twenty-one pieces, without fracturing the bone. No hæmorrhage occurred. It was impossible to ascertain to what extent the articulation had been opened. The patient perfectly recovered. The twenty-one fragments of the ram-rod were exhibited by M. Huguier.

Cancer of the Tongue—Sudden Death from Oedema of the Glottis.

M. MOREL-LAVALLÉE presented a pathological specimen of the parts from a case in which cancer of the tongue terminated fatally by oedema of the glottis and larynx generally.

M. LENOIR exhibited a large calculus removed from Wharton's duct.

ACADEMY OF SCIENCES, PARIS.

Oct. 14, 1850.

Experimental Researches on the Woorara Poison.

M. BERNARD read the following as the results of experiments performed by him-

self and M. Pelouze on the woorara poison, prepared by the tribes inhabiting the districts of Upper Orinoco, Rio-Negro, and the Amazon:—

The woorara is a watery extract prepared from a plant of the *Strychnos* family. The poison acts instantaneously when introduced into the blood-vessels. A weak solution injected into the jugular veins of dogs caused sudden death, without producing cries or convulsive movements in the animal; the animal is, as it were, struck dead, and every trace of life vanishes as quickly as a flash of lightning. When introduced into a wound beneath the skin the poison acts more slowly, varying with the dose and the size or species of animal. *Cateris paribus* birds die most speedily, then mammifera, then reptiles. In every case the signs of poisoning are similar,—the animal will move about as usual for a brief interval, and then lie down and die without a cry or a struggle. Immediately after death the nerves of the animals are as inert and insensible to stimulation as if the animals had been long dead and cold. The blood is black, and does not readily coagulate nor redden on exposure to the air.

The poisonous effects of woorara present a close analogy with those of the bite of a viper, and, like that venom, is innocuous when taken into the digestive tube. That its properties are not then destroyed by digestion was made evident from the experiment of inserting some gastric juice into a wound, forty-eight hours after the poison had been taken into the stomach: the usual poisonous effects were manifested. Thus the strange spectacle was exhibited of an animal carrying in its stomach a substance which in no way interrupted digestion, but which, if taken from its stomach, is capable of inflicting death upon itself or any other animal.

This fact was explained by the discovery that the woorara is not absorbed from the alimentary mucous membrane. So long as the mucous membrane retains its integrity the woorara solution does not pass through the endosmometer. Other mucous membranes present the same results with the woorara: those of the bladder, nasal fossæ, and eyes, were experimented upon. The pulmonary mucous membrane presented the only exception. A few drops of the solution introduced into the air-passages produced the same morbid effects, and the same rapidity of death ensued as if it had been inserted beneath the skin. This exception is referred by M. Bernard to the circumstance that the mucous membrane of the lungs has not the same mucous secretion as is supplied to the other mucous membranes.

Caseine in the Blood of Women during Lactation.

MM. NATALIS GUILLLOT and FELIX LEBLANC announced that they had succeeded in separating caseine from the blood of women, and stated that they should take an early occasion of submitting their experimental researches.

Medical Intelligence.

THE USE OF CHLOROFORM BY THIEVES.

[THE following case, communicated by a correspondent, will show that some restriction on the sale of chloroform to the public is imperatively required.]

A gentleman named Mackintosh had retired to bed at an hotel in Kendal. He was awoke about twelve by a man attempting to suffocate him by means of a rag steeped in chloroform. Mr. Mackintosh, who is an elderly man, struggled desperately with his assailant; but, whether from the fumes of the chloroform, or the disadvantage at which he was taken by his midnight assailant, he felt himself fast fainting, when his cries of "Help! murder!" roused the house. When the landlord made his way into the room Mr. Mackintosh was almost powerless, and the assassin, or robber, was lying upon the bedding, which had fallen upon the floor in the scuffle, apparently sound asleep. On being roughly shaken, the latter professed that he had long been a sleep-walker, and appeared to be astonished to find himself where he was. A policeman was sent for, and the man taken into custody. A strong smell of chloroform was perceived by the parties who entered the room upon the alarm being given, and a bottle containing chloroform was found under Mr. Mackintosh's bed, and a similar bottle in the carpet-bag of the prisoner, who had been at the hotel since Saturday evening. The reverend gentleman's face bore strong marks of the pertinacity of his assailant, and, upon the landlord and the landlady entering the room, his night-dress was found to be covered with blood. There had been no key in the lock of his room, and he had placed a chair between the door and the bed previously to retiring to rest. This chair the people who entered the room on his cries being heard had to remove with some difficulty; so the probability is that the ruffian had secreted himself under the bed. The prisoner, who was brought up next day, it was proved, had retired to bed about half an hour before

Mr. Mackintosh. In the course of the evening he had taken occasion to tell the people of the house that he was a sleep-walker, and on one occasion he said he had walked four miles in his sleep. To the policeman who took him into custody he said he was a traveller, but refused to say for whom he travelled, or in what business. He was fully committed for trial.

. The *lex talionis* should be applied here. It would be only fair to give to this "Thug" in the disguise of an Englishman, such a dose of chloroform as would prevent him from making a similar attempt. Coroners and medical practitioners should be prepared for cases of this kind, since all persons have ready access to this powerful poison.

STATE OF THE PUBLIC HEALTH.

THE Quarterly Return of the Registrar-General announces that the mortality is much below the average, and the public health has never been so good since 1845 as in the present quarter. The rate of mortality is 1·901 per cent. per annum. At this rate, one in 211 persons living, died in three months. The chances of living through this quarter were 210 to 1; the average chances of living through three summer months (1849-50), for persons of all ages, being 192 to 1.

SALE OF POISONS IN FRANCE.

A GOVERNMENT decree of the 18th July had specified the following as substances to be kept and sold with especial precautions:—Hydrocyanic acid; the poisonous vegetable alkaloids, and their salts; arsenic and its preparations; belladonna, cantharides, chloroform, conium, cyanide of mercury, cyanide of potassium, digitalis, hyoscyamus, tartar emetic, nitrate of mercury, nicotine, opium, phosphorus, ergot of rye, stramonium, and corrosive sublimate.—*L'Union Médicale*.

X

A FRENCH SUMMARY OF THE LONDON MEDICAL SCHOOLS.

OUR contemporary the *Gazette Médicale*, in giving a summary of the London Medical Schools, places as third on the list the Lutheran Institute of Medicine and Surgery. The honour of the name is, by a trifling mistake, taken from John Hunter, and, curiously enough, assigned to Martin Luther! Of the 150 Professors which are said to be attached to the London schools, a selection is made of those who are best known to fame. Under the School of St. Bartholomew's, we find specially mentioned a M. Bunsen, whom our readers would hardly recognise as Dr. Burrows!

UNIVERSITY OF ST. ANDREWS.
MEDICAL EXAMINATION PAPERS, OCT. 1850.

First Examination.

To be translated into English:—

STATUS homo, qui et bene valet, et sua spontis est, nullis obligare se legibus debet; ne neque medico, neque intralipia egere. Hunc oportet varium habere vitæ genus: modo ruri esse, modo in urbe, sæpiusque in agro: navigare, venari, quiescere interdum, sed frequentius se exercere. Siquidem ignavia corpus hebetat, labor firmat; illa maturam senectutem, hic longam adolescentiam reddit. Prodest etiam interdum balneo, interdum aquis frigidis uti: modo ungi, modo id ipsum negligere: nullum cibi genus fugere, quo populus utatur: interdum in convietu esse, interdum ab eo se retrahere: modo plus justo, modo non amplius assumere: bis die potius, quam semel cibum capere, et semper quam plurimum, dummodo hunc concoquat. Sed ut hujus generis exercitationes ubique necessariæ sunt; sic athletici supervacui. Nam et intermissus propter civiles aliquas necessitates ordo exercitationis corpus affligit: et ea corpora, quæ morte eorum repleta sunt, celerrime et senescunt, et agrotant.

1. State the ultimate composition of Water, Muristic Acid, Oxalate of Lime, and Acetate of Potash.

2. What are the compounds of Chlorine with Oxygen?

3. Describe the ordinary tests for Arsenious Acid.

4. What are the usual methods of preparing Calomel, Tartarate of Potash, Tartar Emetic, and Prussic Acid?

5. Mention the most important preparations of Iron, the cases in which they are specially applicable, and the mode and dose in which they should be given.

6. How do Digitalis and Colchicum act, when given in ordinary and in poisonous doses? In what diseases, and in what forms and doses, may they be administered? Write a Latin prescription (without using abbreviations) for a diuretic mixture containing Digitalis and Squills.

7. What is Ergot? Mention its most important therapeutic uses, and the best modes of administering it?

Second Examination.

1. Describe the chemical and microscopical characters of healthy Venous Blood. How are the Corpuscles of Blood, Pus, and Milk to be distinguished from one another?

2. Describe the structure of the Lungs.

3. Mention the different parts into which the Small Intestine is divided, and describe its general structure. How can we

determine to what division the isolated portion of gut belongs?

4. Describe the course and the most important communications of the Facial Nerve. What is its function? Mention the most striking effects resulting from its paralysis.

5. Describe the course of the Spermatie Cord, and the parts which enter into its formation.

Third Examination.

1. What are the differences between Endemic and Epidemic Diseases? What are the most probable hypotheses regarding the causes of Epidemic Diseases?

2. What are the physical signs and general symptoms of the different stages of Pulmonary Phthisis? With what diseases is it liable to be confounded? Give a sketch of the treatment to be adopted, (1.) to prevent the disease when it is likely to occur, (2.) to arrest its progress when it is incipient or limited in extent, and (3.) to alleviate the most distressing symptoms in hopeless cases.

3. Give the symptoms, diagnosis, and treatment of Carcinoma of the stomach. Which variety of Carcinoma most commonly affects this organ?

4. Describe the Oxalic Diathesis, and the proper treatment of it. In what form or forms does Oxalate of Lime appear in the urine, and how do you distinguish it from Uric Acid, and from Ammonio-Magnesian Phosphate?

5. Give the distinguishing characters, causes, and treatment of Urticaria, Prurigo, and Psoriasis.

6. What is understood by Caries? Mention its causes, and give a brief outline of the treatment to be pursued.

7. Give the symptoms and treatment of Dissection Wounds.

8. Describe the causes and treatment of Tedium Labour.

List of Gentlemen who had the Degree of Doctor of Medicine conferred upon them, Oct. 18th, 1850.

J. Ainsworth, M.R.C.S.I., Antrim, Ireland.
A. T. Brett, M.R.C.S.E., L.A.C., Surrey.
E. B. Dorman, M.R.C.S.E., L.A.C., Kin-sale, Ireland.
A. Eves, F.R.C.S.E., L.A.C., Cheltenham.
J. Furse, M.R.C.S.E., L.A.C., Devonshire.
H. C. Rooda, M.R.C.S.E., London.
W. H. Woods, A.B., Trinity College, Dub.-
Dublin.

RENCH MEDICAL JOURNALISM AND THE
LAW OF THE PRESS.

Is that glorious land of liberty, equality, and fraternity,—France,—it is now the law that no leading article, or any article

whatever, can be inserted in a *medical* periodical without the name of the author being attached to it, under a heavy penalty of fine and imprisonment. Our contemporaries are politely indignant at this tyrannous exercise of the censorship; but, as it is one of the results of "*vole by bullet*," they are compelled to submit to it.

PREVENTION OF BURIAL CLUB MURDERS.

In the new Friendly Societies' Act there is a provision respecting burial clubs. In future, the money to be paid on the death of a child under ten years old must be under £8, and can only be paid to the undertaker for the actual expenses incurred. No money must be paid on the death of a husband, wife, or child enrolled, except on production of a certificate from a physician, surgeon, apothecary, or seer, according to a form annexed to the Act, that the party had not been deprived of life by any person interested in obtaining the burial money. If any officer of a society pay the money without such a certificate, he will be liable to a penalty of £10, one half of which will go to the informer.

SOCIETY FOR RELIEF OF WIDOWS AND ORPHANS OF MEDICAL MEN IN LONDON AND ITS VICINITY.

A HALF-YEARLY General Court of this Society was held on Wednesday, Oct. 23, at the Gray's Inn Coffee-house, Holborn, Sir Charles Mansfield Clarke, Bart., M.D., the President, in the chair.

The annual ballot for the election of the Court of Directors took place; and, in the room of the six senior directors who retire in rotation, the following gentlemen were elected—viz., Dr. Cape, Dr. A. J. Sutherland, Messrs. Beaman, Lane, Clifton, and Bird.

Thanks were voted to the auditors, and to the acting treasurer, Dr. W. Merriman.

The President announced that, in consequence of the lamented death of the patron, he had, in compliance with the wish of the Court of Directors, written a letter of condolence to H.R.H. the Duchess of Cambridge. This letter, with its answer, was read, and contained a judicious allusion to the merits of the medical profession.

It was resolved that H.R.H. the present Duke of Cambridge be respectfully invited to become the patron of the Society.

At the conclusion of business, the President very feelingly called attention to the fact that the Society's grants to widows and orphans of deceased members, amounting to £780 for the current half-year, are derived almost entirely from the members of the Society themselves, and not from the public, who were too apt to forget the services of the medical profession when not actually needed.

OBITUARY.

ON Friday, the 25th ult., at the residence of his father, at Enfield, Middlesex, Dammett Asbury, Esq., M.R.C.S., and L.S.A. (1848), aged 23, after three days' illness, from the poisonous effects of a wound received at a post-mortem examination.

On the 27th ult., at Ipswich, John Ransom, Esq., surgeon, aged 33.

Selections from Journals.

EXAMINATION OF THE DEAD FOR JUDICIAL PURPOSES. DEATH BY DROWNING. BY DR. CASPER.

THERE is at the present time scarcely any greater desideratum in medical jurisprudence, than a sure criterion for the determination of the fact, whether a person has been drowned, i. e., has died in the water. The best authors differ widely in their decision of the question. How often is it disputed whether a person found drowned has died of apoplexy or strangulation; whether the frothy mucus found in the trachea be a sign of death by suffocation; what is the true value of the *cutis asserina*, the fluidity of the blood, and many other uncertain signs. From the examination of a great number of bodies of persons drowned, Dr. Casper comes to the conclusion that no certain or constant signs are met with in death from drowning.

It may generally be admitted that a person has perished in the water, if no trace of apoplexy, or disease of the lungs, heart, or brain be present; if the blood be dark and fluid, the mucous membrane of the air-passages injected of a light red colour, the larynx and trachea containing more or less of a sanguineous mucus; if on particular parts of the body, more particularly on the shoulders and forearms, the *cutis asserina* be very distinct; and lastly, if, in addition to all these signs, there be added the negative evidence of the signs of any other mode of death. Putrefaction, however, often vitiates all the proofs afforded by the state of the body. Both the condition of the blood itself, and of the internal organs, undergoes great change, and the state of the skin no longer affords any indication. Dr. Casper's predecessor, Dr. Wagner, observed the *cutis asserina* formed in several instances while the body was yet warm. This phenomenon is rarely met in men of dense strong fibre—as labourers, who during life have not a very delicate skin, and in whom after death the integuments exhibit a carnosous state. Where putrefaction has proceeded to the elevation of the

cuticle in vesicles, *cute anserina* cannot be present. In nearly all putrified bodies, the proof of drowning can only be established on negative evidence.

The following cases are related by Dr. Casper, in illustration of his observations:—

The dead body of a child, two years of age, was found in the water. There was slight congestion of the brain. No water was found in the trachea or bronchi, although the glottis was open; the lungs were exsanguineous, and the cavities of the heart were absolutely empty: there were no indications of either apoplexy or suffocation. The blood was of a light red colour, and unusually fluid. The stomach was filled with water, in which floated fragments of meat. No signs of any other mode of death could be discovered, and therefore from their absence, rather than from any positive proof afforded by the examination, the probability of death by drowning was admitted in this instance. The unusual circumstance of the stomach being filled with water was afterwards explained. The child had been playing on a hot summer's day at the water's edge, and being thirsty the nurse had given it a large draught of water, which was drunk with eagerness. The nurse left the child for a short time, and on her return found that the infant had fallen into the water and was drowned.

The following case is here introduced chiefly for its singularity:—

A servant girl disappeared on the 21st of March, 1841. It was reported that she was pregnant, and her disappearance was considered not to be accidental. All search proved unsuccessful, and the case was almost forgotten, when, in December of the same year, nine months afterwards, the cesspool of the house being emptied, the workmen found a putrid human body therein. It was immediately suspected to be the body of the missing servant-girl, and a judicial inspection was forthwith ordered. Dr. Casper states that he had never seen a corpse in a higher degree of putrefaction, as might be supposed after nine months' maceration in human excrement. Even the hardened attendant of the dissecting-room experienced nausea from the indescribable stench which this corpse occasioned.

The skull, the lower jaw, the lower extremities, were for the most part denuded of their soft coverings; the ligaments of the joints were partly exposed; the external sexual organs were not recognisable: what soft parts remained were converted into greyish black shreds. To the question, put officially, whether it was not possible to ascertain the fact of pregnancy in this case, Dr. Casper answered affirmatively, provided that the rudiments of a fetus were discovered

in the uterus. The abdomen was opened for this purpose. The muscles were found converted into adipocere, and the intestines were changed into a greasy mass, in which it was impossible to distinguish the parts or form. The uterus was of a bright red colour, hard to the feel and to the knife, of the virgin size, its form perfectly distinct and normal, its cavity empty, and corresponding to the virgin condition.

Although in this case nothing positive could be stated with reference to the cause of death, it could be affirmed in court that at the time of death the person was not pregnant, thereby removing the unjust suspicions which had been attached to her memory, and to the character of another respectable person, since the girl's disappearance.—*Casper's Wechenschrift*. x

AVERAGE DURATION OF LIFE IN THE NEW WORLD.

THE recent report of this Board of Health of New Orleans contains the following summary:—

The average life in no country reaches threescore years and ten, announced in Scripture as the period for the duration of the life of man. The average age at death in the northern cities—doubtless owing in a great measure to the large mortality in infantile life—is from 19 years 9 months to 20 years 3 months; and in some of the cemeteries where destitute foreigners from the crowded parts of the city of Boston are buried, it is reduced to 13.49. In the south, where it is so much more favourable to infantile life, the average age is much greater. In Charleston the average age at death is near 36 years. In Vera Cruz 24.6, and in the city of Mexico 27.7; while in the city of New Orleans, the average age at death for the last year was 26.69; and in a series of the years, the aggregate of all the cemeteries was 22 years 6 months 3 days.

Of all countries on record, the rural parts of England and Massachusetts are probably most favoured with respect to infantile life; and yet in Massachusetts 40 per cent., and in England 47 per cent., die while they are going through the process of development, and before they enter upon self-sustaining life, in their 16th year. In New Orleans we have not the data to institute an exact comparison at these ages, but very near it; and we find that here only 36.98 per cent. die under 20. In this city data of all kinds are defective; we have, nevertheless, been able to construct a chart, to show the real value of life here at successive ages, and at different periods of the year. It is too lengthy for this report. We may, however, state that it shows the extremely mild character of the climate at

all periods of life: under 20, and above 50, and during all months of the year, and that the chief fatality occurs from 20 to 40 (the ages of the immigrating population), and the period, the latter part of summer. Notwithstanding all this, the following statement shows that we have a larger proportionate population at the *productive age*, that is from 20 to 50, than the most favoured parts of the world: viz. in every 10,000 in the United States, there are 3,708 10,000 in Louisiana, " 3,753 10,000 in England and Wales, " 4,028

While there are in every 10,000 in New Orleans, " 4,924

From all the information we can procure, it is satisfactorily demonstrated, that in countries where from climate, position, and refinement, a very small part of the population reach the age of their natural destiny, they approach it nearer, in proportion as they obey the laws which are adapted to the guidance of life: here, notwithstanding our deficiencies in many respects, sufficient is ascertained to hope for as near an approximation to the primeval age as anywhere; and reference to the table showing the number of the dead above 80 and 100—11.58 per cent. of the first, and 2.26 of the second—buried at the Catholic cemetery; the chief cemetery of the ancient Creole population of the city—will satisfactorily prove the fact.—*American Journal of the Medical Sciences* for July, 1850.

BOOKS & PERIODICALS RECEIVED

DURING THE WEEK.

The Anatomical Remembrancer; or, Complete Pocket Anatomist. 4th Edition. Surgical Anatomy. By Joseph MacLise. Fasciculi 6 and 7.

On the Structure of the Spleen. By Wm. R. Sanders, M.D. Edin.

Pathology of the Human Eye. By John Dalrymple, F.R.C.S. Fasciculi 5 and 6.

Brighton and its Sanative Resources; comprising a special reference to the German Spa. By Edwin Lea.

Report of the Medical Literature of the United States.

Philadelphia Medical Examiner. Nos. 9 and 10, September and October 1850.

New York Journal of Medicine. September 1850.

British American Medical and Physical Journal. October 1850.

Edinburgh Monthly Journal of Medical Science. No. 119, November 1850.

Comptes Rendus. Nos. 14 and 15, 30th September and 7th October.

Annales d'Hygiène Publique et de Médecine Légale. Octobre 1850.

Journal de Chimie Médicale. No. 10, Octobre 1850. —

On the Construction of Locks and Keys. By John Chubb, Assoc. Inst. C.E.

BIRTHS & DEATHS IN THE METROPOLIS

During the Week ending Saturday, Oct. 26.

BIRTHS.		DEATHS.	
Males....	693	Males....	417
Females..	609	Females..	428
1852		845	

CAUSES OF DEATH.

ALL CAUSES	845
SPECIFIED CAUSES	832
1. <i>Erythemic</i> (or Epidemic, Endemic, Contagious) Diseases....	168
<i>Sporadic Diseases, viz.—</i>	
1. Dropsy, Cancer, &c.	41
2. Brain, Spinal Marrow, Nerves, and Senses	97
4. Heart and Bloodvessels.....	28
5. Lungs and organs of Respiration	129
6. Stomach, Liver, &c.	49
7. Diseases of the Kidneys, &c.	19
8. Childbirth, Diseases of Uterus, &c.	8
9. Rheumatism, Diseases of Bones, Joints, &c.	3
10. Skin.....	3
11. Premature Birth	28
12. Old Age	37
13. Sudden Deaths.....	20
14. Violence, Privation, Cold, &c.	23

The following is a selection of the numbers of Deaths from the most important special causes:

Small-pox.....	5	Convulsions.....	35
Measles.....	24	Bronchitis	54
Scarlatina	32	Pneumonia	45
Whooping-cough	24	Phthisis	114
Diarrhoea.....	37	Lungs	6
Cholera.....	3	Teething	3
Typhus.....	49	Stomach	5
Dropsy	13	Liver	12
Hydrocephalus	19	Childbirth	4
Apoplexy	16	Uterus	4
Paralysis	26		

REMARKS.—The total number of deaths was 85 below the average mortality of the 43d week of ten previous years.

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer	29.51
" " Thermometer	42.3
Self-registering do. Max. 64° Min. 18°	
* From 12 observations daily. * Sun.	

RAIN, in inches, 0.9.—Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was 6°.5 below the mean of the month.

NOTICES TO CORRESPONDENTS.

Mr. D. W. Crompton's paper has been received, and will be inserted.

The communications of Mr. Balman and Mr. Oke Clark are unavoidably postponed until next week.

Dr. Mayo's contribution will be inserted in the following number.

R. K.—If our correspondent will call on or communicate with the printers, Messrs. Wilson and Ogilvy, 37, Skinner Street, Snowhill, they will give him the information which he requires. The omission respecting the Royal Exchange Assurance shall be supplied.

RESERVED.—The *Carlisle Journal*.

Lectures.

CLINICAL LECTURE

ON A CASE OF

DISEASED BRAIN, CAUSING
DOUBLE APOPLEXY,

(Delivered at King's College Hospital),

By R. B. TODD, M.D. F.R.S.

Physician to the Hospital.

(Reported by M. H. SALYER, B.A., Dem. Anst.
K.C.L.)

LECTURE XIII.

I PROPOSE to-day to offer you some remarks on the second case which was to have formed part of the subject of my last lecture. It is a case of the same nature as that on which I last commented, and it serves to illustrate the mode in which white softening of the brain is apt to take place, and the way in which that disease favours the production of apoplexy. We are fortunate in having a very full account of the case, taken with very great precision, by my clinical clerk, Mr. Vaux.

The subject of this case was named George Regan, his age was 59 (vol. xxiv.), he was admitted into Sutherland ward 20th of January, 1849. He was a glass-cutter by trade, and had lived well all his life; he, however, called himself temperate, but admitted that he had been in the habit of drinking a good deal of beer and spirits.

This man seems to have suffered long from what he calls rheumatism, but which from his habits and his age I should think was rather of the nature of gout; he had, however, no regular fit of the gout, but complained of pains in various situations, and for these he was for some time an out-patient at the hospital.

About two years ago, he was seized suddenly while at work, at nine o'clock in the morning, with a feeling of stupor: as he happened to be working at home at the time, he laid down on his bed, hoping to sleep it off by dinner-time, but when he attempted to get up he fell, and found that he had lost the use of his right side; he then became comatose, and so continued for some days. Whilst in this state of insensibility he was taken to a neighbouring hospital, where he remained for two or three months, and at the end of that time in some degree recovered from his attack, but not so as to enable him to work as he had done before his illness.

During last October he had a second

attack, of the same kind as the first, but less severe; the limbs were paralyzed, as before, on the right side; he remained a few days in bed, and recovered in some degree the use of the arm and leg, but he has never been able to work since.

On Christmas day last, at nine o'clock in the morning, before he was up, he had a third attack; on this occasion he was delirious for a short time, and afterwards became insensible, but he recovered his consciousness in the evening.

This third attack brought on increase of the paralysis on the right side, so that on this occasion not only was the motor power affected, but the sensibility likewise. This latter function was so far affected, that he could not pick up small objects, and he would frequently let fall things which he meant to retain in his hand, and he staggered when he attempted to walk. It was more than three weeks after the attack when he was admitted, and the paralytic state had not improved; on the contrary, he thought it worse. His condition on admission was as follows: there was imperfect palsy of the right side, as shown by slight ptosis of the right upper eye-lid, and a slight hanging of the cheek, the features being slightly drawn to the left side. He would protrude his tongue straight, but his articulation seemed somewhat difficult. In walking he slightly dragged the right leg; the grasp of the right hand was moderately firm, but not so firm as that of the left; the muscles of the right leg and arm were flaccid, and less nourished than on the left side. The impulse of the heart was very strong, and a mitral systolic bellows sound could be distinctly heard.

For some days after his admission he suffered very much from constipated bowels, which it required the strongest purgatives to overcome. He then began to complain of heaviness of the head, a disposition in his thoughts to wander, and a difficulty in collecting them. These symptoms appeared to me to portend the approach of another attack, such as he had on three former occasions. His remarkably sallow complexion and general leucophlegmatic appearance led me to suspect the existence of renal disease, resulting from that particular form of kidney—small and atrophied—which so frequently accompanies a gouty condition. This opinion was confirmed by the characters of the urine, which was pale, of low specific gravity, and slightly but distinctly albuminous. With this view, and imagining that the uneliminated urea might be contaminating the blood, and affecting the brain, I ordered him to be freely blistered at the back of the neck. This seems to have somewhat relieved him, for the next day he was reported to feel, and

ther easier as to his head, and to have more power over his thoughts. At nine o'clock, however, on the following morning, the house-physician, Mr. Arncliffe, was called to him, in consequence of his having been suddenly seized with a fit; he found him completely paralyzed on the left side, both as to sensibility and voluntary motion; his left eye squinted, and was twisted downwards and inwards; at every expiration his left cheek puffed out from want of power of the buccinator; the right leg and arm moved when pricked, the left not; his breathing became louder and more stertorous, his coma deeper, and at last he died.

There was in the attacks to which this patient was subject a curious combination of the epileptic and apoplectic, the one following upon the other. At the foundation of them, no doubt, was the diseased state of kidney. The first attack of sudden stupor was probably a slight epileptic seizure, the effect of which was a disturbance in the circulation in the brain, and the giving way of some small vessels—a slight apoplexy, with compression and rupture of fibres, and consequent paralysis.

Whilst the one or two small clots which had been effused on this occasion were undergoing absorption, and some attempt at reparation was taking place, he had another seizure of the epileptic kind,—a further disturbance of the circulation and nutrition of the brain on the same side, probably in the corpus striatum, or the fibres which pass from it to the hemisphere. But as the paralytic state appears to have been only slightly increased, and as the coma was of short duration, it is not likely that any effusion of blood took place on this occasion.

The epileptic character of the third attack was manifested in the delirium with which it was ushered in, and which ended in coma. There was a decidedly increased paralysis after this attack; but inasmuch as quite as much paralysis is apt to follow the simple epileptic seizure, it by no means follows that any effusion of blood took place, although it is probable that such must have been the case, from the fact that the palsy showed no signs of improvement, as is generally the case with the epileptic palsy. As this attack occurred at a time when Christmas festivities are more or less prevalent with all ranks and classes, it is very likely that the immediate exciting cause of the attack was due to over indulgence of some kind.

The fourth attack had in its premonitory signs all the characters of a threatening epileptic paroxysm; and knowing, as we did, the existence of renal disease, we were prepared for such an attack. The

epileptic coma, however, soon passed into the profounder coma of a compressed brain; and as a new hemiplegic paralysis of a very complete kind showed itself on the left side instead of the right; it was easy to infer that a new and extensive apoplectic effusion must have taken place on the right side of the brain.

Assuming that the first three attacks were epileptic, followed by an apoplectic effusion, and that this effusion was due to a weakened condition of the arterial coats as the result of disease, it was quite consonant with experience to attribute the fourth attack to a similar cause, and to infer that arteries similarly diseased had given way on the left side of the brain. Most cases of rupture of vessels in the brain at the age of this patient take place from disease of the arteries; and it is very common, as was first pointed out by Bistot, for the arteries of the brain to be affected in a symmetrical manner—i. e. corresponding arteries of opposite sides will be similarly diseased, and to nearly the same extent. In this way disease had been, for some time, making progress in this man's brain symmetrically; there was first palsy of the right side, and then a similar condition of the left; and this simple fact of symmetry pointed to the arterial system as the seat of disease, and therefore to its usual result, apoplexy.

On opening the patient we found there was a double apoplexy corresponding to the double paralysis—one of long standing, and one recent—the first on the left hand, the other on the right. The original hemorrhage affected the corpus striatum and optic thalamus on the left side; and it was evident that the hemorrhage on this side did not take place at once, but on two occasions, at least. The substance of the corpus striatum had evidently been the seat of the older effusion. It exhibited on section several bloody clots, and that peculiar yellow discolouration which always succeeds to a hemorrhage. The more recent effusion was a clot which had formed quite on the surface of the optic thalamus, extending likewise to the corpus striatum, and lodged in a depression on the surface of those bodies. We had thus an explanation of the cause of the original paralysis of the left side; and its imperfect nature was plainly due to the fact, that there was but little destruction of the corpus striatum, and that the compression of the later clot affected chiefly the optic thalamus, and but slightly the corpus striatum.

But the apoplectic effusion on the right side was much more extensive; it involved parts corresponding to those affected in the first seizure, but to a much greater extent; and the brain substance

was more completely torn up and destroyed. The clot was very large, and it entirely filled the right lateral ventricle, breaking through its roof, and tearing up the white substance of the cerebral hemisphere; it likewise tore up the corpus striatum, optic thalamus, and the septum lucidum, all which parts were completely swept away. This remarkable destruction was no doubt owing to a previous diseased state of the brain. I mentioned to you in my last lecture that it is very common for apoplectic effusions to be preceded by white softening; and when the softening has got to a certain extent, the brain substance is no longer an adequate support to the vessels, which, themselves more or less enfeebled by disease, give way, not in one point only, but in many, and the abundant effusion plunges up the softened matter, quite destroys it, and takes its place: all this was the case in the present instance. The arteries of the brain were very generally diseased on both sides, and exhibited that symmetry to which I have alluded.

It is probable that the first part of our patient's last attack—the disposition of his thoughts to go wool-gathering—took place in connexion with the softening; while the final catastrophe—the apoplexy and death—resulted from the hæmorrhage to which that softening, coupled with the diseased condition of the arteries, inevitably led.

We found, likewise, as had been anticipated, a very diseased state of the kidneys: they were very contracted, the cortical substance much wasted, fissured, and granulated on its surface, the tubular substance healthy, and the capsule thickened. This condition of kidney, formerly described as the third stage of Bright's disease, is, in reality, a chronic degeneration or wasting of the kidneys, due to a deranged and damaged nutrition, for which I should be glad to find some other name than chronic nephritis: I have called it *gouty kidney*; and in this and many other cases this name is very appropriate. But it occurs in cases where there is no evidence of gout. The result of the disease is to render the kidneys imperfect excretories for the elimination of the urea, and other elements of the urine, which accumulate in the blood, and give rise to various morbid changes throughout the body, and are especially mischievous to the functions of the brain.

The heart and the arterial system were likewise extensively diseased. In the coats of the arteries were very numerous deposits of atheromatous matter. The heart was much dilated and hypertrophied, especially as regards the left ventricle. The fibrous basis of the valves was extensively thickened, apparently by some deposit, which

rendered it opaque; and impaired its flexibility; and, as is generally the case in this particular form of disease, all the valves were altered in this way; those of the right side, however, being much less diseased than those of the left. The semilunar valves of the arteries, especially of the aorta, had the fibrous festoons at their bases much thickened, the curtain of the valve being very little affected, and its function, therefore, but little impaired; and the cordæ tendinæ of the mitral valve were much thickened, and somewhat shortened, and the curtains of the valve also thickened. Similar changes had taken place in the tricuspid valve and its tendinous cords, but to a much less extent.

Taking, then, a general retrospect of this case, we find there is quite enough to account for all we have seen. The sequence of the events may be thus described:—First, the man gets into a general gouty condition, and the elimination of this morbid material gives rise to an irritation of the kidney, which at length assumes the form of gouty kidney, or, if you will, chronic nephritis; and this chronic nephritis incapacitating the kidney for the perfect discharge of its function is the cause of all the subsequent maladies: the blood becomes contaminated, deposits take place in the tissues of the heart's valves, in the large arteries, and in those of the brain; the diseased arteries of the brain become insufficient channels of supply; white softening is the consequence, and the unsupported and unhealthy arteries at length burst; and thus all the circumstances, from first to last, fall in regular order as cause and effect.

The deposits in the arteries produce a two-fold influence upon the circulation—by roughening the inner surface of the arterial channels they create a certain amount of direct obstacle to the flow of blood from the ventricle; and by diminishing, or nearly destroying, the elasticity of the arterial walls, they destroy one of the most important forces by which the circulation is carried on in the arterial system. Thus the arteries, from being elastic yielding channels, with perfectly smooth inner surfaces, are changed into resisting inert tubes, with rough inner surfaces. It is plain, then, that, under these circumstances, the heart has to encounter great obstacles, and to do a great deal more work than when the arteries are in their normal state. Hence the dilatation caused by the obstacle to the free flow of the blood; and the hypertrophy, by the greater exercise and effort of the muscle of the heart. The increase of force is merely remedial, to meet the increase of obstacle, and is one of those beautiful instances of self-adaptation.

to change of circumstances with which the animal organism, especially the muscular system, so much abounds.

As these deposits go on they impair the materials of the arteries of the brain; the degenerated walls of these vessels possess less strength, and are less able to support their contents. There is no undue determination of blood to the brain, but the reverse, for the blood that goes to the head has, in the erect posture, to be pumped up against the force of gravity; and, therefore, any obstacle in the course of the arteries would be more felt in this direction than in any other. It is a common notion that the hypertrophy of the heart gives rise to the apoplexy, by sending the blood with an undue impulse to the head; but for the correction of this error we need only remember that the additional force is merely such as is necessary for the exigencies of the circulation, and such as shall preserve the force of the blood's current as near as possible to the normal point, in spite of the existing obstruction. The actual force with which the blood circulates in the morbid arteries is most probably less rather than greater than in health. The apoplexy is, in fact, due to the diseased state of the arteries, which renders their walls an inadequate support to their contents, and to the diseased state of brain, which imperfectly supports the arteries.

A vast number of the cases of apoplexy which occur about the period of life of our patient, or after the age of 50, are of this kind—a fact that has an obvious and an important bearing upon the question of treatment.

There is a practice, unfortunately too common, but which, I think, is every day becoming less common—namely, that of following an attack of apoplexy by depletive measures, very much as a matter of course. However applicable such a mode of treatment may be to strong, young, hale, and plethoric subjects, I presume no one will say that it is very well adapted to patients who have passed the meridian of life, and whose blood and whose tissues are more or less contaminated by gouty matters, and with whom a morbid state of the arteries of the brain has already greatly weakened the nutrition of that organ. The case, indeed, which I have just detailed to you, is one of many which proclaim loudly that a depletory system ought not to be pursued indiscriminately, or even generally, in apoplectic cases.

With reference to this question of depletion in apoplexy, I would refer you to an interesting and very useful work by Mr. Copeman, in which he has collected, from a great variety of sources, a large

number of cases which presented the symptoms of apoplexy. Of 155 cases in which the treatment was specified, 129 were bled, and only 26 were not. Of the 129 who were bled 51 recovered, and 78 died—the cures being 1 in 2½, the deaths 1 in 1½. Of the 26 who were not bled 18 were cured, and 8 died, the proportion of cures being 1 in 1½, and of deaths 1 in 3¼. Eighty-five of the cases were bled generally and copiously, and of these only 28 recovered, and 57 died—in other words, two in every three cases terminated fatally. I am quite aware that the small number of cases not bled casts some doubt on the validity of the conclusion to be drawn from the comparison of the results of the treatment. But the fact that considerably more than half of those treated on the antiphlogistic plan died (and we owe much to the industry of Mr. Copeman for bringing it out), is a highly significant one, and should arrest attention.

I think I have now said enough to convince you that the treatment of patients with apoplectic symptoms must not be regarded as a matter of routine, but as a question of grave import, and which demands the most anxious consideration of the practitioner. Let me add, that it sometimes requires the exercise of no small courage and self-possession to resist adopting that practice; for the popular feeling, led by the prevalent medical practice, is entirely in favour of it, and would readily condemn a practitioner as guilty of the death of his patient who suffered him to die unbled. It is a far more daring and courageous thing to open a vein on the spot, and in the presence of a number of anxious friends, than to adopt less showy, and apparently less active, measures.

But, indeed, you need not be inactive, even if you decide against adopting the plan of bleeding. Having placed your patient in an easy position, in which no excitement of muscular action is likely to take place (for you must bear in mind that reflex actions may often be readily excited in these apoplectic cases), you should immediately direct your attention to the state of the stomach and intestinal canal. Sometimes in these cases the stomach is overloaded, or the bowels are confined, and the administration of a little purgative medicine will often provoke a moderate sickness, which unloads the former. Nor can there be any objection to adopting measures to clear out the bowels, either by an active purgative administered by the mouth, or by stimulating and purgative enema.

If, upon full inquiry into all the particulars of the case, you find that your patient is of full plethoric habit, with too much blood in his body, and with a suffi-

clearly strong heart, you may bleed him with every chance of success; but if he has been of intemperate habits, is labouring under organic disease of the heart and arteries, is of gouty or rheumatic constitution, then, whatever popular or medical custom may say, my advice to you is, hesitate much before you deplete by bleeding.

The objects which it is proposed to gain by bleeding are a diminution of the cerebral congestion, and the stoppage of the hæmorrhage into the brain; and where it is quite clear that cerebral congestion does exist, and that that congestion causes the cerebral hæmorrhage, this is clearly a rational practice. But you must bear in mind that in a large number of the cases—probably the majority—there is in reality no cerebral congestion, and that the hæmorrhage is of a kind not likely to be stopped by taking away blood—by establishing another hæmorrhage elsewhere.

On the whole, then, I think that the results of experience denote that the majority of cases of apoplexy are best treated by purging, shaving the head and keeping it cool—perhaps blistering, and that bleeding is rarely applicable, except to the young, vigorous, strong, and plethoric.

QUARANTINE PRECAUTIONS AGAINST CHOLERA IN SWEDEN.

THE system of quarantine against the cholera, abandoned as useless in almost every other state of Europe, prevails in Sweden in its utmost rigour. It is not only enforced against all persons entering the country from abroad, but the several provinces are closed against each other. If the disease appears in any place, it is cut off from all communication with the neighbouring towns. For the protection of Stockholm a quarantine station is established on the Gotha-Elf, under the command of a marine officer. In the interior the isolating system was put in force before it was adopted in the capital. In all the larger towns a guard at the gates sends back all travellers who cannot prove that they have not visited any infected place for ten days. In Ostgothland it is proposed to establish a military cordon along the canal. At Södertelge a ship of war is placed that compels all vessels as they arrive to anchor in the quarantine station. Vessels from the Hjelmar and Mälaree are detained at Långholm, and examined. In the North of Sweden these regulations are only enforced against persons; but in the town of Lund they are extended to goods if they are imported through Malmö.

Original Communications.

ON THE USE AND ABUSE OF ALCOHOLIC LIQUORS.

By THOMAS MAYO, F.R.S.

Fellow of the Royal College of Physicians.

Καλεστων το μετρον ειπεν.

In the "Outlines of Medical Proof" * I have endeavoured to illustrate, from the work of a most distinguished philosopher, defects incidental to the application of hypothesis, through a forced accommodation of such hypothesis to the series of phenomena which it professes to explain. But we need not wander to Giessen in search of evidence to the reality and importance of these defects. We are met by it in our English medical literature, and we find it in works from which we should feel most anxious to exclude such defects; works, namely, which treat of medical or hygienic subjects in their most practical relation to the wants and habits of society. Let us take up the prize essay of Dr. Carpenter, "On the Use and Abuse of Alcoholic Liquors in Health and Disease." The physiological or pathological objects pursued in this work are two-fold—very mixed up, indeed, and confounded in his mode of treating the subject, but capable of being estimated separately; namely, to point out and illustrate the fearful mischievousness of intemperance; and again to point out, that the "Moderate habitual use of alcoholic liquors is not beneficial to the healthy human system;" in the course of which proof he makes his reader fully aware, that he conceives himself also to have succeeded in proving this habitual moderate use to be probably *prejudicial* to the human system.

My purpose being practical, and the mischievousness of intemperance being unquestionable, and admitted at all hands, I will limit my remarks to the last mentioned object of Dr. Carpenter's proof, so far as I can disentangle it from the subject of intemperance and excess, from which I regret to say the author is

* Outlines of Medical Proof, revised and corrected.

at no pains to keep it philosophically separate. It is, indeed, a part of the unintentional policy of the prize essay, so to manage the lights which it throws upon intemperance, that they shall glance off, and shed an ominous colouring upon moderation.

"What," says Dr. Carpenter, "are the effects, corporeal and mental, of alcoholic liquors, on the healthy human system?" This question, in the mode in which it is put, is unreserved, and applies alike to excess and moderation. He answered it first, physiologically,—that is, in relation to the influence of alcohol upon the physical, chemical, and vital properties of the animal tissues and fluids,—then, pathologically.

With respect to the *physiological* answer, it runs thus:—"Corrugation of tissues; coagulation of albumen; impairment of solidifiability of fibrine; irritating action on living textures; temporary exaltation of nervous powers; change in red corpuscles. These propositions are unquestionably the result of a process of inductive inquiry, and are captivating as such; but I have vainly endeavoured to find out what bearing they may have on the moderate habitual use of alcoholic stimulants? With respect to Dr. Carpenter's *pathological* answers, in the remaining sections of the 1st Chapter, they place before the reader a string of maladies all usually imputed to excess, and not brought home by him to the moderate use of alcoholic liquors.

Now I can conceive some one, in his anxiety to obtain information on this very important subject, addressing Dr. Carpenter in the following terms:—"You have laid down, sir, before us five physiological inductions or results of an inductive process. What are your deductions from them? Do they infer mischievous results from the excessive use of alcoholic liquors? Of these every one has long since been aware;—or from their moderate use? But on this latter point we have your own affirmation, in your republished article from the Quarterly Review, that "if the whole world be really temperate in the use of fermented liquors, there would be no need of total abstinence societies." We must, indeed, conceive your deductions, if they apply only to excess, to amount to a truism, which does not reach the object which you evidently have in view in your prize essay,—namely, to discou-

rage the use of these stimulants in any quantity among persons in a healthy state; and yet we must consider, on the strength of the above quotation, that you are not yourself convinced of the soundness of this view, but that you support it rhetorically; and "because," as you say in the same article, "sad experience has shewn, that a large portion of mankind cannot be temperate in the use of fermented liquors; and that nothing short of total abstinence can prevent the continuance, in the rising generation, of the terrible evils which we have at present to deplore, because experience has further shewn that the reformation of those who are already habitually intemperate cannot be accomplished by any means short of entire abstinence from fermented liquors; and because experience has also proved that this reformation cannot be carried to its required extent without the moral influence of the educated classes. Such influence, you say, can only be afforded by *example*." Thus, sir, in your conscientious anxiety to make out a case for the total abstinence societies, you defend it on grounds which appear to us contradictory."

Leaving Dr. Carpenter in the above dilemma in regard to his *general* description of the subject, I may observe that the same method of *physiological* deduction is to be found in other parts of his essay, in which he more particularly discusses the question how far a moderate use of alcoholic liquors is expedient or prejudicial in our normal state. Thus, in testing the capability of alcohol to perform its functions as a heat-producing material, we are told that "Dr. Prout and Vierdot are both of opinion that alcohol, and all liquors containing it, have the remarkable power of diminishing the quantity of carbonic acid gas in the expired air." But Liebig, it seems, is willing to account for this without assuming a corresponding diminution of animal heat, through the increased formation of water which will take place when alcohol is the combustible material. On the other hand, Dr. Carpenter quotes Bonchardat, who has found that "when alcohol is introduced into the system in excess, the blood in the arteries presents the aspect of venous blood;" showing that it has been prevented from undergoing the proper oxygenating process. Now Liebig's explanation

is good against the mere deficiency in the evolution of carbonic acid, used as an argument against the moderate use of alcohol, while the experiments of Bouchardat relate to alcohol as introduced in excess (and, I may add, what is equivalent to excess, on an empty stomach, when venous imbibition will be rapid); under which circumstances it may reasonably be expected to influence the colour of the blood: accordingly they do not apply to the question of moderate use. Here the allegation of mischievousness against alcohol is founded on conditions which are not realised: nor, in the remainder of this part of the work, can I find one argument *made good* against a moderate use of fermented liquors that is not based on some hypothesis equivalent to the supposition that, because an eminent statesman once died of apoplexy the day after eating twelve mutton chops for dinner, the introduction of two mutton chops daily into the system would end in apoplexy. But I may here be answered, mutton chops are not a poison, and alcohol is—at least, according to the author of the Prize Essay. I reply that the meaning of the word “poison” must be unduly stretched if compelled to bear out this view in toxicology.

Thus, again, the quotation from Dr. Hawkesworth, who, in his Voyage to New Zealand, makes peculiar mention of the facility with which wounds heal in the New Zealanders, who at that period drank nothing but water, is rendered inoperative on Dr. Carpenter's physiological argument against a moderate use of alcoholic liquors by his own comments upon it; for he opposes it, not to such moderation, but to “the unquestionable fact that habitual excess in the use of fermented liquors produces a condition altogether opposed to the healthful performance of these processes.” This point I willingly concede to him; but I must urge that his admissions in regard to the occasional benefits of alcoholic liquors under cold and strong exertion, when they promptly supply the means of reproducing heat without disintegration of tissues, might, by parity of reasoning, lead him to much further admissions in favour of a moderate use of alcohol under the normal circumstances of a large part of the community. I have been induced to think that Dr. Liebig's expressions

on the latter point have led to a somewhat incorrect appreciation of the waste of tissues by oxydation under muscular action. It is asserted by Dr. Liebig that “a rapid change of matter determines a greater amount of mechanical force, and conversely, that a greater amount of mechanical force expended in motion determines a more rapid change of matter. That the latter of these propositions is true may be readily admitted; but that a rapid change of matter determines a greater amount of mechanical force is absolutely irreconcilable with experience, as various forms of atrophic degeneration testify. It appears to me that the illogical conversion of propositions here made by Liebig is calculated to prejudice us, both in favour of a waste of tissues, and against the means by which it may be sometimes arrested or mitigated with advantage, and in that respect against the use of alcoholic liquors, though suggested by Liebig himself as useful, under certain conditions, for this purpose.

The question of stimulation comes naturally before Dr. Carpenter in reference not only to excess, but to the moderate habitual use of alcoholic liquors; and to the latter, as well as to the former, is applied the well-known objection of collapse and debilitation, as presumably involved. On this point I have a few words to offer. The word “stimulant” is rather vague: I will define it for my present purpose, whatever is introduced into the system as being promotive of energy irrespectively of, or in the absence of, any nutritious element contained in it. In many instances nutrition alone is sufficient for the production of energy—that is to say, no element is wanted for this purpose that is not contained in the nourishment required to supply the waste of tissues. With some the energy generated out of this source is such, that the amount of food has in some measure to be diminished for the prevention of excitement—i. e., excessive energy. In others—and this latter category is a very extensive one among the educated classes, particularly with those who live in cities—the stock of energy fit for the purposes of life, and suited to the health of the system, is best maintained by being made up in part through certain innutritious or apparently nutritious substances, which are stimulants according

to the above definition. It is to be observed that these persons are always and normally in that state, which Dr. Carpenter appears to consider exceptional, and to which he allows moderate alcoholic stimulants, on the ground of "deficiency of other adequate sustenance." But he does not seem aware how large a category this exception applies to. The principal of these stimulants in European use are alcoholic liquors, tea, coffee, and opium. Of these each and all have their own modes of subsequent collapse; each and all require prudence and temperance in their use; each and all are appropriate only to certain constitutions. I believe, indeed, that of those who require some stimulant, a very large class would be much prejudiced if compelled to depend upon tea or coffee; nor would the hypothetical advantage of theine and caffeine containing the nitrogenous element of life be a compensation to the persons who may be suffering from the malaise in some cases produced by each, and the heating properties of the latter. Dr. Percival, by the way, has pointed out to us, and illustrated his opinion, that wine is the appropriate remedy for the *mischievous* effects of tea.

Now all the three classes of men in reference to whom I have considered the doctrine of stimulation, are obnoxious to the collapse and other effects connected with mismanaged excitement. To all of them the same precautions, in kind, are required, which Dr. Carpenter limits to the alcoholic sources of energy. But I cannot go along with him in considering "that the moderate use of them will manifest itself sooner or later in diminution of the digestive power; that we may with the highest probability, if not with absolute certainty, attribute many of those chronic disorders which affect the digestive apparatus, the excretory organs, and the nervous system in advancing life," to the "excessive action to which they have been subjected under the stimulating influence of alcoholic beverages" *moderately used*; and it is to their moderate use that he addresses these strictures.

In regard to this point, indeed, the arguments drawn by him from the abuse against the use of these gifts of Providence comes into play in a very insidious manner. Doubtless the alco-

holic excitants *do* some way, even in their moderate use, tend to the production of much mischief to the organs of digestion; in a way, indeed, which has been well illustrated by Dr. Carpenter, with the omission, however, of this point in their favour, that they tend to evil through an instrumentality closely connected with great good. By assisting the primary assimilation, these stimulants can enable the system to take in more nourishment than it requires, or than is consistent with the secondary assimilation. In this case it is not the stimulant, but the food taken in and partially digested through its assistance, which damages. Meanwhile the same stimulant, taken in the same quantity, might enable the digestive apparatus to manage the *fit and normal* quantity of nitrogenised food on easier and more wholesome terms, than if the process of air and exercise were exclusively resorted to for the supply of energy to the nervous functions. I may here observe that Dr. Carpenter's statement in regard to the absence of alcoholic liquors in the training of boxers is questionable. They were not excluded by Mr. Jackson; and it is noticed in the recent translation of the work of M. Franz Simon, that two half-pints of ale are the daily allowance of persons thus situated. I notice this, because, in the palmy days of pugilism, much thought and sagacity were expended in producing in them a very perfect state of health, with a view to an exertion which required both strength and endurance.

But the admission of Dr. Carpenter, in his article from the Review, that if temperance in alcoholic liquors could be established abstinence would be unnecessary, authorises me to believe that he would himself abandon the physiological and pathological assault upon the moderate use of these liquors, but for some doubts which he may be supposed to entertain whether his ethical argument would of itself be conclusive,—whether the enjoining abstinence in all, merely to prevent excess in the many, is quite consonant with sound morality, or is likely to prevent the evil which he deprecates. I now turn to this moral plea in favour of teetotalism; and first to its religious element.

"St. Paul," says the Archdeacon of Bombay, as quoted by Dr. Carpenter,

"accounted one single soul so precious, that he would on no account allow himself in any indulgence that tended to endanger a brother's soul. 'If meat make my brother to offend, I will eat no meat while the world standeth, lest I make my brother to offend. It is good neither to eat meat, nor to drink wine, nor anything whereby thy brother stumbleth, or is offended or made weak:' and we must bear in mind that flesh and blood are here mentioned by St. Paul as good creatures of God; they are not intended to designate things evil in themselves." This saying of St. Paul is the charter of teetotalism: "that is to say, under the guidance of the above application of St. Paul's doctrine, and on the ground taken up by Dr. Carpenter, that "the whole amount of crime, and misery, and wretchedness, connected with the abuse of alcoholic liquors, greatly exceeds the whole amount of benefit arising from the right use of them," the many—for they are many—who find, or consider themselves to find, benefit from the use of fermented liquors, are bound to surrender the virtue temperance to the half-virtue abstinence. Might it not occur to the Arch-deacon of Bombay, if he intends this application of his doctrine, that he erects a something whereby "a brother may stumble, or be offended, or made weak," when he *thus* attaches delinquency to acts performed by thousands of his brethren, which have become, by habit, necessary to the comfort of many of them, in favour of which many of them entertain a strong opinion as conducive to their health, the acts themselves being innocent in their own nature? And such is the moderate indulgence in fermented liquors. I am not obliged by my argument to point out what is the correct application of St. Paul's doctrine: but this, I am sure, is *not*. I will only observe that there is no method of employing the doctrine of Scripture more questionable than that which hastily forms on its denunciations a scheme of positive and conventional delinquencies. But there are texts in Scripture which, by parity of reasoning, might have prevented this error being committed in the case before us. The tendency to anger is one at least as strong, and as liable to be abused, as the tendency to alcoholic stimulants. Yet, of the former, Scripture says, "Be ye angry, and sin not; let not the sun go down

on your wrath." It enjoins even in this case, not abstinence, but temperance. Every scheme of morality in some sort places virtue in the avoidance of extremes; and it were strange indeed if the extreme to which the use of wine and vinous things lays us most open were of such enormity as to become exceptional in reference to this rule, and should interdict the practice in kind, as well as in degree, seeing that the first recorded miracle of our Saviour was performed at a feast, and in increasing the quantity of wine served out to the guests.

The victory over intemperance—and that is the object at which Dr. Carpenter really aims, while he erroneously decries moderation—must be gained by the slow but certain influence of good habits produced by sound education. The attempt to reach that point through the charlatanic expedient of teetotalism will only postpone success by averting attention from the right method of attaining it. The promoters of total abstinence from alcoholic liquors have raised a high structure of argument; but, unhappily, they have not based it upon a just consideration of the moral and social elements of the human mind; and it will sink before the reflux of those tendencies which they have endeavoured to stifle, but which they are only qualified or called upon to check. I suspect that a fallacious appearance of success has been given to the well-intended efforts of Father Matthew by the locality in which, and the circumstances under which, he commenced his labours. Stimulants are convertible; and the physical stimulus is easily superseded, for a time, at least, by moral intoxication, when the latter is congenial in kind. The Irish could easily be led to abandon whiskey by their church, when skilful and unscrupulous agitators were feeding them with a stimulant compounded of sedition and treason. Mahomet succeeded in forbidding the juice of the grape to his followers; but he did not thereby prevent the use of opium: and it is for the teetotallers to consider, while they are substituting particular prohibitions for principles, how far it is prudent to risk a substitution of the same kind with us.

It may occur to any one in reading the above remarks, that they are entirely confined to a detection of supposed errors in an able writer. In answer to

this, I reply, that I am not professing to review the prize-essay of Dr. Carpenter. His work is a strong, but certainly an *ex parte*, attack upon a world-wide practice of society. It is, perhaps, desirable that his views should first be treated critically, in regard to their *ex parte* character, as has been attempted in the foregoing remarks; and when the subject may have been thus sifted, that some competent person should undertake a large and comprehensive view of it in all its bearings.

56, Wimpole Street, Oct. 24, 1869.

CASE OF GENUINE COW-POX VESICLE RISING AT ANOTHER PART THAN THE POINT OF INSERTION.

By ROBERT OKE CLARK,

Late Resident House Surgeon and Apothecary
to the Royal South Hants Infirmary.

On the 26th of August last I was requested to vaccinate John Willesden, *et. 9 months*, a fine healthy-looking child, who, his mother told me, had scarcely had an hour's illness since his birth,—a case promising well that the vaccine should take due effect. I inquired very particularly into the present state of the secretions, &c., and found all in proper order. The breast had been as yet the only source from which his nourishment had been derived.

Three good incisions were made with the lancet in the upper part of the left arm, over the belly of the deltoid muscle, and I inserted into each successively two points well charged with the matter, which had been sent me by the National Vaccine Establishment. Not more than the usual bleeding occurred during the operation, and not the *slightest* abrasion of the cuticle could be seen on any other part of the arm, nor was any scratch made during vaccination.

On the 30th there seemed a slight disposition to the formation of vesicles; but during the next day all redness disappeared, nor did any further signs of the matter having taken effect show themselves at this spot.

On the evening of September 2d the mother noticed what she considered a small red pimple about the middle of the forearm, and, on my visiting the case next morning, she showed it to me. It was then larger, and had, in my opi-

nion, all the characters of a cow-pox vesicle of about the third day after vaccination. I watched the case daily, and became more and more convinced that it was genuine cow-pox. Still, not liking entirely to rely on my own judgment in so unusual a case, on the 6th of September I showed it to my father, and his opinion entirely coincided with mine. It subsequently ran through all the successive stages of a common cow-pox vesicle; and, had I not known in what part of the arm the child had been vaccinated, and that the cuticle of the forearm was previously *quite* entire, I should have unhesitatingly said the matter was inserted at this spot, and that all was going on in a normal way. As, however, I had *never* seen an exactly similar case before (although I have had under my care nearly 1000 cases of vaccination), I confess I was inclined to be rather sceptical on the point, in spite of my convictions to the contrary. I therefore determined to vaccinate the child in the other arm on the 14th day after the primary vaccination, and obtained a fresh supply for that purpose from the National Vaccine Establishment. I inserted into four incisions a quantity of matter from glasses, and subsequently four points well charged with the lymph. No other effect was produced than would have followed a simple scratch—viz., there was a little redness of the part for about a couple of days.

Feeling sure the case was a curious one, I wrote to Dr. Gregory, of the Small-pox Hospital,—one of the greatest authorities of the day, I presume, on these matters,—related the case at length, and asked his opinion. He seemed even more sceptical on the case than I had been, and told me “if the single vesicle were *bona fide* cow-pox, the case was a *very* curious one indeed, undoubtedly the first of the kind that ever happened,” and “that some still stronger testimony would be desirable to prove it such. This testimony would be supplied only by the insertion of *very efficient fluid* lymph into the arm of the same child. If, under such renewed application of an active virus (performed so as to leave no shadow of doubt that the second vaccination had been *decidedly* performed), the child should remain unaffected, we must believe the constitutional vesicle you saw on the forearm was ‘*genuine vaccinia*,’ but, on the contrary, if the second vac-

cination rises into good vesicles, it is clear that the first vesicle was only ecthyma." He advised my giving the child two alterative doses of calomel before again vaccinating him, and offered to charge two lancets for me with "matter of undoubtedly good character." I sent him two, fine and sharp, which he charged from two different children; and the boy was, on Sept. 26th, revaccinated with this lymph about nineteen or twenty hours after it was taken. "This," the Dr. considered, "would put the case beyond doubt;" but he "anticipated that the revaccination would take effect, and that the first vesicle would then be proved to be ecthyma."

However, ~~so~~ such result followed: the vaccination was done efficiently, and the only effect produced was just that which occurred after the first revaccination, and nothing more; not the least inclination ever showing itself for a vesicle to rise. No constitutional disturbance whatever was present.

Dr. Gregory states, "he has seen constitutional vesicles frequently; they are certainly not very common; but then he has always seen at the same time ~~unequivocal~~ primary vesicle." Such has also been the result of my own experience. I have seen them in various parts of the body, but never, previously, as now, *without* a primary vesicle at the point where the lymph was inserted. The same is stated to me to have been the case in the experience of others. May not this case, then, be justly entitled to Dr. Gregory's appellation of very curious and unique?

Farnham, Surrey, Oct. 29, 1880.

THE MEDICAL AND CHIRURGICAL SOCIETY.

At a meeting of this Society on the 29th ult., the following resolutions were agreed to:—

1st. That the trustees of the Society be authorized and empowered to sell £600 stock, to meet the expenses consequent upon the completion of the lease and the repairs of the premises, as authorized by two general meetings of the Society, held on May 1st, 1849, and July 11th, 1880.

2d. That the front drawing-room be appropriated to the general uses of the Society, and that the evening conversations and the meetings of the Council take place there.

3d. That the Council be empowered to let the stables, and such portion of the premises as may not be required for the purposes of the Society.

AN ESSAY ON UNHEALTHY INFLAMMATIONS.

By M. BROOK GALLWEY, Esq.

Assistant-Surgeon Royal Regiment of Artillery,
Woolwich, Kent.

[Continued from p. 712.]

ERYSIPELAS (continued).

Mr. Lawrence's views of the pathology of the disease—His treatment consequent thereon—Their mistaken principles and dangerous tendency.

"Treatment.—As this affection resembles other inflammations in its causes, symptoms, and effects, so it must be treated on the same principles; that is, on the antiphlogistic plan. Venesection, local bleeding, purging, and low diet, are the first measures, to which saline and diaphoretic medicines may be afterwards added. The earlier these means are employed the better: vigorous treatment in the beginning will often *cut the attack short*—(this I can easily imagine—*very short!*)—and prevent the disease from spreading beyond its original seat." (pp. 39-40).

Pursuing the system of education so profitably worked out into practical development by the immortal *Mr. Squeers* (whose custom was first to instruct his youthful disciples in the conjugation of the active verb "to clean," and in the declension of the noun-substantive "windows," and then to afford them the means of carrying out the theory upon his own premises), Mr. Lawrence, after, as I have now shown, elucidating the theory of *slaying "made easy,"* proceeds to illustrate his principles by some practical examples:—

"A patient, 57 years old, was admitted under my care at St Bartholomew's, for an extensive laceration and detachment of the scalp, caused by falling down a flight of cellar steps. From the 3d of June to the 14th he was *bled eight times (!)* from the arm. All symptoms of excitement had ceased on the 15th, when the head was easy, the pulse quiet, and his state favourable in all other respects. On the 18th, erysipelas appeared in the arm and neighbouring part of the fore-arm, the affected parts being tumid, bright red, and very painful. He *died* on the 21st, the erysipelas not having extended beyond the parts first affected." (pp 7-8.)

Now, I think it may legitimately be asked, upon a review of this case, how Mr. Lawrence explains the setting up of erysipelas in the present instance, after the abstraction of (probably) eight pounds of blood from the gross amount in circulation, if the conditions necessary for the establishment of this inflammation are such as constitute a state of *hyperæmia* of the system—an exaltation of the sthenic powers of the individual?

Or, to put the question in a plainer shape, is it easy to conceive a case more felicitously illustrative of the position that erysipelas is a disease of *feeble power*, and that "the expressions of passive and asthenic inflammation" are neither "unmeaning, nor calculated to convey erroneous notions?"

The next case that I shall notice is one of the most *striking* description, and in which the issue was happily less unfavourable than in that I have just recorded. The reader will not quarrel with me if I give it in *extenso*. It is interesting as confirmative of the tenets of some modern Sangradores, who cleave to the canon of their illustrious progenitor, that it is an *error to suppose that blood is necessary for the support of our species!*

Mary Carter, aged 19, was admitted into St. Bartholomew's Hospital on the 26th of April, 1826. The patient, who is in service, acknowledges that she has been in the *regular habit** of drinking freely of gin and porter. She has suffered for seven years with disease of the nose (lupus). The ulceration, which has caused considerable loss of substance in the *alæ nasi*, and an aperture in the septum large enough to admit the end of a finger, has been cicatrized for some time; the surrounding skin exhibits patches of scurfy redness. A few days ago she had shivering fits, and afterwards headache, with heat of skin, and erysipelatous redness and swelling of the face, which had increased so much by yesterday, that the eyelids were quite closed."

These symptoms increased, and were accompanied with "severe headache and a *small and rapid* pulse:" but, as there was nothing peculiar in the symptoms, I shall content myself with the recital of Mr. Lawrence's treatment of the case:—

* The "regular habits" of this young lady remind me of the character in the Spectator, who boasted of his "regular habits of irregularity," in never missing the going to bed with a "stitch in his wig" for forty years of his life.

"April 26th (day of admission).—Fourteen ounces of blood taken quickly from the arm.*

27th.—"Venesection to sixteen ounces."†

May 5th.—"Venesection to twelve ounces."‡

18th.—"Venesection to fourteen ounces."

12th.—"A dozen leeches were applied to the temples."

30th.—"Venesection to twelve ounces."

June 2d.—"Cupped on the back of the neck to sixteen ounces."

12th.—"Bleeding to deliquium."

13th.—"About twelve ounces of blood were taken;" also, on same day, "Eighteen leeches to the temples."§

14th.—"Repeat the leeches" (eighteen, I presume)||

15th.—"Twelve more leeches."

18th.—"Eighteen leeches to the head."

27th.—"Ten leeches were applied."

July 3d.—"Cupping on the neck to fourteen ounces."

4th.—"Twenty-eight leeches were applied to the temples with great relief."

23d.—"Cupping to fourteen ounces, and a dozen leeches in the evening."

August 3d.—"Twenty-four leeches to the temples."

"Discharged on the 7th of August, feeling and looking quite well."

This poor girl had again the *advent* (!) of Mr. Lawrence's professional services in the October following. "She was readmitted on the 19th" of that month, "having continued in excellent health till a week ago, when her left

* "A dose of calomel and jalap was ordered" on the same day. Pulse at this date "small and rapid."

† "A dose of calomel and jalap," &c.

‡ "Pulse small, and 140."—"An aperient draught immediately; saline medicine, with antimony, every six hours."

§ Report on 13th.—"She was very faint and ill for several hours after" (the venesection). "The headache continues. The pulse is still excited; skin hot, and tongue foul. She is thirsty, and complains of shooting pains across brow and forehead. She passed the night without sleep, and is now crying, and appears much distressed." For which state of things (pre-eminently symptomatic of loss of blood) "eighteen leeches to the temples, cold cloths to the head" were prescribed. I ought to state that calomel, antimony, digitalis, and jalap, had been intermittently employed for several days before.

|| Mr. Lawrence states in this day's report, "The catamenia have been suppressed since she came into the hospital, adding (artlessly enough) 'previous to admission she always menstruated regularly!' So good a physiologist as Mr. L., should not have forgotten that the menses require blood for their production, and that as *nihil fit!*

eye inflamed, the face swelled again as it had done before, and she became feverish." In addition to conjunctivitis and scleritis (with "considerable pain and copious lacrymal discharge on exposure to light"), "the nose is bright red; the lower eyelid and cheek are red and swollen. She has a frequent pulse, severe headache, foul tongue, and costiveness."

Treatment.—Loquitur Mr. Lawrence again.

October 19th.—"Twelve leeches to the left lower eyelid."

28th.—"The leeches were repeated." (Twelve, I presume.)

29th.—"The leeches were repeated." (Twelve, I presume.)

30th.—"Venesection to sixteen ounces."

November 3rd.—"Venesection to eighteen ounces."

9th.—"Twelve leeches to the temples."

12th.—"Venesection to sixteen ounces."

15th.—"Cupping on the back of the neck to twelve ounces."

19th.—"Twelve leeches to temples."

21st.—"Twelve leeches."

December 4th.—"Twelve leeches to the temples."

27th.—"Cupped to twelve ounces."

31st.—"The evacuation was repeated."*

Jan. 2d.—"She left the hospital by her own desire, being free from pain, with natural expression of countenance; regular bowels, and rather feeble pulse (f)"

Now, if the gentle and contemplative reader doubt that this is a "true copy," or suppose that I have "set down aught in" (scientific) "malice," I entreat him to turn to pages 90, 91, 92, 93, 94, 95, 96, 97, 98, 99, and 100, of the volume from which I have drawn these materials.

It may not, in sequel to this narration, be an unprofitable employment if we make a mathematical estimate, as far as such is attainable, of the exact quantity of blood which Mary Carter afforded Mr. Lawrence for the prosecution of his physiological experiments. The following table will afford a bird's eye view of the matter. The reader will remember

that demonstration was to be a prominent feature of the present undertaking.

How drawn—By lancet.

Date.	Quantity.
	lbs. oz.
April 26 . . .	" 14
" 27 . . .	" 1
May 5 . . .	" 12
" 18 . . .	" 14
" 30 . . .	" 12
June 12 . . .	" 1
" 13 . . .	" 12
	<hr/> 6 0

By cupping.

June 2 . . .	" 1
July 3 . . .	" 14
" 23 . . .	" 14
	<hr/> 2 2

By leeches.

May 29 . . .	" 6+
June 13 . . .	" 9
" 14 . . .	" 9
" 15 . . .	" 6
" 18 . . .	" 9
" 27 . . .	" 5
July 4 . . .	" 14
" 23 . . .	" 6
Aug. 3 . . .	" 12
	<hr/> 4 12
	<hr/> 13 8

From 19th October to 31st December.

How drawn—By lancet.

Date.	Quantity.
	lbs. oz.
Oct. 30 . . .	" 1
Nov. 8 . . .	" 2
" 12 . . .	" 1
	<hr/> 3 2

By cupping.

" 15 . . .	" 12
Dec. 27 . . .	" 12
" 31 . . .	" 12
	<hr/> 2 4

By leeches.

Oct. 19 . . .	" 6
" 28 . . .	" 6
" 29 . . .	" 6
Nov. 9 . . .	" 6
" 19 . . .	" 6
" 21 . . .	" 6
Dec. 4 . . .	" 6
	<hr/> 2 10
	<hr/> 8 0

Add quantity shown in first statement 13 8

Total 21 8

* It is enough here to mention, in a general way, that on this second occasion the lancet was supported by that faithful triumvirate, calomel, antimony, and a league, the evil doings of which, Mr. Editor, that, as one of the staples of our Commonwealth, you yourself to put down.

might that Mr. Lawrence would not for setting down the "bleeding his date, at 14).

the amount of blood drawn by fixed the work done at half I believe the ordinary cal-

It has thus been shown that, during a period of something less than six months, the unfortunate subject of this history was robbed of *twenty-one pounds and a half of blood* by direct abstraction, and God knows of how much more, indirectly, by drastic purgatives and violent sudorifics. One's hair stands on end with very horror at the narration! *Twenty-one pounds and a half of blood from a young lady of 19, of irregular habits, and of an unhealthy diathesis!* The pious Æneas, when the gods appeared to him in his dream and entreated him to fly from Crete, was not more affrighted than was I on perusing this account; and his soliloquy on that occasion scarcely exaggerates the expression of my feelings on this:—

*"Talibus attonitus visis ac voce,
Tum gelidus toto manabat corpore sudor!"*

Now, it has been estimated by physiologists (at a rough calculation), that the quantum of blood in a healthy individual bears the relation of 1 to 5 of his entire weight; so that if we suppose Mary Carter to have weighed 100lbs. (an estimate with which I presume the reader will not find fault), we observe that she surrendered the *last drop* in her possession (and *something more*) to her insatiable physician!

Mr. Lawrence, therefore, must be allowed the merit of having proved to a demonstration the truthfulness of Sangrado's doctrine,—"*C'est une erreur de penser que le sang soit necessaire à la vie.*" And these philosophers together may claim the credit of pointing out the fallacy of the Divine fiat, that "the life of all flesh is the blood thereof."†

Happy, indeed, should I be were these the *only* instances in which I felt myself called upon, by the interests of this important subject, to animadvert upon the consequences of what Mr. Lawrence must forgive me for calling his false

doctrines in connexion with inflammation.

The last five cases of erysipelas in Mr. Lawrence's paper are remarkable enough. It is essential to my present purpose to notice them.

"CASE XXXII.—Charles Neale, a knife-grinder, æt. 47, after having been observed to be altered and strange in his manner, first made a cut in his fore arm, and then a large deep wound, from side to side, in the upper part of the neck. The report does not state, although it is not improbable, that the man lost much blood on the occasion. On the day subsequent to his admission he was bled to 3xvi. for pain in the head and feverishness. Three days later he was bled twice, 'twelve ounces being taken in the morning, and sixteen in the evening;' and, on the following, had 24 leeches applied to the fore-arm, which had begun 'to look red, and to be painful.' The same number were repeated, two days later, 'for an extension of inflammation in the fore-arm, with fever.'"

"In three or four days the local mischief had increased, and assumed the form of phlegmonous erysipelas, affecting the whole fore-arm and ascending the arm." Suffice it to say, that to meet this emergency, Mr. Lawrence "made two incisions through the skin and cellular membrane of the fore-arm, extending nearly the length of the limb. Blood flowed from these, at first rather freely;" fainting ensued, and the patient died before the state of syncope could be overcome. Mr. Lawrence adds, that little more than twenty ounces of blood were lost on this last occasion.

"Mr. Lawrence observed to the pupils, that the fatal event of this case was one of those unusual occurrences, which neither the circumstances of the patient before, nor the examination after death, could elucidate. It might be compared to death from syncope in venesection, which had occasionally happened."*

In the daily detail of the preceding case, we are informed that the poor creature, the subject of it, had, after his self-inflicted violence, evinced "great uneasiness of mind, and a persuasion that he should not recover"—a state of

* Mr. Lawrence disbelieving in the existence of æsthenic or low forms of inflammation, the reader will scarcely be surprised to find this eminent surgeon entirely disregarding, in his treatment, the generally acknowledged axiom, that intemperance so modifies the healthy tone of the nervous system as to render the subjects of such peculiarity intolerant of loss of blood. This may not be an unfitting place to remark upon Mr. Lawrence's infidelity in relation to another canon in medicine. I allude to the well-established principle, that depletion is very differently borne in London and other large towns from that which obtains in country districts.

† Leviticus, chap. xvii. v. 14.

* Med.-Chir. Trans. vol. xiv. p. 176. This case is transcribed from the Lancet, in which it had originally been published.

things denoting how greatly the nervous system was implicated in the mischief going on, and eloquently pleading for the aid of stimulants and opium. The abstraction of large quantities of blood under such a state of innervation is at all times a most hazardous practice; and, sudden death ensuing thereon, a result not unreasonably to be looked for. It was long ago remarked by an illustrious writer—"Cor, vitæ atque roboris principium est; in eo anima, et natura ejus locatur. Quare *syncope*, cum cordis vitæque morbus sit, humanæ constitutioni planè adversaria est, et vinculorum quædam dissolutio est, quibus facultas continetur vitalis.*"

"CASE XXXIII.—John Cogan, a very tall man of *unhealthy* appearance, received a severe blow on the ankle, to which he paid no attention, until the foot and leg inflamed and became so painful that he was obliged to discontinue his ordinary labour." Was admitted under Mr. Lawrence; had *thirty* leeches applied to affected part; took purgatives and antimony, &c. Next day, *thirty* leeches more; and on the third, venesection to 14 ounces. Ammonia was subsequently administered. Still later, incisions were practised in the erysipelatous part, from which, through "the negligence of the attendants," much blood was lost, and the patient in the bargain.

"CASE XXXIV."† will demand from me a little more detail: a most instructive lesson may be learnt from it.

"Mr. James B——, æt. 36, of bilious habit, had been in very good health latterly, though subject to occasional inflammatory swelling of the glands

near the internal condyle of the right os brachii. On the 22d of February, 1827, he examined, at eight o'clock in the morning, the body of a patient who had died of phthisis pulmonalis."

In the evening he felt unwell, but, next morning early, he was sensible of an inflamed, swollen, and painful state of the glands, before alluded to. Rigors, headache, and so forth, came next; "the pulse was 90, soft, and in no respect *unpleasant (!)*" (Query, is Mr. Bullen from the *green* Isle, where one hears, occasionally, such expressions as an *elegant tune*, a *handsome day*, and, possibly, a "*pleasant pulse?*") "The breathing" was, we are informed, "heavy, with a sensation of oppression at the præcordia, the breath expired from the lungs being very hot, and as though boiling." "Countenance flushed; pupils rather *dilated*."

"Twelve ounces of blood to be abstracted from the neck by cupping; twelve leeches to the arm."

"11 P.M.—Twenty-four leeches to the temples;" hyoscyamus; cold lotions to head.

24th.—Arm being "more swollen and painful," with increase of pain and headache, patient had six leeches to inflamed part, and was bled to ten ounces.

25th.—"Arm not worse;" "considerable heat and pain about the head;" twenty leeches to temples; and, at night, venesection to twenty ounces.

27th.—A dozen leeches to the arm yesterday: twelve to temples, with cupping at back of head, to-day. *Erysipelas* had extended to shoulder.

28th.—The inflammation having extended to the body, Mr. Lawrence was now called in, who made two incisions in the arm, "from which from two to three pounds of blood were lost, when partial syncope came on, *during which several stools were passed*. He complained of loss of sight, together with an indescribable sensation of alarm, as he expressed it, in the whole line of the spinal cord."

March 1st.—In addition to partial loss of sight, and the other serious symptoms recorded in the last report, hiccough and stertorous breathing had now been superadded. "Dr. Back saw him" at 3 P.M., "and declared his belief that he had not lost a drop too much blood (!), and that his present symptoms did not arise from loss of blood." (!)

Mr. B. died on the day after.

* Aretæus.—"De Aretæi in rebus Anatomicis Scientia," p. 17.—Some years ago a case fell incidentally under my notice, which has left an indelible impression upon my memory. A fine athletic man, about thirty years of age, who was greatly addicted to the bottle, but who had none of the characteristics of the drunkard in his physiognomy, was overtaken by an attack of delirium tremens, of by no means a formidable character. When I saw him, I believed him to be convalescent (not being in attendance upon him); for he had left his bed, and was up and dressed. I conversed with him for some minutes, when he was not only rational, but appeared to me to be in his ordinary health. I subsequently learnt that, not many minutes after I had left him, he became delirious again, and died suddenly in his chair. I had no reason to believe that he was the subject of organic disease. But his nervous system was gone. Whiskey and the tropics together had literally "*unnerved*" him. The case occurred in the Isle of France.

† This case was communicated to Mr. Lawrence, by Mr. F. Bullin. *Vide Med. Chir. Trans.* vol. xiv. p. 190, et seq.

Mr. Lawrence remarks upon this case as follows. "The local and general means, although active, and followed up without delay, had altogether failed to arrest (query, *increased?*) the inflammation, and the treatment by incisions offered, in my judgment, the only chance of saving the patient. The adipose and cellular texture was found highly inflamed, *loaded with serous effusion*, and assuming here and there the yellowish appearance which precedes sloughing. There was no suppuration." "It is clear from the narration of Mr. Miller," adds Mr. Lawrence, "that the progress of the local mischief was arrested by the incisions, and that they produced, what all the previous measures had failed to effect, a very marked diminution of the inflammation. Whether the fatal event happened in consequence or in spite of them is a question on which opinions will differ, and light would probably have been thrown by examination of the body.

"It does not appear to me," Mr. L. continues, "that Mr. B. died from loss of blood. It has been suggested that he perished from the shock of the operation." This idea Mr. Lawrence thinks unsupported by the evidence, and I quite agree with him in the opinion.

"When we consider how often death has ensued from wounds received in dissection under various plans of treatment, we need not be surprised at a fresh instance of fatal termination. When we reflect, further, that both the local and general symptoms were severe in this case, and that at the time of making the incisions the inflammation not only occupied the whole upper extremity, but had even extended to the body, we may reasonably doubt whether the event of the case was in any way owing to that treatment."

The last case which I shall notice, though rather one of diffuse inflammation of the areolar tissue than of erysipelas, has an interesting relation to my present purpose.

A gentleman, *ætat* 64, a free liver, with a pallid complexion, and who suffered much from gout, sought Mr. Lawrence's assistance on account of stricture of urethra. While under treatment for this affection he indulged freely on two successive days in the pleasures of the table, and on the third exposed himself to a cutting east wind, which brought on fever, and an cedematous swelling of the throat. Twenty ounces

of blood, by cupping, were taken from the back. In spite of this, "the swelling about the neck and jaw increased, with difficulty of breathing." On the following day both these symptoms had increased, and become much more urgent. The heart was beating most violently. "He had not only felt, but heard, its violent palpitation: the pulse was about 144, and still rather full and strong." "I took eighteen ounces of blood from the arm: the landlady of the lodging-house had already applied leeches to the chest: the pulse sunk considerably; but the breathing, although easier, was not essentially relieved: the countenance was pale and anxious, and would alone have indicated great danger." The patient never rallied after this.

Such is a hurried sketch of the principles and practice of this eminent authority in relation to the disease we have in hand, as didactically paraded before the first scientific institution in the world for the approval and instruction of the profession at large.

Now, I hesitate not to assert (and I do so with the utmost deference to his high standing as a surgeon and a physiologist), that Mr. Lawrence has entirely mistaken and misunderstood his subject, and that it is out of his own mouth that I derive the evidence which condemns him. His own cases incontrovertibly prove this position.

"I am quite at a loss to discover in this affection those marks of debility which some have so much insisted on." "Indeed, speaking of the part, I am unable to recognize debility as the cause of any inflammation whatever; and in reference to the seat of disease, I regard the expressions of passive and asthenic inflammation and venous congestion as either unmeaning, or calculated to convey erroneous notions."—Pp. 28, 29.

Now, the very first case which Mr. Lawrence brings forward to illustrate his principles, and which I have extracted verbatim from his essay (p. 7—8), affords, in my opinion, so complete an overthrow of his entire fabric, that I would not desire a stronger argument to support me in an *opposite* view of the nature of erysipelas. A man, fifty-seven years of age, experiences a wound of the scalp, for which he is bled *eight* times from the arm in little more than as many days, *i. e.* between the 3rd and 14th of June. On the 18th, erysipelas appeared in the arm and neighbouring

part of the fore-arm! If inflammation bespeaks a necessarily *sthenic* state of the heart and sanguineous system generally, or of either alone, can any man in his sober senses believe that such a condition was set up in the present instance within four days after this enormous unloading of the bloodvessels? And yet such is the necessary—the unavoidable, conclusion from Mr. Lawrence's reasoning! There is no escape from this position.

Again, in the case of Mary Carter (the second I have related), the erysipelas of the face first made its appearance on the 26th of April; yet, despite the almost daily abstraction of blood from that date, the report on 27th of June states that "her countenance is *again swollen and bright red;*" and on 23rd July, "the face is *very red and swollen.*" She was discharged on the 7th of August, after the loss of *thirteen pounds and a half of blood;* yet, in the October following, so *stubbornly sthenic* was the state of her system, that the original inflammation broke out afresh, and demanded the removal of eight pounds more for its subdual!

Yet, Mr. Lawrence is "quite at a loss to discover" in erysipelas "those marks of debility which some have so much insisted on."

Mr. Lawrence's *sixth case** (not previously noticed) occurred in the person of a girl *æt.* 23. Whilst the fauces and one side of face are erysipelatous, "the pulse is *extremely rapid and feeble;* there were thirty-eight pulsations in a quarter of a minute as she sat in a chair after her throat had been examined. She is so weak that she cannot stand without support. The tongue is tremulous, and obeys the will imperfectly." "In the evening a state of stupor, with *increased determination of blood to the head, was found,* (!) and the insensibility seemed approaching to the state of coma; the pulse still rapid and feeble." The *erysipelas continued to extend the while;* yet, Mr. Lawrence regards the expressions of passive and asthenic inflammation as either unmeaning, or calculated to convey erroneous notions!

I cannot resist affording the reader a taste of Mr. Lawrence's quality in the science of "summing up," as disclosed to us in the sequel of the foregoing case.

"The advocates for the use of bark

and wine in erysipelas would probably have considered this a fit case for that plan of treatment. I felt doubtful, in the first instance, what course ought to be adopted, and gave conditional directions for the management of the patient. In spite, however, of the depressing circumstances to which she had been previously exposed, as well as of the *supposed evidences of debility* (!) which she exhibited on her admission, stimuli were obviously injurious, and she derived great benefit from repeated alvine evacuations."

Mr. Lawrence having himself established the fact that erysipelas will *arise* during a state of extreme prostration of the powers of life, and, of course, of those also of sanguification, as instanced in the preceding case ("the pulse is extremely rapid and feeble:" "she is so weak that she cannot stand without support"), and that the removal of thirteen pounds and a half of blood affords no guarantee against its recurrence, affords numerous examples of another no less important practical fact,—to wit, that the removal of blood and the decline of the disease bear no reciprocal relation to each other. In many of his cases, the more he depleted, the more did the erysipelas persist. This circumstance is strikingly shown in "Case 5th" (that of Mary Carter), as well as in "Case 34" (that of Mr. James B.).

Am I not justified, then, in the assertion that Mr. Lawrence has mistaken or misunderstood his subject, and that he has failed altogether in establishing the relationship between erysipelas and a sthenic state of sanguification? Nay, may I not go a step further, and assume that he has done good service to the real interests of this question, by the very facts which he has brought forward in support of an erroneous and mistaken theory?

But if Mr. Lawrence's views of the nature of erysipelas are incorrect and unsupported by the facts which he himself advances for their elucidation, how pernicious must not be the *practice* which they inculcate! And here I cannot help inviting attention to what to me appears to be a frequent misinterpretation, on the part of Mr. L., of *purely nervous phenomena* for those expressive of inflammation. In no other way can I account for his obstinate persistence in a depletory line of treatment in the face of such *manifest* and

* Loc. cit. p. 100.

dance as his own detail affords of its inutility and impotency in controlling the conditions to which it is addressed. Dr. Marshall Hall in one of his works has addressed a caution to the profession which should be engraven in letters of gold over the portals of every institution for the treatment of the sick, and indelibly, on the understandings of every practitioner,—which is, that *where one abstraction of blood has been unattended*

with good results, we should hesitate about the propriety of repeating it. I believe that no safer guide than this will befriend us in practice, and I earnestly press its importance upon the consideration of my brethren; for, let us not forget the admonition of Hippocrates himself to the physician—"To do good, or, at least, to do no harm." (Epidemic 1.)

[To be continued.]

**FURTHER OBSERVATIONS
ON THE
EXISTENCE OF OXALATE OF LIME
IN THE URINE,**

IN CONNECTION WITH STRUMOUS ENLARGEMENT OF THE LYMPHATIC GLANDS.

BY THOMAS BALMAN, M.R.C.S.,
Surgeon to St. Anne's Dispensary, Liverpool.

Formation of Dumb-bell Crystals of this Salt from the Decomposition of Lithic Acid.

In pursuing some inquiries relative to the histology of scrofula, and more especially that form of the disease characterized by enlargement of the cervical lymphatic glands, I was led by microscopic examination of the urine to notice the very frequent occurrence of oxalate

of lime as a constituent of that secretion.* Since this period, I have continued these investigations, in order to ascertain the comparative frequency of this salt in other diseases, and thereby, to some extent, test its value as a pathological indication of the strumous habit referred to. With this object I have examined specimens of urine in a great variety of cases; the majority from persons applying indiscriminately to the dispensary; others from the wards of the Liverpool Infirmary patients of Drs. Dickenson and Turnbull, who were so obliging as to assist me in procuring specimens for examination in every possible way.

The number of cases of which I have specially recorded notes amounts to 140; of these 28 were well-marked instances of strumous glandular affections of the neck. They are classed in the following order:—

	No. of Specimens examined.	Ditto containing Oxalate of Lime.	Oxalate of Lime un-mixed with any other Deposit.	Oxalate of Lime and Lithates.	Oxalate of Lime and Phosphates.	Per Centage of Oxalate of Lime in Cases examined.
Bronchitis	5	2		2		Per Ct. 40
Phthisis	21	10	5	4	1	47
Rheumatism	8	1		1		12
Organic lesions of the nervous system	7	2	1	1		28
Stomach affections	18	6	3	3		33
Bright's disease	3					
Affections of the skin	12	8	4	4		66
Secondary syphilis	10	3		3		30
Rickets & scrofulous disease of joints	11	4		3	1	36
Cases not referable to any of the above	22	9	3	5	1	40
	117	45	16	26	3	38
Scrofulous gland cases	23	17	10	6	1	74
	140	62	26	32	4	

* See MEDICAL GAZETTE for August.

The Relative Proportion of Instances in which Oxalate of Lime was found Mixed and Unmixed with Lithates will stand thus:—

The 117 Cases taken collectively.		Scrofulous Gland Cases.	
Oxalate of lime	45 give 16, equal to 35	Oxalate of lime	17 give 10, equal to 59
unmixed	per cent.	unmixed	per cent.
Oxalate of lime	45 give 46, equal to 57	Oxalate of lime	17 give 6, equal to 35
with lithates	per cent.	with lithates	per cent.

By the above it will be seen that, taking the 117 cases together, oxalate of lime is found in the urine in 45, or about 38 per cent. In the scrofulous gland cases the proportion is 17 in 23, or 74 per cent. There is one group of cases in which the oxalates appear to be very generally met with: I allude to skin affections, amounting to 66 per cent.: of these 4 were cases of impetigo, in all of which this salt was detected in considerable quantity; 5 were cases of psoriasis, in which it also appears not uncommon. In secondary syphilis I only found it in the more inveterate forms of the disease characterized by deep ulceration of the palate and soft parts of the throat.

As regards stomach affections, my observations do not show so large a proportion; and, indeed, I failed to detect the oxalates at all in a great majority of those severe dyspeptic cases with which it has been by other writers so commonly associated.

In disease of the bones and joints commonly regarded as scrofulous, the oxalates are very frequently replaced by earthy phosphates. In four cases of mollities ossium this was very remarkably exemplified; abundance of beautiful prismatic crystals of the ammoniacomagnesian phosphates hardly requiring the aid of a microscope, being found in each of the specimens.

This leads me to notice one or two interesting facts regarding the disappearance of oxalate of lime in phosphatic urine, which I do not recollect noticing elsewhere. In a former paper I mentioned that a large proportion of specimens in which oxalate of lime was absent from the urine were precisely those in which the alkaline or earthy phosphates were in excess. In the present series, taking the whole 140 specimens, it will be seen by referring to the foregoing table that oxalate of lime was associated, in four instances, with an excess of phosphates; once

only with a distinct sediment of prismatic crystals; and three times with a probable excess, as indicated by heat and its resolution by nitric acid. On examining the former specimen the following day very carefully, I failed to detect a single crystal of the oxalate of lime. Having some other specimens of oxalic urine at hand that had been standing for some time, I remarked that in those which had become putrescent, and therefore deposited phosphates, that the oxalates had likewise disappeared. To test this phenomenon more fully, I procured two specimens of urine from different persons, passed the previous night,—one containing octohedral crystals of oxalate of lime in considerable quantity, unmixed with any other sediment, and strongly acid to the blue litmus paper; the other, alkaline, and depositing prismatic crystals of the triple phosphates. On mixing the sediments of these two specimens in about equal proportions, I found that after standing forty-eight hours the oxalates disappeared, and the phosphatic crystals assumed a beautiful feathery appearance in place of the prismatic form. I have repeated this several times, and almost uniformly with the same results.

I have not observed the same thing to happen in urine holding phosphates in solution, nor so constantly when depositing the peculiar basic star-shaped crystals. In a few instances I have observed the oxalates to disappear in urine, neither alkaline or phosphatic, in a much shorter time—six to eight hours, but the exact circumstances in which this happens I have not been able to determine.

This, however, I have reason to believe is of no frequent occurrence, as commonly this salt remains for months in the same urine when unmixed with any other deposit, the liquid still retaining the urinous odour without the least sign of putrescency. This may not be alone peculiar to oxalic urine; but I

do not recollect to have noticed the same thing in specimens not containing this salt.

We have seen that oxalate of lime is rarely found in phosphatic urine, and my observations further go to show that the one diathesis does not succeed the other. The phosphatic may follow the oxalic diathesis. The reverse does not seem to happen in a general way—that is, oxalate of lime, as a constituent, very seldom alternates directly in urine frequently abounding with earthy phosphates. And here I wish to be understood as speaking of something like a permanent condition, as I am quite satisfied that many temporary variations, depending on a variety of circumstances, may occur at different periods of the same day; and so far my experience accords with the very elaborate researches of Dr Bence Jones.* Thus in scrofulous tumors of the neck I have found the oxalate of lime to persist in the urine for months together, until the health has become so deteriorated, and the powers of the system so reduced, that the earthy phosphates become of no unfrequent occurrence in this secretion: the oxalate of lime is then seldom met with, and rarely directly follows it. For example, one day a specimen may show a well-marked instance of the earthy phosphate; the next or following day, by procuring the urine passed separately by the same person at night and first thing in the morning, the former, for instance, may be quite free from the oxalate, or any other sediment, whilst the latter will show oxalate of lime and lithates associated together. Oxalate of lime, therefore, seems to succeed the phosphates only by the intervention of lithates. Dr. Prout arrived at much the same conclusions when he wrote the following:—

“The oxalate of lime diathesis is preceded and followed by the lithic acid diathesis—a circumstance which seems peculiar to these two forms of deposit; and, when taken in conjunction with the other circumstances already related, appears to show that they are of the same general nature—or, in other words, that the oxalic acid merely takes the place, as it were, of the lithic acid, and, by combining with the lime naturally existing in the urine, forms the concretion in question.”†

That the two deposits are frequently met with together in a great variety of diseases, the foregoing table, framed with some degree of care, abundantly testifies. It also illustrates one other point to which I wish to draw attention—viz., the comparative frequency of oxalate of lime as a constituent of the urine in the respective diseases mentioned, as well as the particular deposit with which it is commonly associated. In comparing together the 117 general cases collectively with the 23 strumous gland cases, a striking difference will be observable in each of these respects: in the former, for instance, the oxalate of lime is unmixed with any other deposit in 35 per cent. of the specimens; mixed with lithates in 57 per cent; whilst in the latter we have directly the reverse—unmixed, 59 per cent.; mixed with lithates, 35 per cent. In the one case, so far as my observations have gone, the oxalate of lime may appear and disappear with the lithates, or be of no long continuance in the urine; whilst in the other (scrofulous gland cases) it is, so to say, permanent—that is, continuing for weeks, or more commonly months, uninfluenced by diet or medicines of any kind. This appears to me an important distinction, the truth of which I have confirmed by careful and repeated examination of the urine, from time to time, of persons labouring under this unusually rebellious disease. They have been desired to abstain from sugar, and every other dietetic substance likely to be converted into oxalic acid during the primary assimilative process of digestion, without much effect: nitro-muriatic acid, given for a considerable time, had only the effect of throwing down a sediment of lithic acid, without producing any decided change in the quantity or persistence of this salt, at whatever period of the day it was examined. As a rule, I believe I may state—for I invariably procured two specimens, one passed at night, the other in the morning—the oxalates were always more abundant in the former, *urina clyli*, than in the latter, *urina sanguinis*. Most of the cases of this disease occurring among the lower orders of people in towns, are generally so tedious and protracted that a sufficient number of cases have not yet fallen under my notice to enable me to say positively that this salt disappears from the urine *pari passu* with

* Lectures on Animal Chemistry—Lawes.

† Prout on Stomach and Urinary Diseases, 2d ed.

the subsidence or removal of the enlargement; but in those cases that I have seen where the disease had got well, and the general health improved, the urine no longer exhibited any traces of the oxalates.

I have not seen the peculiar unaciform or dumb-bell form of these crystals, first noticed by Dr. Golding Bird, more than three or four times: they were in every instance preceded by and associated with octohedral crystals, and were rarely found to be of any long continuance, though the latter continued unchanged. That they are in reality some modification of the octohedral form, and may, like the latter, be formed readily by the decomposition of uric acid, the following experiment, I think, will be considered as conclusive. Take any specimen of urine of moderate specific gravity, free from all traces of oxalate, and add to it a drop or two of nitric acid: in the course of twenty-four hours lithic acid will be deposited at the bottom or sides of the vessel: decant the supernatant liquid, leaving about half an ounce with the crystals; add lime-water until the excess of acid is neutralised, and the fluid becomes turbid; then place it aside for a day or two: if a drop of sediment be then examined under a microscope, you will find perfectly-formed dumb-bell crystals of oxalate of lime, as well as the octohedral forms. In the first experiment there was nothing but dumb-bells beautifully striated for some days, since which I have almost invariably found them mixed with octohedral crystals. I was first induced to try this experiment from once observing a few very minute octohedral crystals of this salt mixed with uric acid in a specimen of urine to which a small quantity of nitric or muriatic acid had been added, and the liquid carelessly set aside for a considerable time. I afterwards repeated this in every possible way without the same success, when Dr. Brett suggested the addition of ammonia, which, however, did not succeed until the addition of lime-water in the way mentioned. I am not aware that any one has succeeded in forming these characteristic dumb-bell crystals in a similar way before. The fact is interesting in two ways: first, as showing that they are probably some modification of the common octohedral form; secondly, that oxalic acid is, as pointed out

by two distinguished chemists, Liebig and Wöhler, a derivative of uric acid; and hence the reason why these two substances are so frequently met with together.* There is one peculiarity in oxalic urine which I do not remember to have otherwise noticed. In two or three instances, specimens of urine, when brought to me, containing oxalate of lime, appeared to remain clear for a considerable time, though the day was not particularly warm, and gradually deposited a rather copious sediment. On examining this in the evening, after an interval of six or seven hours, under the microscope, the deposit was found to consist of nothing but lithate of ammonia and oxalate of lime. On re-examining the same specimen the following morning, the former had completely disappeared, and was replaced by the regular square-shaped crystals of lithic acid, the oxalate of lime remaining in pretty much the same quantity. Whether this arises from the presence of oxalic acid, or the development of a new acid in the urine, I am unable to determine.

Many microscopic observers who have paid much attention to urinary deposits will have noticed the not unfrequent appearance of an occasional globule of starch floating about under the field of vision. On one occasion I found a very copious sediment, consisting of hardly anything else but distinct starch vesicles. The specimen was obtained from a female about 20 years of age, a patient of Dr. Dickenson's in the Liverpool Infirmary. She was apparently affected with some unusual form of paraplegia, and in an extremely debilitated state. The case, I believe, was considered as functional disorder only. The first specimen had the appearance of purulent urine, and the sediment occupied nearly a sixth of the entire liquid. Having some doubts

* I have many times formed octohedral crystals of oxalate of lime by separating and repeatedly washing lithic acid with distilled water, so as to remove any of the other ingredients of the urine, and adding lime-water in sufficient quantity to dispel the greater portion of these crystals after standing two or three days.

In experimenting in this way I have sometimes noticed forms very like dumb-bells, but differing from them by disappearing almost instantly by the addition of acetic acid; and are therefore probably nothing but carbonate of lime.

I have likewise observed crystals of lithic acid, when submitted to this process, assume much the appearance described by Dr. Frick, of Baltimore, only that the central part of the crystal resembled the dumb-bell, whilst it still retained the general outline or framework of the original.

in my mind as to the source of this unusual product, I procured two more specimens, taking care to avoid every source of fallacy by providing perfectly clean bottles, and at the same time cautioning the nurse, who had her under her own more immediate charge. The specimens thus obtained had, however, precisely the same appearance. The sediment was not at all so copious as the former, and exhibited under the microscope a few crystals of oxalate of lime. Dr. Brett saw and submitted each of the specimens to the iodine test under the microscope with me.

I examined a third specimen after the lapse of a fortnight or three weeks, when the urine had become transparent, and without any appearance of this substance. It, however, contained a few large organic globules, epithelium scales, and a considerable quantity of oxalate of lime. In the meantime she had certainly very much improved in her general health and appearance.

I have endeavoured to represent these starch vesicles as they appeared under the microscope in the second specimen. They are all probably from wheat, being smaller than those obtained from common potato starch.



Starch vesicles magnified 200 diameters, and oxalate of lime.

Oxford Street, Liverpool,
Oct. 17, 1850.

POISONING BY NITRIC ACID.

DR. J. M. WARREN has reported the following case:—The patient was a negress, 34 years of age, of abandoned character, and took the acid at six P.M., on the 3d of March, thinking that she was three months pregnant, and wishing, she said, to destroy her child. The quantity taken into her mouth was reported to be three drachms, but most of it was spat out. Alkalies and mucilaginous drinks were used, but the burning in the mouth was intense during the night, with restlessness and delirium. The next morning she was brought from the jail, where the acid was taken, to the

hospital. Yellow stains were then observed upon the clothing, and the whole inside of the mouth and fauces, so far as could be seen, was of a deep yellow colour, the tongue looking as if covered with Indian meal; the respiration being painful, laboured, and stridulous, and speech almost impossible. Extremities cold; countenance of a leaden hue; pulse 120, and very small. For the first four or five days after her admission she suffered from soreness of the mouth and throat, dysphagia, thirst, and salivation, with some vomiting; she also complained of tenderness of the abdomen, but not particularly over the stomach, walking with difficulty and bent much forwards; but this was perhaps owing to her having been thrown down and stamped upon, in an affray, on the day on which she took the acid. After the first day or two she was much of the time up and about the ward: at the end of a week she was reported quite comfortable, and having some appetite; and on the 14th of March, as she was doing well, she was removed back to the jail, there never having been any fever, but rather a state of depression. On the morning of the 16th she was attacked with cramps in the stomach, and excessive pain and tenderness, which were partially relieved by opiates; on the following morning, however, she was found dead in her cell, with a great quantity of blood about her, and which she had apparently vomited.

On dissection, there was observed great rigidity. Upon the middle of the tongue a large, yellowish, smooth patch. Some redness of epiglottis. Œsophagus healthy for the first two inches; but below this it was found exceedingly soft, of a greenish yellow colour internally, purple externally, and full of coagulated blood. The stomach was in a similar, though much worse state: externally, it had the same purple colour, and was universally adherent to the neighbouring parts by recent lymph, except at the left extremity, where there were old and close adhesions to the spleen; internally, it was of a greenish yellow colour, emphysematous, and so perfectly softened and friable, that it could not be separated from the surrounding parts without giving way in every direction; the anterior face being detached from the rest of the organ to a great extent when the abdominal parietes were raised. Cavity filled with recent coagulated blood, and the open orifices of several vessels distinctly seen on inner surface. The intestine contained blood throughout the first two or three feet, but was otherwise sound, as were the other organs, so far as observed. Uterus not gravid.—*American Journal of the Medical Sciences* for July 1850.

MEDICAL GAZETTE.

FRIDAY, NOVEMBER 8, 1850.

In resuming our remarks on the process for extracting sugar by lead, we consider it unnecessary to quote the details of the Report issued by Professors Thomson, Graham, and Hofmann. These will be found in the Parliamentary paper already referred to. We may merely state that these gentlemen made the discovery, that in the mode of refining sugar which is at present generally adopted, traces of lead are to be found both in the bastards and treacle. This impregnation apparently arises from the use of leaden pipes in the ordinary processes of manufacture, and from the general practice of painting with white lead the inner surface of the metallic cones used as moulds for loaf-sugar. No lead whatever was found in four loaves of sugar refined in different operations by the ordinary process. The Report, however, renders it certain that the refined sugar, bastards, and treacle, generally acquire a trace of lead from the *new process* sufficient to be distinguished by chemical tests. We subjoin the conclusions:—

"That the lead found in the refined sugar is minute, the quantity not exceeding that occasionally acquired by the *bastards and treacle in the ordinary process of manufacture*.

"In the bastards of the new process the proportion of lead is not great, but sensibly exceeds the latter standard.

"The lead appears to accumulate in the treacle, but in no case that we have had the opportunity of observing to such an extent as would justify us in pronouncing the treacle to be poisonous.

"The inspection of the process on the large scale satisfied us that the lead may be removed from the sugar to a point beyond danger, provided the operations are constantly watched by a person competent to apply the necessary chemical test of purity to the products,

"In conclusion, although our results do not indicate certain and immediate danger from the use of this process in British refineries, still we must deprecate its extension, on the general ground that poisonous substances should never be used in the preparation of an article of food where they can be avoided; the object effected in this process by means of a poisonous material, the use of which is unquestionably attended with the possibility of grave accidents, being one that is attainable otherwise without danger.

(Signed)

"THOMAS THOMSON.

"THOMAS GRAHAM.

"H. W. HOFMANN."

On the assumption that the specimens had been fairly taken, and the analysis carefully and properly made, this Report was referred to Drs. Pereira, Taylor, and Carpenter, with a statement of the average quantity of treacle consumed by the poorer classes; and their opinion was requested on the probable effects of treacle derived from the lead process on the public health. We find from their Report that the quantity of lead which would be consumed by the use of the patent sugars was calculated as pure metal, probably in order to avoid all objections respecting the actual condition or state of combination of the lead in the sugar. Thus it might be in the mixed form of sulphite or acetate, or as sulphite only, according to the perfection with which the chemical process was carried on. The weight of metal being once determined, the equivalent proportion of any of its salts might be easily arrived at.

It is well known that the Royal Family of France, while residing at Claremont in the year 1848, suffered severely from all the symptoms of lead-poisoning by the use of water containing only a proportion of one grain of metallic lead in the Imperial gallon, or 1-70000th part; and taking this as a basis, the reporters draw the conclusion that the water consumed weekly by each individual, may have contained a quan-

tity of a salt of lead equivalent to 1.75 grains of metal. In from five to seven months this gave rise to most alarming symptoms of lead-poisoning in thirteen adult persons. Had the quantity of lead taken weekly been smaller than this, it is pretty certain, from the well-known cumulative properties of this metal, that it would have been only a question of time. With half of this quantity the effects might have been longer in appearing; but still, if past experience is to be trusted, they would ultimately have shown themselves. Mr. Herapath has stated, in a letter recently published in this journal,* that the use of water containing only 1-500000th part of lead—i. e. *one-seventh* of the proportion found in the Claremont water—gave rise to all the symptoms of lead-poisoning among the inhabitants of a village who used this water: hence it appears to us that the medical reporters have rather under-stated than exaggerated the effects which were likely to arise from the general use of the patent sugars. The returns of the consumption of treacle show that, assuming the chemical analysis to be correct, the quantity of lead consumed at a maximum may be as much as 1.261 grains, and at a minimum, about one-third of this quantity,—i. e., 0.43 grains. The Claremont people were poisoned by an assumed weekly dose of 1.75 grains, although the quantity taken may really have been much smaller. In Mr. Herapath's cases it is not probable that the quantity of lead taken weekly by each individual could have exceeded *half a grain*; and, to have taken this quantity, each person must have swallowed weekly the lead contained in more than *three imperial gallons* of water,—a hard trial to the stomach of one who is not a professed hydropathist. We are the more particular in stating these facts, because the patentees appear to rest their case on

the "innocuity" of small quantities of lead,—a mere delusion to all who, as medical practitioners, are acquainted with the highly insidious nature of lead-poisoning. They admit the facts, but demur to the medical inference,—because, they say, in these water-cases the lead is in the state of a highly poisonous carbonate; while, in the sugar and treacle-cases, it is in the form of the innocuous sulphite! This pretended difference will be a subject of consideration hereafter.

We subjoin the only extract which we think it is necessary to make from the medical report. It embraces the *argument ad hominem*, and goes at once to the probable effects on the public health, of the general consumption of the patent sugars:—

"Perhaps the best test which we can apply to the solution of a question of this kind is, to inquire whether we, by ourselves or our families, would knowingly consume an article of food which would have the effect of transferring weekly into our bodies one grain and a half, or even one grain, of a compound of lead. Our answer is, assuredly not. If we could procure the article of food free from such contamination, we should reject that which contained this proportion of lead-poison; and if we could not procure it in an uncontaminated state, we should prefer abandoning altogether the use of food which might be attended with injurious consequences to health. Even supposing that there were no facts to show that very minute doses of lead-poison taken daily for many weeks may seriously endanger health, the risk is too great to be incurred.

"For the reasons assigned above, it is our opinion that the treacle produced by Dr. Scoffern's process cannot be used as a daily article of food in the quantities specified in the Return, or even in smaller quantities, without exposing those who consume it to the risk of slow poisoning by lead. . . .

"The results obtained by the chemical reporters show that samples of sugar, bastards, and treacle, manufactured with the greatest care by persons well acquainted with Dr. Scoffern's process, do invariably contain a portion of lead,

which is chiefly washed into the treacle. Hence the test and counter-test advised in the process are not capable of showing that the sugar is free from lead, even when these tests are employed by experienced persons. If worked by inexperienced persons, a very large quantity of lead may remain in the syrups, and be diffused through the refined sugar, or carried into the treacle. *Without the constant supervision of a skilful chemist, the public health would be greatly endangered by the general consumption of saccharine substances so prepared*; and, even under this supervision, it would appear, from the analysis of the samples, that while the consumers of refined sugar might possibly escape, the consumers of *treacle* would be liable to suffer, after the lapse of some weeks or months, from the usual effects of lead poison."

Thus, then, the Chemical and Medical Reports confirm those already issued in the British Colonies, and published in our last number. Considering that six gentlemen, whose competency to form a judgment on the premises does not appear to be questioned, and who were employed by Government as a kind of independent Commission to examine and report on the new process without favour or prejudice, and solely with a view to the protection of the patentees on the one hand, and of the public health on the other, have thus publicly recorded their opinions against the new process,—we should have thought that the matter would have been closed, and that nothing further would have been heard concerning sugar refined by lead. If anything were wanting to confirm this view, it would have been the statement which has reached us, on good authority, that the eminent chemist, who happens to be French Minister of Commerce, M. DUMAS, has positively prohibited the introduction or manufacture of the patent sugar in France: and why a man in his position should wish to put a restriction on scientific improvements in manufactures, except

from their probable danger to public health, it is impossible to comprehend. Again, when the subject was brought under discussion at the British Association, in Edinburgh,* the assumption that Sulphite of lead was innocuous,—one of the points on which the supporters of the new process strongly rely,—was condemned as unsound by Dr. Christison, the first authority on Toxicology in Great Britain.

At a recent meeting of the Pharmaceutical Society the process of using salts of lead for the refining of sugar was pronounced to be unsafe by Dr. Ure, Dr. Golding Bird, and other chemists. This accumulation of adverse opinions, however, appears to have had only the effect of making the patentees more active in endeavouring to prove that the process is not only perfectly innocent, but actually safer—i. e., less noxious to health—than the ordinary method of refining sugar! They have put two pleas on the record:—1. That there is no sulphite of lead left in the sugar or treacle when the process is properly conducted; and 2. That, if any sulphite of lead by accident pass into the sugar from defective filtration or otherwise, it is as harmless as chalk, and the sugar may be consumed with impunity.

They have retained as scientific counsel in support of these doctrines, and against the six Government reporters, M. Dumas, and Dr. Christison, two gentlemen—namely, Dr. Gregory, of Edinburgh, and Mr. R. Warrington, of Apothecaries' Hall. Dr. Gregory proves to his own satisfaction that rabbits are *fattened and not poisoned* by the daily use of sulphite of lead! He, therefore, has something of a very original kind to communicate to the public and profession. Not so with Mr. R. Warrington, who occupies a sort of avuncular position in the case: this

gentleman, in a circular letter which has been widely published through the daily papers, carefully abstains from making any statements regarding his own experiments and results, but he adopts the very unusual course of mangling the chemical and medical reports, and, with the ingenuity of a special pleader, he reverses the facts, picks out the minimum in preference to the maximum quantities of lead, as telling more favourably for his clients, and proves, by a species of arithmetical legerdemain, that, while loaf-sugar of the common process contains lead, that of the patent process contains none; and further, that the bastards and treacle of the common process now universally consumed, contain twice as much lead as those saccharine products which have been actually procured by the use of lead-salts! The curious part of the case is, that he attempts to prove all this, not from any results of his own experiments, but from the facts and figures of Professors Thomson, Graham, and Hofmann. Either, therefore, these gentlemen are great bunglers in chemistry and dunces in arithmetic, or the scientific representative of pharmacy at Apothecaries' Hall has stumbled upon a mare's nest.

In another article we shall endeavour to ascertain which of these conclusions is the more probable.

THE ACTONIAN PRIZE OF THE ROYAL INSTITUTION.

WE again think it desirable to remind our readers that the next Actonian prize of £100 will be awarded, in the year 1851, to an Essay illustrative of the Wisdom and Beneficence of the Almighty on the Physiology of any one of the Senses (Hearing, Seeing, Tasting, Smelling, Feeling). Competitors for the prize are requested to send their Essays to the Royal Institution, on or before 10 o'clock, P.M., December 31st, 1850, addressed to the Secretary, and the adjudication will be made by the Managers on Monday, April 8th, 1851.

Reviews.

Synopsis of the Diastaltic Nervous System. By MARSHALL HALL, M.D., F.R.S.S. L. and E., Fellow of the Royal College of Physicians, Foreign Associate of the Academy of Medicine of Paris, &c. &c. 4to. pp. 100, with Illustrations. London. 1850.

Memoirs I. and II. on the Nervous System. By MARSHALL HALL, M.D., &c. 4to. pp. 113. London: Sherwood, Gilbert, and Piper. 1837.

OUR readers are familiar with the contents of the last of the two works above named, which comprises the early announcements of the facts and reasonings on which Dr. Hall founded what has hitherto been known as the "reflex theory." We shall therefore on the present occasion only bring it under notice as explanatory of the new terms proposed by the author in his "*Synopsis of the Diastaltic System.*"

In his preface to the latter work Dr. Hall observes:—

"To treat fully of the infinite number of topics which are here laid before the reader in mere outline would occupy a considerable volume; and for such a volume, in the present state of the profession, there would not be found readers; for 'monographs do not sell'—that is, are not read. The present cheap, elementary, and compilative state of medical literature will exclude for a time all original works. This *Synopsis* has been printed at my own expense. . . . Much more might be written upon this subject. The present has been too justly designated 'the age of medical degradation.' When members of our profession shall be really and fully imbued with all its literature, they and it will take the station which is due to them. But there are those who actually boast that they never read!—that is, that they are without science and without literature; that, for them, Prout and Liebig have laboured and written in vain! The result is, that our profession is indeed in a state of 'degradation!'"

There is much truth in these remarks of Dr. Hall. Compilations, manuals, &c., may be useful, even necessary; but compilers may take the place of, while they suppose themselves to be, original investigators and authors. But, as Dr. Hall truly adds, between the productions of such minds and those

of the minds of Harvey, Hunter, and Jenner, there is as much difference as there is between a painting of Raphael and an attempted copy. Whatever may be the ultimate fate of the "reflex" or "diastaltic" theory, there can be no doubt that Dr. Marshall Hall, has laboured in the spirit, and with the zeal and sincerity, of an original investigator.

We proceed to consider the peculiar or novel features of Dr. Hall's last publication. These consist almost entirely in the introduction of a new nomenclature. We extract the following passages as conveying its chief points:—

"On analysing the facts which have been detailed, I observed that the following anatomical relations are essential:—

"1. A nerve leading *from* the point or part irritated *to* and *into* the spinal marrow;

"2. The spinal marrow *itself*; and

"3. A nerve, or nerves, passing *out of*, or *from* the spinal marrow;—*all in essential relation or connection with each other.*

"On these anatomical facts I have ventured to institute a new nomenclature, descriptive of what I have hitherto designated the *spinal system*, and expressive of these essential points. The term *peristaltic* (from *περι* and *στρέλλω*, to contract) is familiar to us all. It may be justly extended to all the movements of the interior organs—as the heart, the stomach, the large and small intestines, the uterus, &c. These movements, it is well known, are independent of the spinal marrow. But it has been shown that a series of experimental phenomena—and it will be shown hereafter that a series of important functions—are effected by means of the series of nerves in essential connection with the spinal marrow to which I have adverted. The action is shown through the spinal marrow as its essential centre. I propose to designate the phenomena by the term *diastaltic*.

"The spinal system may henceforth be designated the *diastaltic nervous system*,—a designation which will have the advantage of including this system in the invertebrate, as well as the vertebrate, tribes of animals. This system embraces a peculiar anatomy, physiology, pathology, and therapeutics. Perhaps the only *purely* diastaltic function is *respiration*; and this is variously modified by volition, and influenced by emotion.

"We are much in need of other terms still to aid us in this investigation. The terms incident, excitor, and reflex motor, have been used to designate those nerves whose influence proceeds *to* and *from* the

spinal marrow. But they have *lity* and *appeared* to me satisfactory; and I *am* *wished* for others *more* expressive and explicit. The following compounds of *δῶς*, a way, have appeared to competent judges very appropriate to our subject:—*esodic* (*εσω*) will express the action *into*; *exodic* (*εξω*), the action *out of*; *anodic* (*ανω*) will express the *ascending*, *cathodic* (*κατω*) the *descending*, course of action; *pallodic* (*παλω*) and *panthodic* (*πανω*) will express the facts, on which I shall shortly have to dwell at considerable length, of the action of the *vis nervosa* from each *one* point of the diastaltic system, in *many*, or even *all* directions, to *every* other.

"Many important facts, many important truths, will be conveyed to the mind at once by these simple terms. Their root is already familiar to us in the word *periodic*; and several derivations from it have already been employed in the science of his creation, by the illustrious Faraday. It must be distinctly understood that they are *all* to be restricted to the anatomy, physiology, pathology, and therapeutics of the diastaltic nervous system, in which they express the modes of action of the *vis nervosa* distinctly from, and exclusive of, all other influences whatever—as sensation, volition, emotion, &c." (p. 4-6.)

Dr. Hall in the next place proceeds to detail experiments and demonstrations of his views in illustration of the new nomenclature. These, however, consist in great part of facts and experiments similar to those stated in his previous memoirs, which are well known to physiologists and pathologists, and the conclusions from which are disputed by many of the most eminent cultivators of that science. We shall, here, in justice to the author, present our readers with a brief abstract of Dr. Hall's statements on the anatomy and physiology of the diastaltic system.

Dr. Hall divides the nervous system into an *in-excitor* or *astaltic*, and an *excitor* or *staltic* portions. The former embraces the cerebrum and cerebellum, and the olfactory, the optic, and the acoustic nerves; the latter, the tubercula quadrigemina, and the new *class* of the *esodic* nerves, the spinal centre, and the *exodic* nerve, embracing the spinal nerves. The *in-excitor* portion is the seat of volition and of emotion, the latter approaching the excitor. Below the seat of emotion and passion, Dr. Hall places excitability and the diastaltic action; and evenly with these the peristaltic and secretory system, including the ganglionic system.

Anatomical views are set out in form by the author, and an ingenious mode of synthesis suggested, which shows the relations of different parts.

The physiology of the system the author represents as consisting in a series of arcs, formed of an esodic nerve, the spinal centre, and an exodic nerve, essentially linked together, and in diastaltic action. The appropriate stimulus of this action may be applied to any part of this arc, origin, centre, or termination. This position is illustrated by tabular statements of the diastaltic arcs of the iris, eyelid, respiration, larynx, deglutitious orifices and exits, ingestion, and egestion.

The exposition of the anatomy and physiology of the nervous system is followed by the consideration of the diastaltic theory to obstetrics and to pathology.

The views of Dr. Hall, as expounded in this volume, present much extremely interesting matter. They overlook too much, we think, the influence of the brain, and the intimate connection of different parts of the nervous system, by attributing all its phenomena to the spinal centre. The phenomena of disease and of mental emotion manifest a direct control to be exerted by the brain, which Dr. Hall's theory leaves unexplained.

In acknowledging the value of Dr. Marshall Hall's contributions to science, and admitting more especially the great degree of light that he has thrown on the functions of the spinal cord, we cannot refrain from expressing a wish that he could manifest a less quarulous and irritable disposition than is exhibited in the following extracts:—

"I will only add" (to an historical sketch), "in conclusion of these brief remarks, the expression of my regret that any of my professional brethren should be found of minds so ignoble as to persist in the injustice which has been so long, although vainly, attempted in this matter,—continually immolating, during the whole course of nearly twenty years, truth on the altar of misrepresentation and detraction."

In a foot-note to this passage the author names "Dr. Forbes's Review, Dr. Todd's Cyclopædia," &c.

In a pamphlet by Dr. Hall, "On the Neck as a Medical Region," which we

have before us, we meet with the following passage:—

"I am quite aware that neither the professional nor the public mind—they are, indeed, nearly on a par—are raised sufficiently for views so 'rational.' But then I do not write for the present day; and the day will come—and I shall promote its advent—when medicine will form a science, based on physiology, and calling in the aid both of theory and of observation."

Dean Swift wisely said, "It were chimerical to write for posterity, of whose tastes we cannot make any judgment, and whose applause we can never enjoy." If Dr. Hall be as anxious for the applause of posterity, as of his contemporaries, there can be little reason why he should fret under the scrutiny and hesitation with which his views have been received. It is natural that an original investigator should look upon his own labours with a partial eye; but he must know that all eyes have not the same point of view for every object; that other labourers may be at work in different parts of the same field; and lastly, he may remember that, even as regards himself, "*humanum est errare.*"

Contributions to the History, Diagnosis, and Treatment of Croup. By JOHN WARE, M.D. Pamphlet, 8vo. pp. 29. Boston, U.S. 1850.

THE author of this essay divides croup into four forms,—viz., membranous, inflammatory, spasmodic, and catarrhal,—thus treating as separate forms of one disease the different stages or degrees of severity of one or more distinct diseases. The descriptions, however, given of the several forms are accurately written, and would well serve as the basis of a different classification.

Dr. Ware warmly advocates Dr. Green's plan of applying a solution of nitrate of silver to the larynx.

We quote a few remarks on treatment, with which we in a great measure concur:—

"I am well satisfied, from what I have now seen of this method of treating croup, as compared with that which has been followed for so many years, that it has advantages which were pointed out in one of the preceding papers. It is a disease which I would treat without depletion, except perhaps a few leeches,—without vomiting, without purging, without blis-

tars, without antimonials, ipecac, and all those other nauseous remedies which have been usually resorted to. I would trust to opiates, perhaps calomel, emollients, and the local application of nitrate of silver."

Recherches sur l'Etat de la Contractilité et de la Sensibilité Electro-musculaires dans les Paralytiques du Membre supérieur, étudié à l'aide de la Galvanisation localisée. Par le Dr. DUCHENNE (de Boulogne). 8vo. pp. 39. Paris. 1850.

THE observations and deductions from the cases and experiments related in this pamphlet may be summed up in the following conclusions:—

When the electric force is limited to a single muscle, or single bundle of muscular fibres, the paralytic of the superior extremity arrange themselves in two classes. In one, the electro-muscular sensibility and contractility are diminished or entirely destroyed; in the other, the electro-muscular contractility is intact, and the electro-muscular sensibility is either normal, augmented, or diminished, and sometimes destroyed. In the first class are found paralysis from lead and from other lesions of the spinal marrow or spinal nerves; in the second, paralysis following on cerebral lesions, and rheumatic and hysterical paralysis.

1. Lead Palsy.

Certain muscles lose altogether the property of contracting under the stimulus of galvanism in the following successive order:—extensor communis digitorum, extensor indicis, extensor digiti minimi, extensor pollicis longus, extensores radiales, cubitales; abductor pollicis longus, triceps, and deltoid. This loss of power may affect any single muscle. Lesion of muscular contractility is usually accompanied with lesion of sensibility.

2. Palsy proceeding from Lesions of the Spinal Cord or Nerves.

In these cases the diminution of electro-muscular contractility is already observable towards the commencement of the second week.

The law laid down by Dr. Marshall Hall—"Irritability is diminished in spinal paralysis"—should be thus modified: lesions of the spinal marrow *almost always* produce loss or diminution

of electro-muscular contractility and sensibility.

3. Palsy following on Cerebral Lesions.

There is but little difference observed in the muscles in these cases.

4. Rheumatic Palsy.

The electro-muscular properties of the muscles are intact.

5. Hysterical Palsy.

The electro-muscular contractility of the muscles remains; their electro-muscular sensibility is diminished or destroyed.

A Manual of the Dissection of the Human Body. By LUTHER HOLDEN, F.R.C.S., Demonstrator of Anatomy at St. Bartholomew's Hospital. Part III. Small 8vo., p. 307 to 409. London: Hingley. 1850.

THE contents of this part of Mr. Holden's excellent practical manual are—the dissection of the abdomen, of the parts concerned in inguinal hernia, of the male perineum; a side view of the pelvic viscera in the male; structure of the bladder, prostate, &c.; dissection of the female perineum and pelvis; of the minute anatomy of the abdominal viscera; and the dissection of the lower extremity. These are all given clearly, and in as few words as will serve the purpose of the student. We can add our commendations of the present to those of the preceding portions of Mr. Holden's work.

A Treatise on the Climate and Meteorology of Madeira. By the late A. J. MASON, M.D., Inventor of Mason's Hygrometer. Edited by JAMES SHERRIDAN KNOWLES. To which are attached a *Review of the State of Agriculture, and of the Tenure of Land*, by GEORGE PEACOCK, D.D., F.R.S., &c. &c.; and an *Historical and Descriptive Account of the Island, and Guide to Visitors*, by JOHN DRIVER, Consul for Greece, Madeira. 8vo. pp. 888. London: Churchill. Liverpool: Deighton. 1850.

No pains have been spared by the friends of the late Dr. Mason, as will be seen from the names on the title-page of this work, to render this volume complete in its description of the climate of the island of Madeira. That the result has

been successful will be 'apparent' to all who consult the work. Tables and diagrams accompany the meteorological observations by Dr. Mason.

The entire volume constitutes an important contribution to climatology. The additions of Mr. Peacock and Mr. Driver contribute, we may state, very considerably to the character and completeness of the work. On every account we strongly commend it to the perusal as well of the general reader as of the visitor to Madeira.

Proceedings of Societies.

PATHOLOGICAL SOCIETY OF LONDON:

Oct. 15, 1860.

DR. LATHAM, PRESIDENT.

MR. CESAR HAWKINS related a case of *Strumous Disease of the Os Calcis, Femur, and Cranium, resembling Malignant growths*;

and exhibited two preparations from it of necrosis of the os calcis, in which the dead bone, infiltrated with pus, was still entirely connected with the living bone, although the disease had commenced eight years previous to its removal by amputation. The joints between the astragalus of the os calcis were partially ankylosed, and, although the astragalus itself was quite healthy, the joint between the astragalus and the tibia showed the commencement of ulceration of the cartilages; demonstrating, as Mr. Hawkins remarked, the manner in which the inflammation of the soft parts sometimes conveyed disease from one tarsal or carpal joint to another, making the removal of pieces of necrosed bone, or the excision of even an entire bone, not always successful. The case was remarkable, however, more from its history, as illustrative of the difficulty occasionally observed in distinguishing between scrofulous disease and malignant affections.

The patient, a young woman, was admitted into St. George's Hospital with a large phagedenic ulcer of the inside of the ankle, excessively painful, and requiring large quantities of opium for its relief, with much thickening of the soft parts, looking in the section not unlike scirrhus, and having in the centre some warty granulations, resembling the cancer of cicatrices (of which Mr. Hawkins has written a description), in the centre of which the probe

passed down into the joint between the astragalus and os calcis, without touching dead bone. After some attempts to heal these parts, amputation was proposed, but declined by the patient. After a few months a mass of glands formed low down, in the groin, as large as an orange, as if enlarged by the contamination from the apparent cancer below, which did not secrete like strumous glands, but which sloughed gradually away after previous ulceration of the skin; and as some were destroyed fresh masses formed by their side, which went through the same process of sloughing during two or three months. About the same time there formed at the lower part of the femur, on its inside, a firm elastic tumor, obviously connected with the periosteum, but without bony deposit, about four inches in length, and perhaps two inches in height; and soon after this a growth took place on the left side of the frontal bone, soft in the centre, and consisting apparently of a morbid growth in the diploe, making its way by pushing out the outer table, with a little absorption of the bone in the centre. Thus was formed a combination of appearances in every respect seeming to confirm the supposition of the malignant nature of the original disease, and quite sufficient to deter Mr. Hawkins from operation, which the woman was now desirous of. In two or three months more, however, and, as it seemed, chiefly from the use of iodide of potassium and sarcaparilla, all these latter growths disappeared; the tumor of the thigh gradually subsided, the glandular masses also sloughed out, and the part healed, the growth of the diploe went away, the bone recovered its former level, and the skin recovered from the sallow complexion it had assumed,—and amputation was performed. The patient was quite well a year and a half afterwards; and it is presumed, therefore, that the deceptive growths of the femur, and cranium, and glands, could not have been malignant, and perhaps not also the morbid texture in the ulcer of the foot. It seemed, therefore, not undeserving of the attention of the Society, in conjunction with the case about to be related by Mr. Hewett, in which a similar difficulty of diagnosis existed as to the strumous or malignant nature of the disease.

MR. PRESCOTT HEWETT presented a specimen of

Crude Tubercle in the Medullary Cavity of the Femur,

an affection which he considered to be a very rare one, more particularly in this country.

The preparation was taken from a man,

of about thirty years of age, who was admitted into St. George's Hospital a few years back with a tumor situated at the union of the middle to the lower third of the thigh, a careful examination of which proved that the disease was intimately connected with the bone, apparently springing from it, and occupying the greater part of its circumference. Somewhat globular in shape, this tumor was unyielding in its nature, but not painful when handled; its surface was generally smooth, and the circumference of the affected bone was larger in this part, by two inches, than that of the opposite one. The patient reported that, fifteen months previous to his admission into the hospital, he had, for the first time, begun to suffer considerable pain in the bone of a shooting character, which deprived him of rest; three months after this the swelling made its appearance, and gradually increased in size. At this time he also suffered from swelling and induration of the testicles, but this subsided, and then recurred once or twice, leaving the right testicle and chord somewhat enlarged and hard; his health had begun to fail about seven months after he was first attacked, and the pain in the thigh had, with few intermissions, been continual, and of a severe character. The countenance, which was anxious and sunken, was sallow; there was little or no appetite, and the general health was much broken. The prevalent opinion was that, in all probability, the disease about the thigh was of a malignant nature; no operation was proposed, the aspect of the case being too unfavourable; the patient was put upon sarsaparilla, and blue ointment was applied to the testis. In a few days the gums were affected, the countenance became very anxious, and the skin slightly jaundiced; he also now complained, for the first time, of great pain in the right hypochondriac region, where the liver was found to be much larger than usual, hard and very tender. Blisters were applied to the abdomen, the mercury was left off, and hydriodate of potash was ordered with the sarsaparilla. Under this treatment the general health began to improve; the hardness and swelling of the testis gradually disappeared; and in some little time the tumor of the thigh, when measured, was found to be less by an inch. These favourable symptoms continued steadily; and in a few months the man was made an out-patient, the hepatic symptoms being at that time but slightly marked, and the thigh presenting merely some general thickening of the bone at the part which had been affected. He was re-admitted the following year into the physician's wards for ascites; and while there was attacked with erysipelas, of which he died. At the

post-mortem examination extensive deposits of tubercular matter were found under the peritoneal covering of the right lobe of the liver, as well as in numerous parts of the sub-peritoneal cellular tissue generally. Similar deposits existed, also, in the structure of the kidneys, spleen, and lungs. On examining the parts which had been involved in the disease of the thigh, great thickening and condensation were observed about the cellular tissue uniting the muscles, and in that between the muscles and the bone; the periosteum, which was also very much thickened, presented on its free surface a large patch of tubercular matter enveloped in a dense cyst. The bone itself was irregular in shape, much hypertrophied, and very hard: at this part its medullary cavity was filled with tubercular matter surrounded by gray semi-transparent lymph, presenting very much the appearance of the well-known granule of the lung. No other bones were examined.

In addition to the rare occurrence of crude tubercle in bone, Mr. Hewett thought that the history of this case afforded a very good illustration of the great difficulties connected with the diagnosis of tumors affecting the osseous system. The more marked symptoms of the case certainly led to the supposition that the disease was of a carcinomatous nature; but this opinion was proved to have been erroneous by the disappearance of the tumor, which, in all probability, had been formed for the greater part by lymph effused around the tubercular matter found in the cellular tissue between the bone and the muscles.

Mr. ARNOTT exhibited a specimen of *Fracture of the Spinous Process of the Axis, with depression of the separated portion, death occurring one hour after the accident.*

A man, *æd.* 74, was admitted into the University Hospital July 13, 1850. He had for many years suffered much from pains in the head, and for which he had, at different times, been a patient of the hospital: they were described as most violent, recurring periodically, and were most relieved by immissions on the scalp; quinine, &c., being given without benefit. Twice he had suffered from symptoms of acute mania.

The day of the accident he had been occupied upon business of an anxious nature. On his return home he complained of feeling giddy, and immediately afterwards, on descending the stairs to the kitchen, he fell down to the bottom of the flight; when taken up was senseless and motionless, but soon recovered sufficiently to speak. He was admitted for this into

the hospital about forty minutes after the accident.

He complained of pain in the back of the head, on which was found a wound about half an inch long, and to its left a contusion of the scalp. No fissure was detected in the bone below the wound. He was sensible, but knew nothing of the accident; said he could not move his arms, and wished to be raised up; when either arm was raised up for him it dropped immediately, and he complained of no pain in them when pinched; he could move his lower extremities, when desired, with facility.

Right pupil moderately dilated, but acted freely; left eye diseased; pulse 90; breathing quiet; surface of body warm; complained of cold hands and feet, and desired them to be rubbed, though they were as warm as other parts of the body.

Blood in small quantities escaped from the mouth, which was not accounted for.

Soon after admission it became evident he was sinking, and he died twenty minutes after he came in.

Post-mortem examination.—No fracture of the skull; calvarium very thick and dense.

Dura mater.—Near the longitudinal sinus posteriorly were a few opaque patches. Glandulae Pacchioni not well marked; meningeal veins less turgid than usual. On dividing the dura mater on the left side about 3iss. of cere-sanguineous fluid escaped; the same on the right side, though less in quantity; the under surface of the dura mater pale, and covered with a layer of apparently recent lymph. On the left side the membrane was found much thickened over the lateral region, about two lines in thickness, extending upwards about one inch: this thickened part was divisible into two layers—the outer one of the thickness and consistence of the dura mater in other parts, the inner being thicker, of fibre-cartilaginous consistence, and above continuous with the arachnoid, where the opacity and thickness subside. Near the longitudinal fissure thin adhesions extend between the two layers of arachnoid. The longitudinal sinus contained a small clot posteriorly, and about 3ss. of dark fluid blood. The pia mater presented much fine ramiform and capilliform injection; the larger venous trunks rather turgid. On removing the membrane from the left hemisphere a small quantity of serum exuded, the surface of the brain presenting various amounts of punctiform injection; veins of the medulla congested; ventricles empty; choroid plexuses pale; commissures molles absent; fornix a little softer than natural. Weight of brain, 48 oz.; cerebellum, 5½ oz.

Neck.—On cutting down upon the cervical vertebrae, much blood was found extravasated between the muscles, especially between the oesophagi. On reaching the vertebrae, the spinous process of the axis was found broken off with a portion of its lamina, the posterior arch remaining entire. The broken-off spinous process was much depressed below the others, and wedged in between the arches of the axis and the vertebrae below it: possibly it had pressed on the cord; but, though the fractured edge nearest the cord was sharp, it had not injured the dura mater. The canal being exposed by the removal of the arches of the vertebrae, a considerable quantity of dark fluid blood escaped from the canal; and, as low as the sixth dorsal vertebra, blood was effused into the cellular tissue outside the dura mater. Around the origin of the nerves to the upper limb the amount was greater than elsewhere, the greatest being above the enlargement of the cord from whence these nerves are given off. The articulations between the axis and third cervical vertebra were very loose.

DR. PRACOCK exhibited

Two Specimens of Disease of the Heart.

1. One of these was an example of extensive aortic valvular disease. The specimen was from a boy, 18 years of age, who had been ill for nearly two years; and, during a considerable part of that time, had been a patient of Dr. Pracock's at the City Hospital for Diseases of the Chest. His illness was brought on by a sudden mental impression, which produced a fit, and was followed by symptoms of cardiac disease. In addition to the usual asthmatic symptoms, a loud murmur was heard with the systole of the heart, and this was followed by a feeble diastolic murmur. The murmur was most distinctly audible at the base of the heart, along the course of the upper part of the sternum, and on the right of that bone, and it was accompanied by a purring tremor, felt chiefly between the third and second, and the second and first right cartilages. The cardiac dullness on percussion was very greatly extended beyond its normal limits. The boy continued to be subject to fits of an epileptic character at intervals of two or three months, and he died suddenly. He had never had rheumatic fever, or any other serious disease before his seizure.

On examination, the heart was found of very large size, weighing 28 oz. avoirdupois. The hypertrophy and dilatation was very much confined to the left ventricle. The aortic valves, and especially the left and posterior folds, were extensively adherent at their angles of attachment and sides, and they

were also much thickened and indurated, so as to lessen the capacity of the aortic orifice. The anterior valve was studded with cretaceous vegetations, and was broken down so completely as to leave an aperture admitting the point of the little finger, by which a column of fluid freely regurgitated into the ventricle from the aorta. The aorta was much dilated in its ascending portion. The ventricular walls had undergone in places the fatty degeneration, and a small piece of bone was attached to the endocardium not far from the apex. The cavity at the apex was much expanded, and the walls were there very thin, and the endocardium much indurated.

2d. The second case was one of very advanced mitral valvular disease, removed from the body of a female, 39 years of age, who had suffered from two attacks of rheumatic fever within the last two years. She had been subject to palpitation and dyspnoea since the first attack, and had twice been a patient at the Hospital for Diseases of the Chest. She presented the usual symptoms of cardiac disease; and a loud murmur was heard with the systole of the ventricle,—most intensely at the apex, and then towards the left axilla.

On examination, the heart was found large, weighing 15½ ounces. The left auriculo-ventricular aperture was contracted to a mere chink, scarcely more than half an inch in length; and the folds of the mitral valve were thickened and rigid, and the cords much shortened.

Mr. TOYNSON exhibited a specimen of *Molluscum contagiosum* developed in the *External Auditory Meatus*.

The specimen was taken from a man, æt. 67, and the existence of the disease had not been observed during life. The tumor was about the size of a small nut, and occupied the posterior surface of the outer part of the meatus. The outer surface was of a pearl white colour, and shining. Upon being opened, the interior was found to consist entirely of epidermoid scales. The tumor projected backwards, had caused absorption of the layer of bone separating the mastoid cells from the meatus, and occupied a considerable portion of the mastoid cells.

Mr. AVERY presented a specimen of *Strangulated Femoral Hernia reduced en masse*.

The preparation was taken from the body of a woman, aged 60, who was brought into the Charing Cross Hospital on the 1st of September, 1850. Mr. Avery was absent from town when she was admitted, and did not see the case till the

post-mortem examination. Three days before her admission she had been seized with well-marked symptoms of strangulated femoral hernia, and was seen by a very intelligent practitioner, who persevered with the taxis until the swelling disappeared from the groin. He thought he had succeeded in returning the bowel safely into the abdomen; and the symptoms were considerably relieved at the time, and a free evacuation was procured by the aid of aperient medicines. Some few hours after, however, the pain and vomiting returned, and continued so obstinately that she was sent to the hospital. There she was treated with calomel and opium,—had leeches and fomentations, &c. applied to the abdomen. She gradually sank on the fifth day after her admission, passing from time to time fecal evacuations, which circumstance prevented any explorative operation being had recourse to, to ascertain the condition of the hernial sac.

On examination, the hernial sac, as large as a walnut, and containing a portion of small intestine firmly grasped by the neck, was found fairly pushed up, as raised, through the crural ring, and lay in the loose cellular tissue between that opening and the peritoneum. The neck of the sac did not embrace the entire circumference of the gut, which was proved by passing a director with care close to the mesentery, from the portion of intestine above the stricture into that portion below it. This latter circumstance accounted for the continuance of fecal evacuation, and deprived the poor woman of a fair chance which might have been afforded her by an explorative operation. Two or three inches of gut below the stricture were very dark, and bore the marks of constriction, and had been clearly contained in the sac a short time previously; probably, liberated at the time the sac was returned into the belly. At the bend of the thigh, in the situation the sac had occupied, there remained the condensed, cellular, and adipose covering, so well described by Mr. Luke, from which the sac had been separated. It was loose and flabby, as large as the end of the thumb, and had the appearance as if the hernial sac was still there. Mr. Luke, in his paper, has described five cases of reduction *en masse*, which he had seen; and my colleague, Mr. Hancock, has mentioned, in a recent work on Hernia, that out of eighty-four cases where the bowel was returned unsuccessfully, forty-four were of this character. It offers another instance where the operation external to the sac would probably have failed.

There was slight effusion into the cavity of the peritoneum; but the marks of peritonitis, and injury to the coats of the

bowel, were not considerable, and she seems to have sunk from exhaustion.

Dr. BRITH, R.N., presented a very interesting specimen of

Diseased Bladder and Dilated Ureter, with Calculi impacted in the latter.

A man, *et.* 53, had an attack of hemiplegia sixteen years ago, from which he never perfectly recovered. Three or four years ago he suffered from stricture of the urethra, for which he applied for relief; and in May last was attacked with hæmaturia and pus in the urine. Since that time has suffered almost constant pain in the hypogastrium and perineum, which occasionally assumed a violent paroxysmal form, accompanied with difficult and painful micturition. The urine alkaline, and loaded with pus and mucus, occasionally with blood. He died October 10th, having been in no way relieved by treatment.

Post-mortem examination.—The right kidney was converted into a large cyst four or five times the size of the healthy organ. It was filled with thick green fluid; from it there was a large opening passing down to the ureter. This latter tube was also considerably dilated, and its coats much thickened. Its lower opening into the bladder was entirely obliterated, so that the contents of the cyst and the ureter could not escape. The inner surface of both the cyst and the ureter was rendered uneven and irregular by numerous bands passing round portions of the walls, and constricting the cavities at these points. In the ureter this occurred to such an extent that it might be said to be divided into several compartments, one above the other. In the compartments three calculi were found imbedded, the largest of which occupied its superior third, and was about three inches long and three-quarters of an inch in diameter. The left kidney was about three times its natural size, but healthy in structure. The coats of the bladder were much thickened, and its cavity considerably smaller than usual. Its inner surface was studded with phosphatic deposits, and near its fundus was a fungoid mass as large as the palm of the hand. Examined microscopically, this mass was found to consist of triangular, oval, and other cells of a very irregular shape, resembling those found in malignant disease. The cerebellum presented a singular appearance; it was tough and indurated, and appeared to have been converted into a fibrous structure. The whole had an uniform straw colour, the appearance of the *arbor vitæ* being nearly obliterated. The brain otherwise was healthy.

Dr. JAS. EADON BISHOP exhibited a specimen of

Cancerous Disease of the Omentum, Mesentery, Pleura, &c.

A man, *et.* 60, of temperate habits, and who had enjoyed good health, was admitted into St. Thomas's Hospital, under his care, with symptoms of ascites and a chronic ill-conditioned ulcer, involving the ball of the great toe. He died five weeks after his admission, having suffered chiefly from dyspnoea when in the recumbent posture.

On examination, the abdominal cavity was found distended, partly by serous effusions, but chiefly by a large floating mass consisting of the enlarged omentum. The mass had a lobulated character, and consisted of clusters of smaller masses, the separate portions of which had, at first sight, very much the appearance of fatty nodules, resembling the appendices epiploicæ; but on cutting into them they presented a medullary appearance and consistence, and, when examined under the microscope, were found to be composed of globular, distinctly nucleated cells, intermingled with fatty matter. The mesentery was similarly affected throughout. The right pleural cavity was filled with clear serous fluid, by which the whole lung was compressed against the spine. Scattered over the serous membrane there were a number of small, grayish, lymph-like masses, which, on examination, were found to be of precisely the same character as the clustered masses in the abdomen. The bronchial glands, and the tissue of the lung itself, were healthy; and the left pleural cavity presented nothing abnormal: the liver was somewhat enlarged, and of a pale waxy appearance. The disease of the toe appeared to be also cancerous; and if this was the origin of the disease, it was worthy of remark that none of the abdominal glands were much, if at all, involved in the disease.

MEDICAL SOCIETY OF LONDON.

Oct. 26, 1850.

Dr. J. R. BENNETT, PRESIDENT.

Enormous Fatty Tumor.

THE tumor, which weighed fifteen pounds, was exhibited by Mr. HAYNES WALTON. Its great size rendered it remarkable. The man from whom it was removed was aged fifty; he had acquired notoriety from having been the inmate of several hospitals, to which he had resorted for its removal, but he always left before the intended operation. Just twenty years ago he observed the first

traces of its growth. He applied to him (Mr. Walton) in March of this present year, and begged him to remove it. At the time he had a slight cough, and was getting thin. His chest having been examined, it was suspected that he was suffering incipient disease of the left lung. He subsequently much improved, and his cough left him; and he begged of Mr. Walton to operate. After a consultation, it was determined that an operation was justifiable. There did not appear to be the development of any disease that forbade it—an opinion which had been expressed at all the hospitals to which he had applied. The operation was performed on the 21st of July, in the presence of several surgeons. The cast exhibited was taken a few days before the operation, and was a correct representation of it. It showed the tumor to be of great size, of a quadrilateral form; its situation was on the right lumbar and gluteal regions; its base measured three feet.

In the operation, the integuments were divided by two transverse incisions and a vertical one. The investing capsule was peculiarly dense, and so adherent to the tumor that not a particle of the connection could be torn through, but required to be carefully dissected, especially as strong septa passed between and tightly embraced the small lobes that covered its surface. A considerable quantity of blood was lost, not from arteries that fed it, for they were small and quickly tied, but from the emptying of the blood that was contained in the fatty mass. He was very low for several hours after the operation, but rallied wonderfully by the next day. The density of the capsule, its firm adhesion, the hardness of the fat, and the vascularity, were peculiarities, some or all of which might have been due to the many irritating substances that had been applied to it for years with the view of dispelling it.

On certain Affections of the Nervous System simulating Chorea. By Mr. PILCHER.

CASE I.—In August 1847, Mr. Pilcher saw a young man, aged twenty, in Berkshire, born of healthy parents, and living in a healthy district, who was affected with slight erratic movements of the upper limbs and face. There was also, apparently, extreme dyspnoea; the breathing was stertorous, and inspiration appeared to require the most laborious exertion of the muscles; expiration was less laborious. This had lasted for several months, without interruption, except during sleep. Walking was very difficult, though the patient could keep himself erect with very little assistance. He was treated in various institutions, and had now in a great measure recovered,—the following being his condition on October

20th, 1850:—The bowels, urine, and appetite were all well; sleep good; pulse rather quick; face flushed; he blushed easily, and considered himself very nervous. No cause could be assigned for the affection.

CASE II.—Mr. J. M., a respectable tradesman of Christchurch, in 1837, had a severe attack of bilious fever; and, in the next year, had occasional very slight "unpleasant" feelings in his head, when sitting in a confined posture. Two years after this, the feeling had become more intense; he could not sit at ease without putting his hand to his head, though without being able to assign a reason. After this, when he was sitting in a confined posture, the head would "give a sudden snatch" to the left side; and this was always prevented when he was able to keep his hands applied to his head. In 1842 the affection had become much more severe, but frequently did not occur when the patient most feared it. Various kinds of treatment—leeches, aperients, iron, nitrate of silver, electricity, &c.—had been tried; but with at most only temporary success, and the disease still continued unabated up to the present time. It was mitigated in severity by mild tonics and occasional aperients; and increased by nervous excitement, powerful purgatives, and depletion. There was an elevation of the right shoulder, with fulness over the right scapula; probably arising from the excessive action of the muscles of that side, especially the trapezius. The trapezius and sterno-mastoid muscles of the left side appeared partially paralyzed. The patient had felt no pain in the head or neck. Two analogous cases, in females, furnished to Mr. Pilcher by Dr. Davies, were also read. The patients had recovered under tonic treatment.

Mr. Pilcher believed that ordinary chorea was an affection of the excitomotor system, and mostly produced by eccentric irritation; but, in the cases now related no eccentric cause could be assigned: the disease seemed essentially functional. He compared the choreic affection, to a certain extent, with the hysterical affection of joints, described by Sir B. Brodie; the alliance appearing to exist in the disorders being similar in character, commencing in the brain, but affecting different parts of the nervous system; the sensory nerves being affected in one series of cases, and those of volition in another. That they were cerebral or mental affections, seems proved by their occurrence in persons of weak mind, or whose intellects, naturally powerful, had been overworked; and by the success obtained by renovating the mind, either through the medium of the body, or by directing it to more healthy occupations.

The tenderness of the spine had been observed by Mr. Filcher in the first two cases, nor could any mental cause be assigned.

ACADEMY OF MEDICINE, PARIS.

Oct. 22, 1880.

Arsenic and Adansonias digitata in Intermittent Fever.

DR. BERNIER stated that he had employed arsenic in chronic cases of ague attended with enlarged spleen, very successfully: cases that had resisted quinine had yielded to two or three doses of arsenic.

M. BOUVIER, in the name of M. Chatin, related several instances of the exhibition of the adansonias in intermittent fever, by which it was shown to have completely failed.

The Employment of Zinc in Painting regarded in a Hygienic Point of View.

M. BOUCHUT stated, as the result of his researches, that since the employment of zinc in painting is not attended with any of the evil effects which follow the use of white-lead, it should officially supersede the latter metal.

Iodine Vapour in Phthisis.

M. CHARTROULE read an essay on the use of iodine vapours, in which he attributed to this mode of the exhibition of iodine a superiority over other modes or forms of its combination.

BIOLOGICAL SOCIETY OF PARIS.

Monthly Summary, August 1880.

New Researches on Contractions of the Skin under the Stimulus of Galvanism. By M. BROWN-SEQUARD.

THE following conclusions are stated by M. Brown-Sequard in reference to the opinion of Kölliker that the *cutis anserina* is only produced by galvanism within a circumference of from half an inch to an inch; while M. Sequard had stated that it might be produced over an entire limb. The difference in opinion is explicable by the experiments of Kölliker having been performed with moist instead of dry conductors, as used by M. Brown-Sequard.

1. *Cateris paribus*, contraction of the skin is more distinct and extensive in proportion to the force of the galvanic current.

2. The conductors should be dry, or the current is readily conducted to subjacent structure.

The experiments of M. Brown-Sequard lead him to the conclusion that the contraction of the skin takes place from the action of the galvanic stimulus on its contractile fibres, and not through the medium of the nerves of the integuments.

Deranged Nutrition of the Eye consequent on a Lateral Section of Half the Spinal Marrow in the Dorsal Region. By the same.

Everybody, M. Brown-Sequard remarks, is familiar with the consequences to the eye, of section of the fifth pair of nerves within the cranium, or of removal of the superior cervical ganglion; the effects of worms in the intestines also, and of certain affections of the spinal cord in producing derangement of vision, and even amaurosis, are well known. M. Brown-Sequard observed analogous effects ensue in four out of nine cases where he had divided the lateral halves of the spinal marrow at the tenth, eleventh, or twelfth dorsal vertebra, in guinea-pigs, the morbid conditions being observed to follow on the side corresponding to the lesion.

M. Brown-Sequard has never seen similar effects follow, in both eyes, the complete division of the cord.

Microscopical Examination of Alkaline Urates from a Tarsal Articulation. By M. ROUGET.

The specimen was taken from the body of a woman, forty years of age, appropriated for dissection. The urate was found to be deposited in cells analogous to the cells of the pavement epithelium lining the synovial membrane. From this examination M. Rouget inferred the manner of the formation of this deposit, that it was a change in the secretion of the synovial membrane: the cells, being filled with the alkaline urate instead of being re-absorbed, and constituting, by accumulation and absorption of the fluid parts, the concretion known as tophus.

ACADEMY OF SCIENCES, PARIS.

Oct. 21, 1880.

Researches on Human Gestation.

M. COSTE read an essay, in which he stated the results of his examinations of the bodies of women dying suddenly from violence, or by suicide, at different periods of gestation.

In the bodies of all women dying suddenly during or immediately after menstruation, examined by M. Coste, he has found the uterus lined by so thick a mucous membrane that, if its constancy had

not proved it to be normal, it must have been regarded as a morbid product. This membrane was formed of glandules having orifices for the most part visible to the naked eye. In thickness it was equal to a fourth or a third of the muscular substance, and, in some instances, presented convolutions or folds pressed one against the other. In extra-uterine pregnancies this mucous membrane was thicker, and its convolutions as large as those of the cerebral surface, having a breadth not less than ten millimetres (= 1 centimetre, or .393 Eng. in.) From these facts M. Coste considered it demonstrated that every time an ovum arrives at maturity in, or is detached from the ovary of a woman, the uterine mucous membrane undergoes a change to prepare for its reception.

M. Coste has also examined the Fallopian tubes with a view to ascertaining whether in the pregnant state these remain free and pervious, or whether their uterine orifices become closed by a membrane which is to form the decidua. The result of his researches lead him to assert that their orifices remain open, and that the ovum falls into the cavity of the uterus, unless any abnormal cause obstructs its descent.

The changes which the ovum, &c. then undergo were thus stated by M. Coste:—

From the twentieth to the thirtieth day after conception the uterine orifices of the tubes communicate freely with the cavity of the uterus, the mucous membrane is thickened. The ovum, instead of being free in the uterine cavity, is buried in the mucous membrane; so that, on first opening the uterus, the existence of pregnancy might be doubted. Consequently, M. Coste considers that normal pregnancy may be said to be at first, interstitial growth. The ovum continues progressively enlarging, distending its covering of hypertrophied membrane; this distension going on, the ovum sensibly protrudes into the uterine cavity, attached by its opposite aspect to the base of the mucous membrane. The projecting portion becomes what anatomists have named the reflected decidua; that portion which remains attached to the muscular surface becoming the placental decidua; the rest of the mucous covering of the ovum becomes its parietal or uterine decidua. These three deciduas possess the same organisation as the mucous membrane whence they are derived, and it is only by the progress of development that they lose this character.

It is not, then, necessary, M. Coste remarked, to have recourse to the pseudo-membrane of Hunter, in order to explain the formation of these parts of the decidua: the development of the mucous membrane

of the uterus is sufficient to explain the phenomenon. If these portions of the decidua be but modified mucous membrane, they should be cast off after delivery,—and this is precisely what occurs; and the mucous membrane of the uterus is regenerated after all the debris of the decidua have been cast off with the lochial discharge.

This explanation of the nature of the mucous covering of the ovum, M. Coste considered, throws much light on certain otherwise obscure phenomena—*e. g.*, post-partum hemorrhages, the lochia, the propagation of puerperal inflammation by infection, and certain irremediable forms of sterility.

The Formation of Sugar in the Liver.

M. CL. BERNARD read an essay in which, after referring to the experiments of Magendie and other physiologists, which demonstrated the existence, sometimes, of sugar in the blood and other animal fluids; regarded by them as accidental and dependent on the nature of the food; it was shown that the presence of sugar in animal organisms is constant, and indispensable to the performance of certain regular functions; that the production of sugar is a constant and special function of the liver; that this function is under the immediate control of the nervous system; and lastly, that the blood which flows from the liver into the sub-hepatic veins invariably contains sugar in man and animals, whatever may be the nature of their aliment.

Nervous System of Insects.

A memoir was read by M. FELIX DUJARDIN, in which the following propositions were stated:—1. That in certain articulate animals a true brain exists, proportionate in structure and size to the intellectual manifestations. 2. That the brain or sub-oesophageal ganglion contains complex, symmetrical, pedunculated bodies, which vary in size according as intellect or instinct predominates. 3. The same pulpy substance of which these pedunculated bodies consist, is found exclusively to constitute the thoracic and abdominal ganglia that regulate and combine the instinctive actions.

LEGACY TO KING'S COLLEGE HOSPITAL.

THE treasurer of King's College Hospital has received the handsome legacy of £500 from the late James Bailey, Esq., and a further sum of £200 from the executors of the late W. J. Frodsham, Esq.; forming, together, a very seasonable assistance to an institution depending entirely upon voluntary bounty, and having so many demands upon its resources.

Hospital and Emancipation Reports.

KING'S COLLEGE HOSPITAL.

Stricture impassable to instruments—complicated with Perineal Fistula—Perineal Section.

A SHORT time since we reported an interesting case of stricture of the urethra, which was complicated by perineal fistula and by calculus, and where it was necessary to perform the operation of external section. Shortly after this patient had been dismissed from the hospital, another inveterate and intractable case of stricture presented itself, and was treated in a somewhat similar manner, and with the greatest success; and as the subject of treatment of stricture by external incision has of late been productive of much discussion amongst surgical authorities, we shall give an account of this case, and make some further remarks upon it.

The patient, a man about forty years of age, was admitted into King's College Hospital, under the care of Mr. Fergusson, in the middle of June last. The immediate cause of his admission was the fact of an immense swelling of the scrotum having suddenly taken place; he laboured also under sharp irritative fever, and he had great difficulty in urination. His history previous to this attack was as follows:—

When a young man he had gonorrhoea, and about seven years ago he first began to feel the symptoms of stricture. As year by year advanced he felt these symptoms increase, and the difficulty in passing his water became more marked. About fifteen months ago, effusion of urine took place suddenly, and he was taken to one of the London hospitals, where incisions were made in the perineum; and the patient recovered from this attack. Whilst he was under treatment in this hospital, no attempt was made to pass a catheter until three weeks had elapsed, at which period chloroform was given him and an instrument was passed, but he did not remain sufficiently long in the hospital to gain any permanent relief. A fistula remained where the opening had been made in the perineum, and this did not close. At the end of nine months this patient was admitted again into the same hospital, as the symptoms of stricture continued to be aggravated. A decided attempt was now commenced for the cure of this stricture by dilatation: an instrument of considerable size, according to the patient's own account, was passed into the bladder and retained there for the space of twenty days, when it was extracted;

but in the extraction there was great difficulty, as the end of the instrument had become thickly encrusted with phosphates, which, in the words of the patient, "looked just like an oyster shell." This operation gave the man excessive pain; and in all probability the urethra was much lacerated, as, on the next attempt to pass an instrument, only one of a very small size could be passed. Upon this the patient left the hospital again and returned to his work, and continued at it until the commencement of June, when suddenly one day he noticed that his scrotum had become greatly swollen. Upon this he sought admission into King's College Hospital.

On examination, it was discovered that the scrotum was immensely swollen and distended with some fluid. When a catheter was attempted to be passed, none could enter into the bladder, and there was a fistulous opening in the perineum. A free incision was made into the distended parts, and a large quantity of matter mixed with foetid urine was evacuated, to the great relief of the patient, who quickly rallied from the irritation which had been produced. It was now advisable to attempt to dilate the urethra, and Mr. Fergusson examined the patient with a catheter, but found it stopped at the bulb by a very tough stricture, which would not allow the smallest instrument to pass. Several attempts were made in the same manner to get a passage into the bladder, but none of them succeeded; consequently Mr. Fergusson determined to open up the perineum, and divide the stricture by free external incisions. This he was the more inclined to do as there was excessive irritability of the urethral canal, and there was great induration of the tissues of the perineum.

On Saturday, July 6, Mr. Fergusson performed the operation in the following manner:—The patient being put under the influence of chloroform, a catheter was passed down to the seat of stricture; a free incision was then made through the indurated tissues in the centre of the perineum down upon the urethra, and the point of the catheter was reached. The knife was now carried further along, and the whole extent of the strictured part was divided, until the surgeon was enabled to pass an instrument fairly into the bladder. There was no particular amount of bleeding, and no more difficulty in the operation than usual. The patient was placed in bed, and a gum-elastic catheter was retained in his bladder.

8th.—This patient has not had a bad symptom since the operation, and has suffered only the ordinary amount of sympathetic fever; pulse is 98, tongue clean; the catheter is producing no irritation, and the water passes freely away through it.

13th.—The catheter has been producing some amount of irritation; it was, in consequence, taken out of the bladder; but there was some little difficulty in withdrawing it, as its extremity was entangled with phosphatic deposits. Mr. Ferguson introduced another instrument, and ordered it to be retained.

16th.—Mr. Ferguson to-day introduced a No. 12 catheter into the bladder with ease. The wound is looking well, and is granulating in a most healthy manner; the patient is improving rapidly in his health; appetite is good, tongue clean, pulse good, and he is recovering fast. The catheter which was last passed could only be retained two or three days, as it produced considerable oedema of the prepuce; it was accordingly extracted, and the parts were allowed to remain perfectly quiet, and no other instrument was introduced. After this the wound rapidly healed. On the 22nd no water at all was coming through the perineal opening, and he could pass it along the urethra in a full stream. Everything continued going on well with this patient: an instrument of full size was introduced along the urethra once or twice more, and he was dismissed well in the middle of August.

This case presents, in many respects, features of interest, and especially as lately there has been considerable discussion as to the line of treatment which should be adopted in certain cases of inveterate stricture which will not yield to ordinary measures. Surgeons of all ages, and of every school in this country, have recommended that those cases which will allow of their removal by dilatation alone should be treated by the catheter; and the majority of cases which we have to contend with, either in hospital or private practice, are of this nature,—that is to say, in by far the greater number of cases of stricture of the urethra, a temporary cure, at least, can be brought about by the cautious employment of dilating force. But hitherto the most experienced and careful surgeons have now and then met with such obstinate cases of stricture—cases not only obstinate, but complicated to such a degree, either in consequence of their situation or extent, that they have found it absolutely necessary to resort to stronger measures than those afforded by the use of the catheter or bougie. Thus every now and then the surgeon meets with a stricture involving the orifice of the urethra or extending some way within it. In such an instance it is almost useless to attempt a cure by dilatation alone; the readiest way is to divide the stricture by a cutting instrument, and then to apply a dilating power subsequently:

by this means a cure will be rapidly brought about by an exceedingly simple operation.

Then, again, there are a class of cases which are most troublesome to treat, and which are likely to lead to the most disastrous results to the patient, if some permanent relief is not given; and they generally, if not attended to, go on to such an extent, that it is found needful to lay aside the bougie, and to employ the knife. The early history of the present patient is an instance of this. A stricture is met with, and forms about the bend of the urethra. No care is taken about it; the person indulges in his usual habits: year to year is passed in this manner; the obstruction increases in severity and extent; and, if application is made to a surgeon, in all probability no instrument can be passed through the stricture, either in consequence of its extent and hardness, or from the circumstances of the urethra being in a most irritable condition. Under such circumstances there is constant danger of the urethra giving way behind the strictured part; in consequence of the straining efforts the patient feels compelled to make. This does happen suddenly, and he is either carried off, or, by timely interference, he rallies; but a fistulous opening remains in his perineum, and, as an outlet is thus produced for the urine, some relief is obtained; the patient again goes about his occupation without attending to the disease. The continual trickling of the urine through the fistulous opening produces a chronic inflammation and thickening of the tissues of the perineum; the urethral canal becomes more and more obstructed, and at last the condition of the patient becomes so bad that he is obliged to seek relief.

In some of these cases, if there be no particular urgency of symptoms, and if the obstructed portion of the urethra is not very extensive, and there be not very great induration of the tissues of the perineum, an entrance may be obtained into the bladder by the catheter, if both surgeon and patient persevere for a sufficient period. And when this is effected, and the urethra has become fully dilated, the fistulous sinusses will close up, and all the bad symptoms resulting from the obstruction of the canal, and from the unnatural course which the urine had been taking, will disappear.

But, in some of these cases, matters have become so bad, that recourse must be had to some more powerful agent than the catheter. In consequence of ulceration having taken place behind the stricture, extravasation of urine occurs, and places the patient's life in jeopardy: if he recovers from the first attack, and yet no instrument has been got into the bladder, or the case

has been neglected, he is liable to its recurrence, as in the case of the patient just narrated. On two occasions this serious accident took place; and, on the last at least, an enormous quantity of pus and urine was evacuated by the surgeon. As the patient, therefore, had already suffered so much, and as all the evil consequences of neglected stricture had developed themselves, and, after careful attempts, it was found impossible to get an instrument into the bladder by fair and gentle catheterism, Mr. Ferguson considered it a proper case in which to perform the operation of external incision. He had had several instances in the hospital of a somewhat similar nature, when the operation had been followed by the most perfect success. The same good result fortunately took place in this instance. In a short time the urethra had become fully dilated, and the patient left the hospital entirely free from every vestige of the troublesome malady which had annoyed him for so long a time.

This case, together with the one we reported in this journal some time since, will probably excite some interest at this time, as there has of late been a free, and somewhat angry, discussion amongst certain parties respecting the propriety of cutting strictures at all. Some eminent authorities, and one in particular—Mr. Syme, of Edinburgh, have distinctly affirmed that the division of a stricture at the point of the catheter, in such a case as here narrated, is never necessary, and, in fact, is an unwarrantable proceeding on the part of the surgeon. These gentlemen state that no stricture is truly impermeable, and that, with sufficient patience, a surgeon may always get a passage into the bladder. We are by no means disposed to deny this assertion; we believe that a metallic instrument may, by a sufficient amount of force, be carried through into the bladder, however firm the stricture may be. There are others, again, who advise somewhat freely the external division of strictures; and it must be confessed that surgeons of the greatest eminence, from Le Dran down to the distinguished Liston, have carried this doctrine into practice in such cases as were deemed proper for such operation.

We have now had an opportunity which perhaps has not fallen to the lot of very many, of observing with accuracy and care the effects of the treatment of stricture by perineal section; and we are now fully impressed with the conviction that neither the one doctrine, that strictures ought never to be cut, nor the other, that perineal section is frequently necessary, and should be unsparringly carried into practice,—is either correct or safe to act upon. We have now seen several cases in which the patient was

brought into such a state of misery and danger, and the difficulties in the ordinary treatment were such, that it was found absolutely necessary to resort to the perineal section; and the operation has been followed, as we have before stated, by such excellent results, that any surgeon who would call such practice unwarrantable would either show great prejudice or want of experience. On the other hand, again, we have seen death resulting from the division of the urethra, even in cases where there has been but little difficulty in the operation; and this has happened in instances where it was by no means absolutely necessary to perform perineal section. In one case death resulted ten days after the operation which had been performed, when even a moderate-sized catheter could be passed into the bladder—an unconquerable argument against the performance of perineal incision in cases where a catheter can be previously passed into the bladder, as recommended by Mr. Syme.

As, then, it is found that death does occur after division of a stricture by external section, the surgeon is in duty bound to weigh all the circumstances of any particular case well before he puts this proceeding in force; and we are of opinion that in no instance whatever should the operation of perineal section be performed unless the most careful attempts have been previously made to introduce the catheter, or unless the patient be either in danger, or is particularly anxious to undergo it. In such a case as we have narrated above, the surgeon is perfectly justified in attempting to make a complete cure of the disease when he finds other means fail; and it will be, for the most part, found that the greatest success after operation ensues in those very cases which present the greatest difficulty, and where the disease has been of long standing, and great disorganisation of the tissues of the perineum and scrotum has occurred; for in such cases the system has, as it were, become accustomed to the irritation which has been constantly kept up, and it will be the better able to bear an operation. But in instances where there has been disease of a milder character, or of shorter duration, the system has not yet accustomed itself to constitutional irritation; and if a serious operation, as cutting through the perineum into the urethra, be performed, it will readily take alarm, and such excitement will be produced as will be sufficient to carry the patient off. Such, at least, has been the result of our own observation, after watching nearly twenty cases in which perineal section has been performed.

We believe, then, that the safest and most humane part to take in this much

discussed matter is to steer a middle course, and to act according to particular circumstances—to operate only in the worst cases, which will yield to no ordinary measures, and by no means to fly hastily to the knife when there is no actual necessity for it.

As for the proposal of Mr. Syme, to cut patients for stricture when an instrument can be already passed into the bladder, we confess it must be looked upon as a proceeding by no means justifiable, except on the urgent demand of the patient himself. We know of instances where fatal results have occurred after this proceeding, and others have been related where it was not only not attended with any good result, but where patients have been rendered worse, in consequence of the non-closure of the opening made during operation, and, of necessity, the existence of a perineal fistula. It appears that even in Edinburgh this method of treating permeable strictures has been denounced, and by a no less eminent surgeon than Professor Lisart, whose great experience and practical knowledge give great weight to any opinion he may express on so important a matter as the one under notice.

Medical Intelligence.

UNIVERSITY OF LONDON.

M.B. SECOND EXAMINATION.—1850.

Monday, November 4th.—Morning, 10 to 1.

Physiology.

Examiner, DR. CARPENTER.

1. In what does the process of *Digestion* essentially consist; what share in it is performed by the Salivary, Gastric, Pancreatic, and Hepatic Secretions respectively; and what are the results of its operation upon different kinds of alimentary material?

2. Compare the Physiological condition of Reptiles, Birds, and Mammals, as regards the relative activity of their *Circulatory* and *Respiratory* functions, and the influence of this activity upon the production of Heat and the general train of the Vital operations; and describe the mechanism concerned in each case.

3. What are the nerves concerned in the act of *Deplutition*; and what are the inferences you might draw from the circumstances under which that act may be performed (as shown in states of Disease, and by Experimental inquiries) with regard to the nature of Reflex action in general?

4. Compare the structure of the Com-

pound eye of the *Insect* with the Single Eye of *Man*, and show how each of them becomes subservient, as an optical instrument, to the reception of visual impressions: point out, also, the share which the *Mind* has in the ordinary exercise of the Visual Sense, and state how this may be proved.

5. What is the nature of the *Muscular Sense*; and what share does it take in the performance of Voluntary Movements?

6. Describe the successive modes in which the *Human Fetus* derives nourishment from its parent; and mention at what stages in the evolution of this plan it is checked in Fishes, Reptiles, Birds, and the inferior Mammalia.

Afternoon, 3 to 6.

General Pathology, General Therapeutics, and Hygiene.

Celeus de Re Medica.

Examiners, DR. BILLING and DR. TWEEDIE.

1. Explain what is comprehended under the expression *physical diagnosis*, as applied to the investigation of diseases.

2. Give an outline of the principles on which the treatment of spontaneous hemorrhages should be conducted.

3. When there is predisposition to gout or rheumatism (hereditary or acquired), sketch the hygienic rules you would recommend with a view to prophylaxis.

4. Translate the following passage into English:—

De fritione verò adeo multa Asclepiades, tanquam inventor ejus, posuit in eo volumine quod "communium auxiliorum" inscripsit, ut quum trium tantum faceret mentionem, hujus et aquæ et gestationis, tamen maximam partem in hæc consumpsit. Oportet autem neque recentiores viros in his fraudare, quæ vel repererunt vel rectè secuti sunt; et tamen ea quæ apud antiquiores aliquos posita sunt, auctoribus suis reddere. Neque dubitari potest, quin latius quidem et dilucidius, ubi et quomodo frictione utendum esset, Asclepiades præcepit; nihil tamen reperit, quod non à vetustissimo auctore Hippocrate paucis verbis comprehensum sit: qui dixit, "Frictione, si vehemens sit, durari corpus; si lenis, molliiri; si multa, minui: si modica, impleri." Sequitur ergo, ut tam utendum sit, quum aut adstringendum corpus sit quod hebes est; aut molliendum quod induruit; aut digerendum in eo quod copia nocet; aut alendum id quod tenuis et infirmum est. Quæ tamen species si quis curiosius æstimet, quod jam ad medicum non pertinet, facile intelliget omnes ex una causâ pendere, quæ demit. Nam et adstringitur aliquid, eo dampno, quod interpositum, ut id laxaretur, effecerat; et mol-

litur, ea detractis quod duritiam creabat: et implemus. Non ipsa, frictione, sed et cibo quipostea inquantum, cutem digestionem quidam relaxatam penetrat. Diversarum vero rerum in modo causa, est.

PASS EXAMINATION.

Tuesday, November 5.—Morning, 10 to 1.

Surgery.

Examiners, Sir STEPHEN HARRISON and Mr. HODGSON.

1. Name the different forms of Erysipelas: describe the appearances, symptoms, and progress, of Phlegmonous Erysipelas: state the most frequent causes of that disease, also its pathology: mention the principal modes of treatment which are employed, and state those which you should adopt.

2. Give the various dislocations of the Shoulder-joint, with the method of reducing each variety; and, in case of being unsuccessful in your first attempt, state the means to be pursued to secure a more fortunate result of other trials.

3. What is a Femoral Hernia? Describe the symptoms and treatment of a Femoral Hernia when in a strangulated state; and, when an operation is required, give the method of performing it, with the subsequent management of the case up to a favourable or any other termination.

M.B. SECOND EXAMINATION.—1850.

Afternoon, 3 to 6.

Medicine.

Examiners, Dr. BILLING and Dr. TWEDDIE.

1. Describe the local phenomena and terminations of inflammation.

2. Give a definition of purpura. Enumerate its varieties, and sketch the progress and treatment of purpura hemorrhagica.

3. Sketch the different forms of insanity. How is mania to be distinguished from febrile delirium, or from delirium tremens?

4. Describe the general and local symptoms of empyema, its diagnosis, terminations, and treatment.

5. Give an outline of the pathology and treatment of scarlatinal dropsy.

6. Describe the causes, diagnostic symptoms, and treatment of hydro-pericardium. Explain also how secondary effusions into the pericardium may take place.

Wednesday, November 6.—Morning, 10 to 1.

Midwifery.

Examiner, Dr. RIGBY.

1. Enumerate the symptoms of pregnancy; and then state which are the certain and which the uncertain ones.

2. Describe the last stage of natural labour, and your management of it.

3. Enumerate the indications for using, and the rules for applying the forceps:

4. Describe the various forms of contracted pelvis:

M.B. PASS EXAMINATION.—1850.

Afternoon, 3 to 6.

Forensic Medicine.

Examiners, Prof. BRANDE, Dr. PERRY, and Dr. RIGBY.

1. Describe the distinctive characters of oxalic acid, and the means of detecting it, whether free or combined, in organic mixtures.

2. How may the presence of chloroform be ascertained in the blood and in the organised tissues?

3. Describe the symptoms and treatment of poisoning by the essential oil of bitter almonds.

4. A pregnant female is suspected to have died from the effects of saving. On post-mortem examination a green powder is found in the stomach. By what characters would you recognise it to be saving?

5. Enumerate the evidences of recent parturition in a living woman.

6. Describe the condition of the uterus after labour at the full term of pregnancy.

SOCIETY OF APOTHECARIES OF LONDON.

QUESTIONS IN THE EXAMINATION FOR PRIZES GIVEN BY THE SOCIETY OF APOTHECARIES OF LONDON FOR THE GREATEST PROFICIENCY IN THE MATERIA MEDICA AND THERAPEUTICS.

October 1850.

1. STATE the process, and the theory of the changes taking place during the process, of the preparation of *Potasse Antimonio-Tartar.*

2. What are the diseases, and what are the doses, and the most beneficial forms of combination, in which this medicine may be administered? State, also, the doses and states of combination with strict reference to those diseases respectively, and to their stages and complications.

3. Enumerate the preparations of opium, and state the quantity of opium each contains.

4. Describe the preparation, and state the theory of the process in the preparation, of *Potassii Iodidum.*

5. What are the doses, the uses, and the physiological action of this medicine? State, also, the diseases in which the Potassii Iodidum is indicated, and the modes of exhibiting and of combining it for those diseases respectively.

6. Give a general view of the modes of operation, or physiological effects, of medicines.

7. Give a physiological classification of medicines.

8. Enumerate the sub-divisions of the class *Evacuants*, and state the several modes in which the sub-class *Diuretics* produce their effects.

9. What are the modes in which the sub-class *Expectorants* produce their effects?

JAMES CORLEND, M.D., F.R.S., &c.

First-prize, gold medal; awarded to Mr. F. W. Headland, King's College. Second prize, silver medal, awarded to Mr. D. T. Morris, Charing Cross Hospital.

BOTANY.

Examination paper, Wednesday, August 14th, 1850. Hours from 10 A.M. until 6 P.M.

1. "Singula, quæ Creator Ter Opt. Max. in globo terraqueo nostro produxit ordine et nexu inter se coherent mirabili, et a mutuis officiis conservacionem expectant perpetuam."—*Lin.*

Illustrate the above passage, by pointing out the connexions and dependencies of the Vegetable Kingdom.

2. What are the objects proposed by a natural method?

3. Why is the natural method to a great degree artificial?

4. Describe the structure of the anther in Lauraceæ, Berberaceæ, Ericaceæ, and Epacridaceæ.

5. Describe the structure of the gynoecium of gymnospermous plants.

6. What is our present state of knowledge respecting the fructification and germination of Ferns?

7. Describe the development of the ovary, the embryo, and the anomalous corollas of Ranunculaceæ.

8. "Urit amor plantas."—*Lin.*

How far is this true respecting the fecundation of plants?

9. Describe the structure, &c., of the stem and leaves in the specimens numbered 1 to 10.

10. Show the importance of the characters derived from the stem and leaves in the formation of natural orders.

11. Describe the specimens of fructification numbered 1 to 8.

12. Give the reasons for, and against, the admission of Rhizogens and Dictyogens as distinct Classes in the Vegetable Kingdom.

13. Give the character or characters which distinguish the following natural orders from each other, viz. —

Ranunculaceæ	from Magnoliaceæ
Nelumbiaceæ	" Nymphæaceæ
Gnaphalaceæ	" Compositaceæ
Malvaceæ	" Geraniaceæ
Xanthoxylaceæ	" Anacardiaceæ

Rosaceæ	from Myrtaceæ
Umbelliferae	" Araliaceæ
Lobeliaceæ	" Campanulaceæ
Verbenaceæ	" Labiatae
Apocynaceæ	" Asclepiadaceæ
Lauraceæ	" Thymelaceæ
Myricaceæ	" Betulaceæ
Zingiberaceæ	" Cannaceæ

14. Enumerate the principal medical products of the above orders.

N. B. WARD.

There are three Prizes given by the Society—1st. A gold medal; 2nd. A silver medal, and books on Botanical Science; and, 3rd. Books only.

The successful competitors are—first Prize, Mr. Frederick William Headland, King's College; second, Mr. George D. Brown, St. Thomas's Hospital; third, Mr. John Z. Lawrence, University College.

REAPPEARANCE OF THE CHOLERA AT ALGIERS.

WE regret to find, by letters lately received from Algiers, that the cholera has fallen like a thunderbolt on that place, and that in two days 150 persons were swept away.

THE PAYMENT OF MEDICAL REFEREES BY INSURANCE OFFICES.

IN reference to our recent remarks on this subject, it is only fair to state that one of the oldest offices in London—namely, the Royal Exchange Office—has set the liberal example of paying medical referees for their certificates. The Royal Exchange Office was founded in 1720, and does a larger business than the Sun, Rock, Eagle, and Atlas. There can be no doubt that it is a good and safe office for selection.

ROYAL COLLEGE OF SURGEONS.

THE following gentlemen having undergone the necessary examinations for the diploma, were admitted members of the College at the meeting of the Court of Examiners on the 1st inst.:—Messrs. George Phillips Bevan, Beaufort, Breconshire—Mark Edwin Bullen Nicholson, Cambridge—Mark Rowland Day, Oxford—John Frank Ross Lang, Newcastle-upon-Tyne—Adam Fletcher, Bury, Lancashire—James Edward Dickenson, Hart Street, Bloomsbury—Francis Robinson, Nova Scotia, North America—Henry Goode Wright, Hereford—Roger Lewis, Narbeth, Pembrokeshire—James McCaskey, Romford, Essex—and George Hugh Sidley, B.N. This gentleman passed as naval assistant February 7, 1845. At the same meeting of the Court, Mr. James Smith Ayerst passed his examination for naval surgeon. This gentleman had previously been admitted a member of the College; his diploma bearing date April 12, 1847.

APOTHECARIES' HALL.

NAMES of gentlemen who passed their examination in the science and practice of medicine, and received certificates to practise, on Thursday, 31st October, 1850:—Joseph Brady, Dewsbury—Reuben Sykes, Snaith, Yorkshire.

OBITUARY.

On the 4th inst., Joseph Freeman, Esq., of Spring Gardens, in his 59th year.

On the 5th inst., at 4, Mylne Street, Myddelton Square, John Thorne, Esq., surgeon.

BOOKS & PERIODICALS RECEIVED

DURING THE WEEK.

Principles of Medical Jurisprudence, designed for the Professions of Law and Medicine. By Amos Dean, Counsellor-at-Law, and Professor of Medical Jurisprudence in the Albany Medical College. New York, 1850.

On the Therapeutic Application of Electro-Magnetism in the Treatment of Rheumatic and Paralytic Affections. By R. Froirip, M.D. Berlin. Translated by R. M. Lawrance, M.D. &c.

Deafness practically illustrated. By James Yearley, M.B. &c. 3d edition.

Notice of an Unpublished Manuscript of Harvey. By G. E. Paget, M.D. &c.

The Studies of the Medical Man. By R. Lankester, M.D. F.R.S.

The Races of Man, and their Geographical Distribution. By Chas. Pickering, M.D. With an Analytical Synopsis of the Natural History of Man, by J. C. Hall, M.D. &c.

The Report of the Board of Health on the Supply of Water to the Metropolis. By J. N. Warren, C.E.

The Scottish Temperance Review. Vol. v. No. 2.

The American Quarterly Journal of the Medical Sciences. Philadelphia, October 1850.

The Dublin Quarterly Journal of Medical Science. November 1850.

The London Journal of Medicine. November 1850.

The Pharmaceutical Journal. November 1850.

Casper's Wehenschrift. Nos. 39 to 41, 28th September to 12th October.

BIRTHS & DEATHS IN THE METROPOLIS

During the Week ending Saturday, Nov. 2.

BIRTHS.		DEATHS.	
Males....	770	Males....	404
Females..	749	Females..	481
1519		945	

CAUSES OF DEATH.

ALL CAUSES	945
SPECIFIED CAUSES	930
1. Zymotic (or Epidemic, Endemic, Contagious) Diseases....	312
Sporadic Diseases, viz.—	
1. Dropsy, Cancer, &c.	41
2. Brain, Spinal Marrow, Nerves, and Senses	116
4. Heart and Bloodvessels	51
5. Lungs and organs of Respiration	138
6. Stomach, Liver, &c.	49
7. Diseases of the Kidneys, &c.	8
8. Childbirth, Diseases of Uterus, &c.	6
9. Rheumatism, Diseases of Bones, Joints, &c.	7
10. Skin	3
11. Premature Birth	30
12. Old Age	41
13. Sudden Deaths	10
14. Violence, Privation, Cold, &c.	33

The following is a selection of the numbers of Deaths from the most important special causes:

Small-pox.....	9	Convulsions.....	38
Measles.....	21	Bronchitis.....	59
Scarlatina.....	41	Pneumonia.....	61
Whooping-cough.....	20	Phthisis.....	113
Diarrhoea.....	19	Lungs.....	13
Cholera.....	3	Teething.....	15
Typhus.....	65	Stomach.....	2
Dropsy.....	11	Liver.....	12
Hydrocephalus.....	23	Childbirth.....	3
Apoplexy.....	17	Uterus.....	2
Paralysis.....	20		

REMARKS.—The total number of deaths was 14 below the average mortality of the 44th week of ten previous years.

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer	29.63
" " " Thermometer	46.3
Self-registering do. Max. 63° Min. 23°	
* From 12 observations daily. * Sun.	

RAIN, in inches, '01.—Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was 3.4 above the mean of the month.

NOTICES TO CORRESPONDENTS.

Mr. F. H. Hewitt.—A letter will be sent in reply to the enclosures received.

The Bed-side Sketches from the pen of Dr. J. C. Hall, of Sheffield, have been for the present postponed. Dr. H. has promised us that they will be resumed in the next volume.

We are compelled to postpone until next week the lecture of Dr. Bellingham, and the papers of Mr. France, Mr. Crompton, Mr. Hunter, and Dr. Snow.

Mr. H. Smith's address is also unavoidably postponed. It will be published in the following number.

RECEIVED.—Mr. W. Parker.—Dr. F. Behrend.

COMMENTARIUM.—In Dr. Jamieson's lecture, last week, page 745, col. 1, line 28, for "pathology," read "psychology."

Lectures.

LECTURES
ON THE
MEDICAL JURISPRUDENCE OF
INSANITY.

*Delivered in the Medical School of King's
College, Aberdeen.*

BY ROBERT JAMIESON, M.D.
Lecturer on Medical Jurisprudence in the
University.

LECTURE VIII.

Testimonial capacity of Lunatics—Civil responsibility in reference to contracts, marriage, &c.—When does insanity justify coercion?—Cases which do not demand the restraint of an asylum—superintendence always requisite—Lucid intervals—how judged of—Recovery—the marks of recovery—Discharge of Lunatics—liberation and emancipation—Criminal responsibility of the Insane—legal test—consciousness of right and wrong—power of self-control—insufficiency of the legal test—evidence of irresponsibility—how sought to be obtained—Medical opinion—how dealt with in law-courts and by the press—insufficiency of ordinary juries to determine upon insanity—necessity for an improved mode of procedure—Punishment of the Insane—mitigated penalties—dispunishable conditions—Does insanity diminish the expectation of life?

Testimonial capacity of Lunatics.—I concluded last lecture with some remarks on the testamentary capacity of the insane, and shall now allude to their testimonial capacity. Those who labour under amentia, mania, or dementia, are unfit to give evidence as witnesses, but the evidence of those labouring under partial insanity may sometimes be admissible. The testimony of a lunatic during a lucid interval in his disease, is received in reference to matters which have occurred during the existent intermission, and which he may be supposed fully to comprehend; but should a paroxysm have intervened betwixt the events and the time that the testimony is required, then his evidence will be held as of little or no value, because it is presumed that the mental attack may have dissipated or distorted his recollection. The testimony of a monomaniac about matters unconnected with his lunacy may be good, but so liable are the emotional faculties to be perverted

in such patients, and so difficult is it to be satisfied of the non-influence of the governing delusion, which may act in the most unsuspected ways, that the testimony, if uncorroborated, is of small value, and even though it stand the tests of recapitulation and cross-examination, should be admitted *cum nota* and with much caution. These are delusions which would incapacitate the witnessing of those possessed by them on any subject. I have more than once known one puzzled to determine without investigation whether the insane statement of an asylum patient were truth, mischievous falsehood, or pure lunacy; so unexpected, yet so plausible, circumstantial, and consistent, has the story been. The testimony of an individual who has truly recovered from insanity, regarding the circumstances of his illness or the events which preceded it, I believe to be very much more trustworthy than the evidence of a lunatic during a lucid interval, regarding even the circumstances occurring during that interval.

Civil responsibility of the insane in reference to contracts, marriage, &c.—Civil contracts entered into by those labouring under general insanity would be void, but in cases of partial insanity a legal agreement is not of necessity void if it be not the consequence of disordered mind, but fair and rational in its character, and accepted or proposed by one ignorant and unsuspecting of the existence of the insanity.

A marriage contracted in a state of general insanity would be illegal and void, as those afflicted by such a state are incapable of a lawful consent. In regard to marriages contracted by those partially insane, there is not an absolute rule. If the marriage were in any way a manifestation of insanity, as the result of insane delusion, or were contracted during a lucid interval by an interdicted lunatic, or were brought about by the collusion of interested parties, the insanity being on the one hand fraudulently taken advantage of, or on the other unsuspected or misrepresented, the agreement might be annulled at the suit of the party injured or misled. Very mad people do sometimes get wed. I have known an instance in which a female was married when in a state of absolute mania. She was under treatment and difficult to control beforehand, and much excited at the time of the ceremony, but next day she was perfectly ungovernable, and had to be sent to a lunatic asylum. Being in different circumstances her maintenance had to fall on the parish, but whether on her husband's or her own became an interesting question. The business went forward in spite of the representations of her medical attendant; the relatives insisting that she was merely what is some-

times called hereabouts "carried," and would get sobered down by being married; the bridegroom that she was crossed and irritated by her friends, and would become quiet and tractable when removed to her new home. You will find it stated, that the behaviour of the person at the time of the ceremony will form evidence of the mental condition and of the validity of the marriage; but in Scotland, where the ritual part of the affair is, as concerns the parties joined, a much more passive matter than it is in England, briefly and privately performed in the bride's house, there is not the same opportunity for extravagance to be manifested; and in this particular case the clergyman, who was unwitting of the true state of matters, although things looked something queer, saw nothing in them, as he thought, for the short time he was occupied, but what perhaps he had seen on other such occasions—the effects of a rather premature and incautious indulgence in good cheer.

When does insanity justify the coercion of the patient?—In what cases are we justified in certifying that an insane person's condition is such as to require his confinement in a lunatic asylum? It has been said that no lunatic should be placed under restraint who is neither dangerous to himself nor to others; but both medical and legal practice will authorise more than this. A lunatic may be lawfully restrained for his own or others' protection, for the control of his conduct, for the requisite care of his person, and for the treatment of his disease. You will be justified in certifying that a lunatic is a fit subject for confinement in an asylum upon any of the following grounds, if these cannot be efficiently dealt with by any other course: viz.—

- To protect him from external danger.
- To keep him from injuring himself.
- To prevent him injuring others.
- To prevent him from injuring property.
- To obviate a propensity to criminal, indecent, and riotous conduct.
- To guard him from maltreatment and neglect.
- To subject him to efficient treatment for the cure of his disease.
- To subject him to efficient treatment for the alleviation of its symptoms, when the disease is incurable.

Before adhibiting your name to "soul and conscience" documents, testifying confinement to be required, be sure that you recognise the existence of some of these foregoing grounds, in combination of course with the fact that the patient is truly insane. Imbecility of mind, or delusion, does not of itself justify restraint; it is the medical necessity of the case, or the

loss of control in the patient, which does so; and even these in special cases may be dealt with otherwise than by confinement in an asylum. In addition to the medico-legal question of restraint, there is a medico-ethical one in regard to its mode. If an insane person be quiet in his conduct, easily directed and superintended, labouring under an incurable form of the malady, and in such circumstances as to be able to afford all necessary expense for comfort and attendance, a medical man should be shy of becoming a party to the confinement of such an individual to an asylum. The good of the patient is the object to be kept in view. Station in life makes an important practical difference in the matter. A poor man when he becomes insane becomes a burden upon others for his maintenance, and however quiet, harmless, and incurable he may be, it is necessary for his due protection and comfort that he should become the inmate of a public hospital. Cases of senile dementia in the better ranks of life do not require confinement. In cases of puerperal insanity also, a lunacy certificate should be a matter of serious consideration, as this form of disordered mind is most frequently curable, and that within a short period. In the better ranks insanity arising from such a cause is more likely to be speedily removed by judicious medical treatment at home or in a private family than in an asylum, and the patient will after recovery be in every case found to be highly pleased that the one course was adopted in preference to the other. I might go on to cite many examples in proof that it is not always either good judgment or fair play to consign a lunatic to the safe keeping of the walls of a lunatic asylum, though it must be admitted to be in most instances, and on the patient's own account, a right, a politic, and an unavoidable measure.

Although restraint in an asylum is not to be accounted necessary in every case of insanity, superintendence and control is requisite in all. Every one who is lunatic ought to be reported to and known by the authorities, if not under confinement; and no one labouring under insanity ought to be permitted to wander about at large by himself, for it is impossible to predict the actions to which he may be impelled in consequence of his mental disease. Although we may be supposed to have sufficient knowledge of the features of his case to measure the extent of his understanding and his amount of self-control, and imagine ourselves able to enumerate all his delusions; yet we cannot foretell or reckon upon the morbid actions to which he may be suddenly led, by insane ratiocination or diseased impulse.

Lucid intervals.—What constitutes a

lucid interval? During what is termed a lucid interval, the lunatic is held to resume the civil position of a sane person, and may enter into an agreement, give evidence, make a will, &c. He is also presumed to be again responsible for his actions. A lucid interval is an intermission of the disease for a longer or shorter period, and is not a necessary, neither a common occurrence, even in acute cases. Such intermissions are never observed in amentia, imbecility, or chronic dementia,—are very rare in monomania, and occur most ordinarily in cases of maniacal excitement. They have to be distinguished from remissions. Remissions have that frequency amongst the insane, which legal writers falsely assume for lucid intervals; wherever there exists any degree of excitement or depression, there are remissions. They are observable, therefore, in the great majority of lunatics—in indeed in every case of insanity, whether chronic or recent, which does not simply consist of congenital mental deficiency: remissions are very common, lucid intervals rather rare. In judging of a lucid interval, the best course to pursue will be to get the patient to exhibit his powers of mind in writing, and then to have a personal interview, in which the history and features of his own case should form the chief matter of conversation. If the personal examination be satisfactory, and also the character of the epistles, and if you find that his feelings and emotions are no longer morbid, but natural, you may safely certify that you consider the condition to be at least a lucid interval, but I would advise you in no case to do so solely upon the grounds of apparent intellectual sanity—the mere absence of incoherence and delusions. A first lucid interval is not to be distinguished from a recovery by its features, but may frequently be suspected from the suddenness of the improvement. The shorter the interval which marks the particular case, the less are you to depend upon the integrity of the mind during the state of calm. The truest lucid intervals most usually occur about regular periods, and last for several weeks or months; they are generally of longer duration than the paroxysms, but as the disease progresses they come at longer intervals and less regularly, they have a shorter duration and are less perfect, having more the character of remission than intermission. The first fully developed intermission is the interval which is most lucid. There is occasionally a very short lucid interval preceding death, which I have not myself witnessed but in some instances of recent acute mania, in which the excitement was so violent as to prove speedily fatal by the exhaustion of nervous energy. The patient, after suffer-

ing for two or three weeks from a state of sleepless, unceasing, and extreme frenzy falls into a typhoid-like condition, manifests a return of soundness of mind, and shortly expires.

In regard to lucid intervals the law exercises a praiseworthy caution. The evidence of sanity must be as clear as were the proofs of disease. Though the validity of a will executed in such a condition has been frequently upheld in law courts, there is no example of any one having as yet been punished for a crime committed in such an equivocal state of soundness, as an intermission of insanity must be considered to be. In all lucid intervals which are not actual recoveries, it is to be presumed that though the symptoms of the malady are for the time inappreciable, pathological causes of the disease still remain. The flame has gone out, but the inflammability continues, and the slightest cause may be sufficient to evoke the fire again before it would of itself burst forth. "Burdened as the criminal law is," says Dr. Ray, "with false principles on the subject of insanity, the time has gone by when juries will return a verdict of guilty against one who is admitted to have been insane within a short period of time before the criminal act with which he is charged."

Recovery.—When is a person to be held as having recovered from an attack of insanity? The following are the grounds which will justify this conclusion.

First, The return of the natural complexion and expression of countenance, and the disappearance of the signs of related physical disorder.

Secondly, The absence of intellectual disorder,—delusion and incoherence; the patient being capable of recognising his insanity, and conscious of its removal.

Thirdly, The absence of excitement, manifested in the return of adequate self-control.

Lastly, The restoration of the emotional faculties to their natural character, the return of the benevolent emotions, the feeling of sympathy for others, the love of kindred, &c.

Such changes in the patient indicate the removal of the pathological causes of the disease, the re-establishment of the understanding, and the restoration of his moral faculties to their healthy tone. When they all concur, you may be satisfied that the patient has recovered from the attack of insanity, but if any one of them be wanting, you must consider that an essential step still remains to be made. It very seldom happens that a patient recovers in one or two of these points without recovering in them all, but it does so happen occasionally, and gives rise to no little difficulty in

determining upon the continuation of restraint.

When once a person has been found lunatic by commission, or his insanity declared in any other published form, the law is very jealous of allowing him to resume his civil freedom, and requires the clearest substantiation of the fact of recovery, before the commission is superseded and free enlargement allowed. To certify a lunatic's recovery you ought to make as scrupulous and careful an examination as you have been instructed to do before becoming concerned in a certificate of lunacy. Be careful not to mistake partial for complete recovery, and remission or intermission of the disease for absolute removal. Unless the patient has recovered in every particular above mentioned, you may be certain that he will not remain stationary after his enlargement, a man with a new personality; and in the generality of instances the chances of relapse are much beyond those of confirmation. As a general rule, you may be doubtful of the stability of recovery directly as its suddenness. If the disease has come on gradually, be sure that it will be still more gradual in its retiring. Every cure has to be tried by the test of time, and should the disorder have manifested the recurrent type, the proof must be a slow one in order to distinguish between lucid intermission and recovery. The distinction of cure and intermission is however more medical than medico-legal, but it is one of importance in reference to the full emancipation of the patient.

Discharge.—When should a lunatic be removable from confinement? It is to be wished that the law with us in Scotland were somewhat explicit on this subject. In the present state of matters, any insane person who is confined in an asylum may be removed at any time by those who have obtained his confinement; the consequence of which is, that many who are progressing favourably enough may be made, through the mistaken conduct of friends often not strong in judgment themselves, hopelessly incurable, and not a few come to perpetrate criminal atrocities. No lunatic should be consigned to an asylum who does not need to be so, but having required this, for any of the reasons previously alluded to, his enlargement should be obtainable only by petition and warrant on sufficient grounds, in the same way as his constraint was effected. In addition to the plea of recovery a patient might be removable for better treatment elsewhere, or be discharged because he is incurable and harmless, or in a lucid interval which former experience has shown may be longer than his paroxysm, but in no case, unless in that of thorough recovery, ought discharge from confinement

to imply full emancipation from control. Even in some cases of recovery it would be well if perfect freedom were to be interdicted, as, for example, when insanity has resulted and recurred from drunkenness, for in every such case, without careful superintendence, discharge is certain relapse, and freedom a foolish and a cruel gift, precipitating its unfortunate possessor, with every exercise of it, deeper and deeper into the darkest gulph of madness. There should also be a control exercisable by competent authority over the managers of public asylums, to prevent them also from dismissing lunatics, unless upon grounds that are satisfactory to those who have entrusted them to their care, and to the sheriff on whose warrant they accepted the trust.

Criminal responsibility of the Insane.—Theorists have occasionally run mad on the subject of criminal responsibility. Some have held that punishment should in every instance overtake crime by whomsoever committed; others that punishment should follow crime in no case, all crime being a result of faulty or diseased organization. Dr. Heinroth of Leipsic's insanity was, that all disease depends on sin, therefore instead of being a plea for exemption from punishment, is but the more a proof of guilt.

An English judge tells us, that medical men have the delusion that insanity renders a man dispensable, which is a delusion of the bench; such a mistake is not to be found recorded by any English medical authority. The insane we all know may be legally punished, whether justly or not. There is an insanity which annuls criminal responsibility, and an unsoundness which leaves it unmitigated. How are these distinguished? The law directs the application of the following test:—Was the individual's disease of the mind such as to make him incapable of distinguishing right from wrong at the time he committed the offence? if so, he has a criminal capacity; and if otherwise, he is not guilty on the ground of insanity. The phrase "right and wrong," may be read lawful and unlawful, legal and illegal, and the person be supposed irresponsible, either on the ground of perverted moral sense, thinking wrong right, or of obscured intellect, being incapable of remembering and reflecting, and having no right consciousness of the relations of things. How is this knowledge of the criminal's mind to be got? It is sought for by an investigation of the history of the offender previous and subsequent to the act, in order to discover the character of his immediately antecedent or subsequent conduct. The fore or after existence of diseased mind, and the value of the indications of it, are referred to medical opinion.

The deduction as to the consciousness or unconsciousness of right and wrong in regard to the act itself, is left to the jury.

Now let us consider this test, and the way it is managed. The test is sufficient in cases of amentia, mania, and dementia, the patients in such forms of insanity manifesting in their general conduct evidences of non-development or disease, and clear incapacity for competent ethical or metaphysical distinctions, but is I believe incapable of application or insufficient in cases of partial insanity, the sole cases in which difficulties occur, and special tests are desirable. On the one hand, we may suppose an individual unconscious of right and wrong, and yet not the subject of disease, the voice of conscience being silenced by vice, or perverted in its sound by false training, false reasoning, false principles; on the other hand, and here we do not presume, but assert, a person may be conscious of right and wrong, and yet not be a proper subject of punishment. Before a person can be deemed responsible for his actions, he must not merely have the power of *distinguishing* right from wrong, but the power of *choosing* right from wrong; a criminal being properly punishable, not because he knew good from evil, but because he voluntarily did the evil, having the power to choose the good. If a special test of insanity were to be insisted upon, the power of self-control, as being the true index of responsibility, would seem to be better than that of the integrity either of consciousness or conscience. Had the lunatic, at the time of committing the deed, a knowledge that it was a criminal one, and such a control over his actions as might, if exerted, have hindered him from committing it? Most lunatics have an abstract knowledge that right is right, and wrong, wrong;—as much of it as should keep them from being guilty of unlawful deeds, were but such knowledge sufficient for that end,—but the voluntary power over actions and thought is in every case impaired. I do not say that free agency is annihilated; this were untrue; there could then be no moral treatment of insanity; but it is much limited, and overruled by various insane motives. Lunatics have that amount of freedom of will, which it is philosophical, charitable, and advantageous to recognise for their benefit, but at the same time such a defect of free agency as makes the full burden of responsibility to imperfect human legislation, a discreditable and unjust oppression. Many of them may be fully accountable *ex foro conscientie*, but in all other courts, if the insanity be apparent, the defect of self-control should be presumed to exist, and the individual condemned to restraint, or, if considered liable to punishment, subjected

to only a mitigated penalty. There are many states of insanity in which the will is not completely fettered. When the disease is commencing, the will may often be observed struggling before it is overcome; indeed, a highly energetic will may prevent insanity, it may uproot or control a delusion. When the disease is retiring, the will may also be observed gradually re-asserting its authority over the morbid fancies. Here is an example. A recovered patient, to whose narrative I have more than once referred, was directed in his illness by voices which he heard misleading him. They told him that he was in heaven, and he believed them; that such and such an individual was the Son of God, and the person became to his belief the Divinity; but by and by we find that the voices talked to him in a very singular strain; they said, "You are in a lunatic asylum, *if you will*; if not you are in such and such a place."—"That person is Samuel Hobbs, your attendant, *if you will*; if not he is the Saviour." In short, he was beginning to recover from his delusions, or rather from the manifestations of them; his madness was becoming a matter of his own choice; his will was becoming free, and the state of responsibility returning.

In order to uphold the inference of the unconsciousness of right and wrong, when the plea of insanity is urged, evidence is sought of the existence of a sufficient degree of delirium, delusion, or imbecility of mind, either antecedent or subsequent to the action. No direct conclusion on the point is allowable from the character of the act itself, and on this account it may happen that the best evidence of insanity is rejected: indeed, cases of undoubted insanity from time to time occur, in which there is no sufficient evidence of the morbid condition, apart from the circumstances of the criminal act for which the person is tried; and it sometimes will happen also that these are so extraordinary in their kind, as to compel a verdict of insanity, despite legal orthodoxy. The important point of hereditary predisposition has been objected to as part of the evidence in defence, but I conceive that the danger to be dreaded from allowing such a circumstance its due force, is quite counterbalanced by the chance of injustice which may result from its rejection. Delusion is no adequate plea in any case where it cannot be shown to have impelled the criminal to his crime: in this rule, if absolute, there is manifested the little knowledge that may lead to much error. An insane act is the index in most cases of a delusion, yet the motive may not be apparent though the result is, and the delusion which seems merely to be co-existent with—not related to the act—may yet be

impulsive; for the connection between an act and a delusion is often of a most insane kind, not to be surmised, nor understood when known, but existing nevertheless. Even when the crime is shown to be a morbid result, it may not be a legally sufficient plea for exculpation; for, on the "right and wrong" principle, it matters not that the act flowed from a delusion, if the person was at the time aware of its illegality and its consequences.

The medical witness is not much to be envied when summoned for the defence in cases where the plea of insanity is raised, for though his opinion on the existence of insanity be worth that of jury and judge together, it may be suppressed by the bench, in some manner not very complimentary, and ignorantly assailed in the criticisms of the press. I declare it to be impossible for any one to tell you with certainty what is allowable medical evidence in such cases. Do such and such facts in the evidence, or such and such facts in the case, indicate insanity? Was the prisoner insane at the time that the act was committed? Was he labouring under delirium or delusion? Could the crime have flowed from his insane notions? These are all questions which the judge would probably object to, or find fault with being answered if they were put, as they have been, by counsel. The ordinary objection is, that to question the medical man so, is to make a jurymen of the witness; this does not seem a sufficient reason, for the opinion conveyed in the answer, though a scientific opinion, is not a verdict or declaration, unless the jury see fit to adopt it. Scarcely an instance occurs of acquittal on the ground of insanity, in a case of much public interest, that the medical evidence is not impugned by smart editors, in astonishing leaders on the incompetency of medical knowledge in such matters, and the punishable capacity of the insane. The proceedings and decisions in our courts in cases of diseased mind are discreditable to the judiciary of the age; the dogmas of the law stand in much need of being reconsidered, and the whole course of procedure amended. It is by the influence of medical, and not of legal expositions, that the desired reforms are likely to be brought about. "The same courage," says Dr. Conolly, "which causes the physician to brave the dangers of pestilence, should support him in this duty, beneath the assault of pestilent tongues and pens. Not the voice of the people calling for executions; not the severities of the bench, throwing down pathological truth, should shake his purpose as an inquirer and a witness. His business is to declare the truth. Society must deal with the truth as it pleases."

The jury determine on the guilt of the

offender, which they are understood to do by solving the question of consciousness of right and wrong. To do this, they are not of necessity permitted the light of a scientific opinion on the point. Without adequate knowledge of the phenomena of diseased mind, and unguided by those who are familiar therewith, it is not to be wondered at that great mistakes are made, that verdicts are inconsistent with the evidence brought forward, and that parallel cases are decided in opposite ways. It would be better that the ordinary assize determined on the mere fact of the commission of the crime by the accused,—on the guilt of the person apart from the insanity,—and that the responsibility of the offender should be a matter for the after consideration of a special jury. When a presumed lunatic commits an offence, let him be tried first on the charge, and be entitled, on the certificate of his mental state by competent examiners, to appeal against sentence, on the ground of insanity; then let his mental condition be testified to, and proved before a jury, specially qualified to understand and appreciate the evidence. Such a jury might, as suggested by a writer in the *Journal of Psychological Medicine*, consist in England, of the Lunacy Commissioners, and of medical men experienced in mental disorders. The matter to be investigated should be the diseased or defective condition of the offender, and whether it be such as to limit or annul his responsibility. The evidence should be obtainable from the history previous to the crime, the circumstances of the deed itself, the history subsequent to the crime, and the personal examination of the prisoner.

A lunatic who is found guilty of a capital charge, ought to be, and generally is, confined for life in a lunatic asylum. When the crime is of less magnitude, the confinement should be until recovery, and for a certain length of time beyond it, in order to prove the stability of the cure; before enlargement the patient should be certified to be sane in mind by competent examiners, and the evidence of recovery tested by a special jury. Some have advocated the punishment of all partial lunatics who commit offences against the law, on the principle, not merely of example to all, but also on that of deterring more particularly those who are disordered in mind from giving way to criminal impulses. For the former object we have unfortunately opportunities sufficient, without dealing by disease as if it were a crime, and the latter is not capable of being affected by such irrational means. Oh! but punishment is found to have its advantage in hospitals, as part of what is called the moral treatment of the insane. It may be so, perhaps, but

then it is punishment of a beneficial kind, applied to the lunatic himself, and not inflicted on his fellows for his warning and improvement. No lunatic will be edified and improved by the punishment of an insane offender, for he will not consider himself as in the class for whom the experiment is specially intended. If capable of being deterred, the example of the penalty endured by those who are truly criminal will amply suffice for the purpose. An insane criminal action is committed either in delirium—the patient not knowing what he is about; in delusion—the offender believing the act to be not criminal and avoidable, but necessary and right; or in obedience to diseased impulse, when to choose and to do otherwise is impossible. Under peculiar circumstances, a lesser than the ordinary penalty might be inflicted on the partially insane, just as we see occasionally ordered by magistrates for children under the age of full responsibility; but in general, I believe the punishment of lunatics to be useless for any good purpose,—a mistake and a folly, if not even a sin.

To conclude. As a general rule a lunatic is properly punishable, because disease has restricted or destroyed his capacity as a free agent. Every lunatic should be under control, to prevent him from illegal actions. No one labouring under amnesia, or mania, is a fit object of punishment. A monomaniac ought not to be punished, when it is found that the crime has flowed out of the governing delusion; when the connection betwixt the two cannot be traced, if punishment is awarded, the penalty should be a mitigated one. The morally insane should not be punishable to the extreme. No one labouring under secondary dementia ought to be accounted punishable; and in no case of imbecility, in any degree, should punishment be to the extreme.

Does Insanity diminish the chances of life?—Lunatics are more liable than others to the whole class of nervous disorders, and especially to epilepsy, apoplexy, and paralysis. The peculiar condition of a lunatic's nervous system renders other disorders, with which he may become casually affected, more liable to have a fatal termination. I must tell you, however, that some have held an opposite opinion. It has been asserted that insanity does not reduce the chance of life below the averages. Independent of its complications, there are many grounds for considering that insanity is a condition unfavourable to longevity; instead of enlarging on these, however, I shall rather lay a proof of the proposition before you. The average age at death, in the York Retreat, was under 43 years; the average age at which the

disorder was manifested, was 38. Now, says Mr. Thurnam, the able statistician of that hospital, the expectation of life at 38 years of age, according to the most recent researches, is not less than 28 years, so that the mean age attained should have been 66, instead of 48. The average age at which lunacy appears is 38, at which age the expectation of life is 28 years more; but the average age of lunatics at death is 48,—only 10 years more—instead of 66; hence, insanity shortens life. In 126 fatal cases of insanity in the Aberdeen Asylum, the average is, I find, 46 years nearly. The statistical tables of the York Retreat lead to another conclusion, less expected, which also shows the unfavourable influence of the disease on the chances of life. It is this: the duration of life is not of much higher value even in those who recover, for the average age at death in those who had been discharged recovered was 57, instead of 66, or nine years less than the average probability. The subject is of much importance in connection with life-assurance.

TESTIMONIAL TO DR. CANHAM, OF RAMSGATE.

At a public meeting recently held at the Town Hall, Ramsgate, it was proposed by Mr. Whitehead, seconded by the Rev. G. W. Sicklemore, and unanimously resolved:

"That this meeting having learned, with much regret, the intended departure of Dr. Canham from Ramsgate, are anxious to mark the grateful sense they entertain of his public services, and their sincere esteem for his private worth."

Proposed by Mr. Sackett, seconded by Captain Hitchens, R.N., and resolved unanimously:—

"That a piece of plate be presented to Dr. Canham, as a testimonial, with an inscription embodying the sentiments expressed at this meeting."

Proposed by the Hon. H. S. Law, seconded by Lieut. Hutchinson, R.N., and resolved unanimously:—

"That a subscription be entered into for this purpose; and that a committee be formed to carry out the intentions of this meeting."

* * * We perceive, by the list of subscriptions, that the sum of £2183. 9s. has been already contributed. This is a satisfactory proof of the high estimation in which Dr. Canham's professional services and private worth are held by the inhabitants of Ramsgate.

Original Communications.

CASES AND OBSERVATIONS IN SURGICAL PRACTICE.

By DICKINSON W. CROMPTON, F.R.C.S.

HYDATIDS IN BONE.—UNUNITED FRACTURES.

I HAVE the authority of Dupuytren for stating "that the presence of hydatids in the substance of the humerus is a very unusual circumstance: indeed, the cases cited by authors of hydatids in the bones generally are very few, and their diagnosis extremely difficult, not to say impossible." In Le Gros Clark's edition of Dupuytren's Clinical Lectures, one case is recorded of such disease which ended in death (p. 34), to which I beg to refer, as there is a striking similarity in the course of the case to the one I wish to report, except that the Frenchman died under the exhaustion of profuse suppuration, and gave an opportunity for a post-mortem examination; whilst my patient, I am happy to say, after much difficulty, is now in health, with a very useful, if not perfect, arm: I believe by this time she is completely recovered. There are few cases of hydatids in bone recorded, though Professor Miller (p. 489 of his 2d edition of Principles of Surgery) says—"Hydatids have not unfrequently been found in the cancellous tissue of bone." On referring to the cases, I find them alluded to by different authors; but the references are, for the most part, to the same cases. Mr. Stanley refers to a few cases only: and the one seen by him in St. Bartholomew's Hospital proved fatal, after severe constitutional disturbance from suppuration. A case recorded by Mr. Keate (Med.-Chir. Trans. vol. v.), where the disease was in the bones of the cranium, ended in restoration to health. Two others, in which the disease occupied the tibia (one recorded by M. Corvisart, Journ. d. M.-d. Chir., treated by M. Cullerier; and the other by Mr. Wickham, in the Lond. Med. and Phys. Journ. vol. lvii.), are also to be placed amongst the successful cases; but a third, recorded by Mr. Webster in the New Eng. Journ. of Med., vol. viii. old series, and vol. iii. new series, p. 29, 1819 (which, by the way, was a case that occurred in Guy's Hospital, 1815),

proved fatal, even after amputation had been had recourse to, on account of the great constitutional disturbance that suppuration of the tumor produced. Dupuytren refers to the case of hydatids in the tibia treated by M. Cullerier, recorded by M. Corvisart, before mentioned. He himself once saw hydatids in the body of a vertebra, and once also in the lower jaw.

The disease, at all events, when met with, is one attended, at some period of its course, with considerable danger to the limb, if not to the life of the patient, as the cases above referred to are sufficient to prove.

CASE.—*Ununited Fracture of the Humerus—Hydatid Cyst—Seton—Cure.*

Mary Ladbury, set. 27, a dressmaker, was admitted into the General Hospital, Birmingham, June 4th, 1849. She stated that four months ago she fell down stairs and broke her right arm. She was attended by a surgeon at Redditch, who placed her arm in splints, and, after a time, tried various methods to produce union of the bone, without success. She considers her general health to have been good previous to the accident. The fall was one of great violence. The arm was fractured about the middle, just below the insertion of the deltoid. The point of the fracture was extremely moveable, so that I could easily bend it to a right angle. On the next day the ends of the bone were rubbed together, and the arm secured in splints in the usual manner, only that greater pressure was used with plaster and bandage than would have been justifiable in a recent fracture. The arm at this period was rather thinner than usual, which I attributed to the want of use, and the long employment of splints. There was room between the ends of the bone to press the fingers upon the ligamentous structure that united them.

She was ordered meat daily, and beer.

July 2d.—On examination, no benefit was perceived. The ends of the bone were again well moved upon each other till some pain and heat followed, and a starched bandage was applied from the wrist upwards to the shoulder.

28th.—The bandages were again removed, and no improvement was observed. I therefore, on the 29th, passed a large needle (formed at the end like a spear, and armed with a thick skein of

ligature thread) through the arm between the bones: much force was required to push so large a needle through. I held the integuments and the vessels and nerves tightly inwards, and punctured to the bone with a scalpel, so as to inflict as little pain as possible; and, as the needle emerged through the ligament, and could be felt on the outside of the arm. I cut down upon the point and released it. The arm was supported in a sling, and ordinary splints loosely applied to the humerus.

August 3d.—Considerable inflammation had taken place; the arm was much swollen. *The seton was removed*, and pus of a healthy character discharged. A poultice was applied.

24th.—The inflammation had nearly subsided, but no attempt at union had been made. A starched bandage was again applied from the wrist upwards, leaving openings for the discharge of the matter.

September 6th.—On squeezing the arm, which was swelled and painful, pus was discharged, and at least a teaspoonful of gelatinous matter was passed out of the anterior wound.

10th.—On again squeezing the arm, a few white globular masses were discharged; they were opaque externally, but, on puncturing, were found to contract and spit out a limpid fluid. These I believed to be hydatids that had perished from the presence of pus, which appears to be a fatal poison to them. Some few were whole, but many more were broken, and evidently had been larger cysts than those which escaped unbroken. The suppuration was very abundant and offensive. Gutta percha splints and bandages were now used to prevent motion as much as possible, and to support the arm, which was otherwise very painful to the patient.

26th.—A thickening was evidently taking place, as if from the effusion of lymph under the integument of the whole arm; and mobility of the separated ends was consequently diminished. The wound was looked at and dressed every other day. The matter was allowed to discharge itself into cotton wool, which I always apply under gutta percha splints when a bandage is not used beneath them, as gutta percha is apt to irritate the skin of most patients. Hydatids continued to be discharged on pressure at each dressing, to the number, perhaps, of from six to a dozen.

some skins appeared to have belonged to individuals as large as a small nut, others as large as shot of different sizes.

October 4th.—New bony matter appeared to be forming, particularly from about an inch above the elbow, and passed upwards in a thin crust, so that pressure of the fingers produced a crackling sensation. Hydatids continued to be discharged: and, after careful search, we were able to detect the peculiar hook-like processes, said to be diagnostic of the presence of true hydatids. The microscope had been frequently had recourse to, for the purpose of ascertaining the true nature of the opaque bodies that had escaped from the wounds, by Dr. Heslop, Dr. Bell Fletcher, and myself, and also, on one occasion, by Dr. Gairdner, who was passing through Birmingham: each, with true philosophical caution, hesitated to give a name to these bodies till the presence of the hook-like processes was evident.

10th.—The discharge varies much in quantity; but the arm, even above the shoulder, and above the deltoid muscle, is now much swelled, and feels doughy, as if great suppuration was about to take place. The girl looks pale, is emaciating, and her health is evidently much depressed by the present state of the discharge.

21st.—The arm, up to and under the deltoid, as well as over the acromion, part of the scapula, and under the humeral end of the clavicle, is considerably swollen, red, and in part softened, so that fluctuation is perceptible; on steady pressure over these parts downwards towards the wounds, about six ounces of pus, with a quantity of hydatids, were pressed out. An abscess was opened in the axilla on the 24th.

29th.—The discharge is so great as to require the arm to be dressed every day: two smaller collections of pus have been evacuated, one under the acromion, the other at the back part of the arm; yet the arm is becoming firmer, and the number of hydatids discharged is much diminished. The arm, elbow, and shoulder-joint, are kept as immovable as possible by gutta percha splints and bandages.

November 15th.—The improvement in the arm is steady, but the discharge is still so great that the patient's health suffers very much. I have, therefore, recommended her to go into the country.

28th.—Abscesses still continue to come to the surface, which I open. There is one place in the posterior part of the arm from which I can generally press out matter, and a broken hydatid skin or two through the wound in the anterior surface of the arm.

December.—This patient is now returned home with the arm quite firm, but at present, from the consolidation of the integuments and muscles caused by such extensive suppuration, she has not much power of motion, or of use in the extremity. She still looks pale, but is improving.

June 27th, 1850.—I have seen Mary Ledbury to-day. The use of the arm is much improved; she can sew a little, but cannot quite straighten the elbow, or raise her arm to the horizontal position from the shoulder. There is still some little purulent discharge, and minute exfoliations have taken place.

Hospital practice gives the surgeon frequent opportunities of seeing cases of delayed union. Within the last seven years I have seen three cases of ununited fracture of the humerus, in all of which the seton was tried—in two successfully, after extensive suppuration. In the third I could not pass the needle between the ends of the bone, which was broken obliquely at about the insertion of the deltoid. The man was a very powerful person, and was stronger in that arm than I was with either or both of mine; so I advised him to be content, and let the arm remain as it was.

Three cases of ununited fracture of the tibia have been under my care, and all much benefitted by the use of the seton, pressure, and rest combined. I believe they are all well now, but I have not seen any of them for the last few years. A fourth case was fracture of the tibia, with other injuries, under the care of Mr. Wood: the man had severe pleurisy, during which the fractured extremities became loose. The seton was introduced between the ends, and sufficient action renewed to produce perfect union. I have also seen two cases of ununited fracture of the thigh, one close above the knee-joint. The man could manage to walk pretty well by the aid of a strong leather case. On that account, therefore, as well as because of the close proximity of the fracture to the joint, I recommended nothing more to be done.

These cases led me to endeavour to collect the recorded cases of ununited fractures, with a view to ascertain the bones, and the particular situation in each bone, in which this unfortunate termination had been observed. I had collected 86 cases when M. Malgaigne's extensive work on Fractures and Dislocations came under my notice. Under the head of "Ununited Fractures" I found references to a paper, by Mr. Norris, in the American Journal of Medical Science, vol. xxix. Jan. 1842, Through the kindness of my friend Dr. Edmund Parkes, I obtained the volume, and was surprised to find that so excellent and extensive a statistical work on such a subject had been (as far as I am aware) unnoticed by any writer on this side the water, except M. Malgaigne, who certainly has made full use of it. I cannot but think it would be well worth the attention of some of our journals, and be very desirable (with the consent of the writer) to republish both the paper on Ununited Fractures and the equally valuable and laborious one on Aneurism, for which the profession is so deeply indebted to that gentleman. In the meantime, I append an abstract of the *résumé* with which Mr. Norris concludes his paper on Ununited fractures:—

Of 150 cases of ununited fractures—

48	occurred in the femur.
33	" " leg.
48	" " humerus.
19	" " forearm.
2	" " jaw.

Of 112 cases, in which the age is noted, there were:—

Between 10 years and 20	. . 14
" 20 " 30	. . 53
" 30 " 40	. . 21
Above 40 24

M. Malgaigne has with care made out a list of 104 cases, with the precise date of the fracture as to the age of the patient suffering from the accident, with a view to the theory of the greater difficulty of union in old people. He finds

1	under 5 years.
2	from 5 to 10
3	" 10 to 15
4	" 15 to 20
50	" 20 to 30
19	" 30 to 40
14	" 40 to 50
6	" 50 to 60
3	" 60 to 70
2	above 70

Mr. Norris's summary of treatment is as follows:—Of 46 cases treated by seton, 36 were cured; 3 partially so; 5 received no benefit; 2 died.

Of 38, in which resection, scraping, &c., were used, 24 were cured; 1 partially so; 7 received no benefit; 6 died.

Of 36, in which pressure and rest were used, 29 were cured; 1 partially so; 6 received no benefit.

Of 8, in which caustic was used, 6 were cured; 2 received no benefit.

Of 11, in which frictions were used, 11 were cured; but, in 36 cases cured by seton, friction had failed in 8; and,

in 24 cases cured by resection, friction had failed in 5.

Of 11 cases in which other means were used, 5 were cured by iodine; injection, 1; erysipelas (!) cured 1; and hot iron, 1.

Amputation (!), 5 cases. One was cured; 2 died; 1 failed; 1 not stated.

The table of results from the five methods of treatment most commonly used—namely, 1st. Compression and rest; 2. Friction; 3. Seton; 4. Application of caustic to the seat of fracture; 5. Resection of the ends of the bones—

Shows, that out of 36 cases included in his table treated by compression and rest, 29 were cured Of these—

13 were in the femur, of which 9 were cured.				
7	"	leg	"	7
12	"	humerus	"	9
5	"	forearm	"	4

From the seton and its modifications, in 46 cases

13 were in the femur, of which 9 were cured				
10	"	leg	"	10
16	"	humerus	"	10
6	"	fore-arm	"	6
1	"	jaw	"	1

Accidents following the use of the seton in the 46 cases occurred 12 times—namely, arterial hæmorrhage twice; fever, erysipelas, or profuse suppuration, 10 times.

From the use of resection in 38 cases, 24 were cured; 1 amended; 7 failed; and 6 died. Of these,

12 were in the femur, of which 7 were cured. Rather more than half!				
6	"	leg	"	5
12	"	humerus	"	6
7	"	forearm	"	5
1	"	jaw	"	1

and one amended

In 17 of the cases in which resection was employed, other methods had failed; the seton was used six times.

In the 38 cases in which resection was used, accidents *not* ending in death, but dependent upon the operation, followed 9 times—namely, erysipelas 6; profuse suppuration and abscesses 2; phlegmasia dolens 1.

Mr. Norris concludes that—

1st. Non-union after fracture is most common in the thigh and arm.

2nd. That the mortality after operations for cure follows the same laws as after amputations, and other great operations upon the extremities—viz. that the danger increases with the size of the limb operated upon, and the nearness of the operation to the trunk; the mor-

tality in them being greater in the thigh and humerus than in the leg or forearm.

3d. That the failures after operations for their relief are most frequent in the humerus.

4th. That after operations for the cure of ununited fracture, failures are *not* more frequent in middle-aged and elderly people than in young subjects.

5th. That the seton and its modifications are more safe, more speedy, and more successful, than resection or caustic.

6th. That incising the soft parts previous to passing the seton augments the danger of the method, though failures are fewer after it

7th. That the cure by seton is *not* more certain by allowing it to remain

for a very long period, whilst it exposes to accident.

8th. That it is least successful on the femur and humerus.

Such are some of the important results for which we are indebted to the great industry of Mr. Norris: five times more than is contained in many thick octavos—not to mention the statistical paper on Aneurism which has been inserted in a more recent number in the same unassuming manner. He must be a bold man who, after reading his paper and the case in the "Lancet" of October 19, 1880, would again first break by the force of pullies, then reset, then resect, and then amputate, for a firm though badly united femur.

M. Malgaigne has given a statistical table of the relative frequency of fracture in all the bones of the body; but he has not particularised the exact situation of fracture in each bone. Mr. Adams has given similar tables in some volumes of the MEDICAL GAZETTE.*

Until we have tables that particularise the exact situation of the fracture, we cannot compare the relative frequency of ununited fractures in the various bones with fractures that unite in a normal manner. Such tables could easily be made out by an intelligent house-surgeon of any of our hospitals, who necessarily sees the cases of fracture at the earliest period, and before swelling has occurred, or a right position been given to the bones. I think it very probable that some law would be found to exist that might explain the reason of its occurrence. Possibly it might be found that fracture near to a joint, or to the insertion of some powerful muscle, would have much influence in producing such an accident, on account of the increased difficulty, in such situations, of procuring the required immobility. It is certain that peculiarity of constitution will not account for the production of ununited fracture in a great proportion of the cases in which it has occurred.

Birmingham, Oct. 26, 1880.

* Malgaigne's table of the total number of fractures in 2222 patients is—

Of trunk	379
Superior limbs	925
Inferior limbs	1694

FURTHER REMARKS ON THE EMPLOYMENT OF CHLOROFORM BY THIEVES.

By JOHN SNOW, M.D.

In a former communication on this subject* I said, that if "thieves and prostitutes were to resort to the use of chloroform in the public streets, in the manner alleged, the attempt would only lead to their instant detection on the spot." This opinion has been more than confirmed by what has since occurred. In two or three cases in which it was alleged that robberies had been effected by means of chloroform, that part of the accusation which related to the use of this agent broke down on the cross-examination; whilst in two instances—the only ones, as I believe—in which it has been proved that chloroform was used with felonious intent, the culprit failed to induce insensibility, and was detected on the spot, although the attempt was not made in the street, but in a more secluded place. The first of these cases occurred in London, a few months ago. A young man returning after midnight with his sweetheart from a dance at a public-house, induced her to accompany him down a Mews or stable-yard. He there took out a bottle containing chloroform, and poured some of it on a handkerchief, which he applied to the young woman's face. She tore away the handkerchief, and called out in such a manner as to bring a policeman from a neighbouring street to her assistance, who secured the offender, and picked up the bottle, which the latter had thrown away in his flight. The reader will probably remember that this case was amicably compromised, by the prisoner marrying the complainant, whilst he was remanded, and out on bail.

The other case, which happened recently, and is detailed in the MEDICAL GAZETTE of yesterday, is of a more atrocious kind. A man who was lodging at an hotel in Kendal, secreted himself in the room of an elderly gentleman, whom he attacked in the middle of the night with a rag steeped in chloroform. The means used, so far from keeping the gentleman asleep,

* MED. GAZ. last volume, p. 237.

had a contrary effect; and although the robber struggled with his victim till blood was drawn by his violence, and the bedding had fallen on the floor in the scuffle, he did not succeed in making him insensible, or in preventing his bringing assistance by his cries. It is quite true that the attempt, had insensibility been induced, might have resulted in murder; for the thief in the dark, and without experience to guide him, could not have known when to stop in time to spare life, if that were his intention, I therefore coincide in the remark in the leading article of the *MEDICAL GAZETTE* of yesterday, respecting the insufficiency of eighteen months' imprisonment as a punishment for such a crime.

There may, however, have been circumstances which would have some weight with the Bench, and have not appeared in the newspapers.* I can easily suppose that it was the man's first attempt at robbery; for I cannot imagine that an experienced thief, or one who had the advantage of belonging to a gang, and therefore of consulting about, and trying beforehand, the means to be used, would, on hearsay or newspaper evidence, have adopted the use of an agent so ill suited to assist him. Again, a chief object of punishment is to protect the public by preventing future crime; but in this instance the signal failure of the attempt at robbery will do more to deter others from using a like means than any example that could be made of the criminal. I think that this kind of attempt is not very likely to be repeated; and although chloroform is one of the thousand articles the use of which should be confined to medical men, yet the subject, in my opinion, is hardly of sufficient importance to require legislative interference. The murders by arsenic, in the rural districts, are a disgrace to the age and country in which we live, and have long and loudly called for a legal enactment to limit the sale of this poison: but if Parliament have to discuss what articles have to be admitted into the provisions of a "Sale of Poisons Restriction Bill," I am afraid that we shall be long in obtaining such an enactment; and if, when at last obtained, it be complicated by including too many substances, it will not work well.

The public have been greatly alarmed about the employment of chloroform by thieves, but what they really have to dread is, that robbers will still resort to the old means of the bludgeon, the pistol, and the knife, and not to one which allows the victim so good an opportunity of escape, and themselves so great a chance of detection. Every person who has inhaled chloroform must be quite aware that it could never have been given to him, in his sober senses, without his knowledge, even though every care were used to lessen the impression it makes, by beginning with the vapour largely diluted, and gradually increasing its strength; and no domestic animal, however tame, can be induced to take chloroform voluntarily, but before being made insensible, or in the least affected by it, must always be in some way secured. Therefore, to use chloroform for the purpose of overcoming a person is to adopt a means that cannot be put in force till he is already overcome, and involves a difficulty just like that contained in the juvenile problem of catching a bird by applying a little salt to its tail.

*** Chloroform has been used in France for the perpetration of rape, and the offender tried and convicted. We hope Dr. Snow's prediction, that it cannot be used for robbery without the certainty of failure and detection, may not be proved wrong by experience.

ON CONDUCTORS OF SOUND.

BY JOSEPH SWAN.

IN mammalia the bones of the skull, the external walls of the tympanum and the ossicula, are not so hard as the petrous portion in which the labyrinth of the ear is placed. Sounds cannot extend from the ear except in a slight degree beyond the anterior point of the petrous portion; they are therefore expended on the cells of the mastoid process posteriorly, and on the concha. When the bones of the skeleton are light, as in small animals, the same thickness of the petrous portion does not exist, but the hearing is not then deteriorated, as the parietes of the labyrinth, from their hardness, are still capable of receiving and modifying sounds to a nearly

similar extent. The reception of sounds may then be clear, but there is a deficiency in the sense of solidity and power, so that very loud sounds are tremendous and overwhelming. The petrous portion, by giving a sense of solidity, allows a superior mode of discrimination, and fits the animal for acting steadily, especially in combat amidst conflicting sounds. In birds the freedom of communication between the interior of the bones and the air causes slighter impulses necessary, and, therefore, very slender ossicula. The semicircular canals and cochlea are seen to a great extent from the cavity of the skull, as in the smaller mammalia, and loud sounds therefore terrify them in the same manner. In both mammalia and birds, the bones, from their hardness, would readily conduct sound, but the joints and soft parts, and especially the integuments and their coverings, are unfavourable. When the body is recumbent on hard substances, much of the sound received from them is mechanically conveyed through the bones of the head to the ear.

In amphibia the bones of the skeleton are very hard, and in some instances the skin from its hardness resembles bone; both are therefore capable of conveying strong impulses from the ground.

In cartilaginous fishes the bones are not so hard, but there is a power of continuing similar sounds from the ground, as in amphibia. Osseous fishes probably do not receive such strong impulses from the water alone, as the cartilaginous do from it and the contiguous ground, and therefore are provided with hard stones, which are better conductors of sound. Amphibia and cartilaginous fishes have soft cretaceous matter in the sac of the labyrinth, for lessening and modifying the impulses received from the ground, which would otherwise render nugatory the influence of such sounds as are usually determined by the labyrinth.

In many instances of mammalia and birds in which the petrous portion of the temporal bone does not surmount the semicircular canals, a space is left which is occupied by a lobule of the cerebellum, but this has no communication with the labyrinth. It is probable that the molecular form of the grey

matter prevents impulses from reaching the brain, in the same manner as the molecular form of other matters impedes the progress of sounds to the ear, and, through this, to the nerves and white matter of the brain.

From many experiments it appeared that ashes from a coal fire, coal dust, sand, wheat flour, and powdered chalk, were bad conductors of sound. When each of them—except wheat flour, which was not tried—was mixed with water, the conducting power was somewhat increased. Sound passed readily through oats, also through wheat bran. A piece of chalk, about seven inches square, had a cavity about three inches deep made in it, for holding a watch placed flat; powdered chalk was then put over the watch, so as to fill the hole, and there was a very faint sound heard; but when the powdered chalk was pressed down, the sound was much louder; the sound was also increased when powdered chalk made into a mass like mortar surrounded the watch tied up in oiled silk. The same was done in the other experiments with wet matters, and with dry the watch was enclosed in paper; in some the watch was surrounded by the matters in a porcelain vessel—in the others, in one of glass.

It seems that the more the several substances preserve their molecular form, the worse conductors they are; and, when the molecules are aggregated, there is a difference of this power according to their degree of cohesion; so that chalk appeared a low conductor, then sandstone; but marble, glass, and fine earthenware were good conductors.

The preceding experiments were made for ascertaining the difference of power in conveying sounds between the varied of matter; and therefore, such forms as were at hand were tried; but, as it afterwards appeared that several of them were frequently used in the construction of walls of rooms, they suggested that the errors in providing for sufficient means of hearing proceeded as much from the materials employed as from the shapes of the rooms; so that, when very bad conductors of sound are used, the vibrations are not continuous throughout, but broken off at the walls, and the tension is not then sufficiently preserved for extending the voice much beyond the speaker, without great and almost uninterrupted exertion.

MEDICAL GAZETTE.

FRIDAY, NOVEMBER 18, 1850.

It appears to us that gentlemen who makes use of newspapers for the publication of a document in reference to scientific questions affecting public health, should be especially careful not to conceal or misstate the broad facts of the question at issue. We regret to perceive that a letter by Mr. R. Warington, dated "Apothecaries Hall, September 26th," in which the Government reports on the use of lead salts in the manufacture of sugar are criticised and condemned, is written in open violation of this rule. This document, which is rather remarkable for the *impresario* *vari*, has found a wide circulation through the *Times*, *Morning Herald*, and other newspapers. Of course it is to be presumed that the letter has been published by the knowledge and with the approval of the writer. Seven weeks have elapsed, and there has been no retraction or explanation of statements injuriously reflecting upon the researches and reports of the Chemical and Medical Commissioners appointed by Government, and consisting of gentlemen, not employed by solicitors to advocate only one view of a great sanitary question, but authorised by Government to exercise a free and independent judgment on the whole of the circumstances.

The letter to which we have above referred, is, in our opinion, well calculated to mislead those persons who are unacquainted with the nature of scientific researches, and to induce the public to believe that they may consume the patent sugars, prepared with the poisonous salts of lead, with actually less

risk to health than attends the consumption of sugar manufactured by the ordinary process.

The City article-writer of the *Times*, who appears to be completely deceived by this document, thus prefaces its publication by the following language:

"Having twice interfered in an affair of such importance, the Government were bound to bring it to a definite conclusion, and not to leave the public to find their way out of the alarm that had been raised by an examination and comparison of such casual statements as might chance catch their eye in the public press. But this, unfortunately, is now the position in which the controversy has been placed, and having inserted an analysis of the Parliamentary papers, we therefore give room to the following letter to the editors of the *patentees* from Mr. Robert Warington, the secretary of the Chymical Society of London, and Chymical Operator to the Society of Apothecaries, which throws out light on the entire subject. At the same time it is to be understood that the discussion, except so far as any necessary corrections may be concerned, should, for the future, be carried on through such professional channels as may be most appropriate for it."

We are sorry to have to express our conviction, that the whole question has been most unfairly dealt with in the City-articles of the *Times*. While Mr. Warington's letter and other documents in favour of the use of lead-sugar have been given *in extenso* in that journal, the analysis of the views of the Government Reporters referred to in the above extract, and published in the *Times* of September 12th, comprehends merely twenty-nine lines of extract from the *Chemical*, and sixteen lines of a summary of the *Medical Report*! In fact, even in the article in which these extracts appear, a much larger space is devoted to the views of the lead-sugar advocates, than of those who have reported against it. If, therefore, it rested with the City-article writer of the *Times*

* *Times* newspaper, Friday, October 18th.

and Mr. R. Warington, "the Chymical Operator to the Society of Apothecaries," the consumption of this sugar would be forced upon the British public in spite of their reasonable prejudices against the risk of slow poisoning by lead, and in the teeth of the *independent* Government reports. In order to counteract the one-sided views which have thus found the powerful advocacy of the *Times*, we think it right to analyse a document to which our contemporary has thus given a wide circulation.

MR. WARINGTON first deals with the *Chemical Report*, which was drawn up and signed by Professors THOMSON, GRAHAM, and HOFMANN. Apparently placing implicit confidence in the method of chemical analysis adopted by these gentlemen, the writer says:—

"It appears that the patent process, as directed in the specification of Dr. Scoffern, was followed out three several times by Professors Thomson, Graham, and Hofmann, without any trace of lead being found in the results, such results having been submitted to rigid analysis. The process is therefore quite unobjectionable in every respect on this evidence."

In a subsequent paragraph, however, we find the following statement:—

"The mode of analysis adopted by Professors Thomson, Graham, and Hofmann, for the estimation of the lead, which they state to be very easy and complete, and the sufficiency of which was tested, appears to me, judging from the published results, to be very unsatisfactory. For instance, we find it stated that one and the same quantity of treacle, made by the old process, yielded in one experiment 0·62 (gr.) and in a second 1·21 (gr.), or very nearly double the quantity. Surely a method of analysis that will yield so widely differing results from the same sample, cannot be received as complete or satisfactory."

No doubt the writer will be able to explain this somewhat conflicting opinion regarding the chemical skill of three gentlemen, two of whom are well-known

veterans in practical chemistry, by the fact that in the first case the result of their operations is in favour of his clients, and in the second case against them.

Let our readers, however, mark the disingenuousness of the statement impeaching the accuracy of the Chemical reporters. The chemical results are objected to, because "*one and the same quantity* of treacle yielded in one experiment 0·62, and in a second experiment 1·21 grains, or very nearly double the quantity." These decimals look very mysterious in the *Times* newspaper, and as the *quantity* of treacle which was the subject of experiment is not at the same time stated, the fact that the proportion by one result is double of that obtained by another, conveys the impression that Professors Thomson, Graham, and Hofmann, have made an awful blunder, and that their "mode of analysis, judging from their published results," &c., is "very unsatisfactory." "Surely," remarks Mr. Warington, with particular emphasis, "a method of analysis that will yield *so widely differing results* from the *same sample*, cannot be received as complete or satisfactory."

We must supply Mr. Warington's no doubt unintentional omission, and state for the information of our readers, that the quantity of treacle used in each experiment was *four pounds* (28,000 gr.), and that the awful difference in the quantity of lead found in the two analyses was no more than 0·59 grains—i. e., less than *six-tenths* of a grain, in operating upon this large quantity. Mr. Warington must have been perfectly aware that he was addressing newspaper readers, not well informed in decimals, or he would not have ventured to impeach the accuracy of the chemists upon so absurd a ground as this. Scientific men who might happen to see his letter, would not on

the other hand have it in their power to take any exception to his conclusion, because he had accidentally omitted to state that the quantity of treacle used was four pounds.* Now we must take leave to express a doubt whether the "Chymical operator to the Apothecaries' Society" would under the same circumstances have arrived at results differing less "widely" than these. Further, the treacle here referred to being derived from the old process, contains, according to Mr. Warington, carbonate and oxide of lead, arising from the use of leaden pipes and of moulds painted with white lead. The carbonate of lead is not a soluble salt: it may be, therefore, mechanically diffused through the mass of treacle in very variable proportion. There is, at any rate, no proof that every four pounds of a given specimen of treacle contains an equal quantity; and yet, in defiance of this palpable inference, and of his previous statement regarding the condition of the lead, the writer of the letter proceeds to impeach the chemical accuracy of such men as Thomson, Graham, and Hofmann! Mr. Warington assumes that the results should not have differed, although the difference was exceedingly small, when the bulky nature of four pounds of treacle is considered. In fact, as the lead-salt was in an insoluble and mechanically diffused form, it might have been inferred, *a priori*, that two specimens would not have given precisely similar quantities of lead. This objection, therefore, to the chemical results, is not only unfounded in fact, but calls for the stronger condemnation, because it is directly opposed to circumstances which must have been within the writer's knowledge. It was

* In a preceding paragraph wholly disconnected from that in which this absurd objection is made, it is stated generally that the results of the examinations, as detailed in the report, were obtained from four pounds weight of the sample operated on.

Mr. Warington's duty in such a case to have made analyses of common treacle; and then he would probably have found that in spite of the skill he could display, the results would have differed, perhaps even more than six-tenths of a grain on four pounds! Until he had gone through this special research he was not entitled to take exceptions to the results of others,—exceptions which, had the whole of the facts been properly and candidly stated in his letter, would have been found to tell strongly against the accuracy of his judgment.

To us it appears that the Chemical reporters have been most unfairly dealt with by the patentees. We learn from the report, that they obtained "by the courtesy of Messrs Goodhart, sugar-refiners of Limehouse, who were working under the new patent, *properly authenticated samples of refined sugar of ten different operations*, with one sample of bastards and two samples of treacle, *all produced by the new process*."* These samples were sent to the Chairman of the Board of Inland Revenue, with a letter dated March 30th, 1850, from which the following is an extract:—

"We have to remark that, owing to the defective nature of the filtering apparatus after the *gassing operation*, the liquor was at times turbid (though we have since remedied that defect, and obtain constantly a brilliantly bright liquor); but it is probable that in some of the sugars, as per samples herewith sent, a portion of sulphites may be detected, but we believe no other traces of lead, further than may have been caused by contact with leaden pipes and cisterns, which we, in common with most other sugar-refiners, make use of, as also white lead in painting the iron moulds."

This extract, it will be perceived, involves a very cleverly expressed reservation on the part of the manufacturers. Had the results turned out

* Chemical Report, page 2.

favourably, and no lead been discovered well and good: we should have heard no more of this letter or of the defective nature of the filtering apparatus. As it is, we have no information whether the sugar from which these ten loaves, &c. were taken—the result of defective filtration—was sold to the public, or destroyed as unwholesome after the publication of the Chemical Report! The adverse nature of the Government Reports, however, has caused this letter to be brought into prominent notice. Thus Mr. Warrington condemns these Reports as being based on the examination of products admitted to have been obtained by the use of defective filtering apparatus! He says—

"The products of the process of which the patent forms a part, as obtained from parties who had been working only for a short period, are then experimented on. Now, it must be obvious that the first introduction of a new process, however perfect, and however suitable to the large scale, will at first, to a certain extent, be attended with difficulties in its practical working out. The evidence of Mr. Goodhart's letter proves this to be the case in the present instance. . . .

"No examination appears to have been made of the syrups obtained by the new process before they were brought in contact with the leaden pipes, cisterns, and painted moulds, alluded to in this letter; and all the lead found by Professors Thomson, Graham, and Hofmann, in these samples may really have arisen from this source; and if so, a great injustice may have been done by the Report from want of proper circum-spection.

"In regarding, therefore, the results obtained by Professors Thomson, Graham, and Hofmann, from the samples procured under these circumstances, it seems to me that a much nearer approach to accuracy will be attained by taking the minimum weights as evidence of the working out of Dr. Scofield's process, which process they themselves report as perfect. The results, then, will stand thus:—In loaves, no lead; in bastards, 0.48; in the treacle, 0.87; or, by comparison,—

	Old process.	New process.
Loaves	0.05	none.
Bastards	0.25	0.48
Treacle	1.21	0.87

Mr. Warrington must be perfectly aware that the Government was only desirous of having what the patentees considered fair samples of their sugars submitted to analysis. The manufacturers were allowed to send their specimens; and it appears from their letter that the ten loaves sent as samples were derived from different days' work at various periods, from December 31st to March 13th. Hence the defective filtering apparatus appears to have been worked during a quarter of a year. Of the other samples, one of treacle was marked "to be passed through a bed of charcoal." The analysis was commenced about the latter end of March, and the Report was not presented until the 28th of the following June. The manufacturers and the patentees, without making any remonstrance, or proposing to substitute perfect for what they have since called imperfect specimens, had, therefore, allowed the chemists to proceed with their elaborate researches during the long period of three months; and it was only when they found the results adverse that they fell back upon the letter which accompanied the samples to show that they were not fairly-taken specimens of the new process, and the results were therefore untrustworthy! That manufacturers deeply interested in the pecuniary proceeds of a patent should take this sinuous course is not surprising, but we must express our astonishment that they should have found a gentleman, occupying Mr. R. Warrington's situation at Apothecaries' Hall, to aid and abet them—nay, to justify them in such a proceeding. Let us suppose that Government had referred to him the analysis of certain drugs alleged to be adulterated: he is allowed by the drug-merchant to take samples with the knowledge that he is

to submit them to chemical analysis, and report on them for the information of Government. After having been engaged *three months* in his operations, and having made a report showing no disposition to prejudice the merchant, he is then coolly informed, upon the authority of some "chymical operator" who has not taken the pains to analyse the drugs, that his analysis and report are worth nothing, because the samples had become contaminated by some defective drug-grinding apparatus. It is beyond all question certain that, had the Chemical and Medical reports been favourable to the patentees of the new sugar process, we should have heard nothing of the defective filtering apparatus, &c. The fact that they allowed the specimens to remain three months in the hands of the chemists, either entirely does away with the plea now put forth in their favour by Mr. Warington, or in future there can be no *bona fides* in similar scientific investigations.

Let our readers observe the results of Mr. Warington's ingenuity in asserting that the *minimum* weights of lead contained in the patent sugar may be taken as a much nearer approach to accuracy. Thus, while he represents that the loaf-sugar of the patent process contains *no lead*, the report from which he draws this conclusion states in figures, that out of ten samples *nine* actually contained lead—one sample as much as a grain in four pounds! Thus, according to Mr. Warington's mode of dealing with chemical facts, if you find lead in nine specimens out of ten, it is quite fair to tell the public, through the newspapers, that there is *no lead* present!

With respect to the loaf-sugar of the old process, Mr. Warington's table represents it as containing 0.05 grains of lead in four pounds. This is a complete misrepresentation of the statement of the Chemical reporters, which we now quote from page 2 of the Report:—

"No lead whatever appeared to be present in four lbaves of sugar refined in different operations by the ordinary process: at least, no much as is equivalent to 1-20th of a grain (0.05 grain) of sulphate of lead could not be present in four pounds of any of these samples."

Let our readers compare this statement with a representation of their facts and figures as given in Mr. Warington's table. He describes them as actually finding 05 grains of lead in four pounds of loaf-sugar, when they positively state that they found *none*, and that there could not have been present so much as Mr. Warington assigns as the average. This is what Mr. Warington considers to be "a much nearer approach to accuracy," we should describe it as the reverse. The same sort of free and easy dealing runs through the other series of figures. Thus, while in the extract from his letter Mr. Warington asserts that the *minimum* weights will afford a much nearer approach to accuracy, he gives the benefit of this estimate only to the patentees, upon the principle, probably, that any amount of inaccuracy will answer for the sugars of the old process, and the readers of the *Times* would not probably discover that the *maximum* weights in one column were compared with the *minimum* weights in another.

Thus we find in the Report that the bastards and treacle prepared by the old process gave the following weights of sulphate of lead:—

	Max.	Min.	Mean.
Bastards	0.25	0.19	0.22
Treacle	1.21	0.62	0.91

It will be perceived by reference to Mr. Warington's figures that in treating of the old process he takes for comparison the *maximum* weights of lead salts; and in dealing with the new process he selects the *minimum*. The old process is entitled to fair play; but it certainly does not get it at the hands of Mr. Warington, or of the City-article writer of the *Times*. They are determined that the public

shall swallow the lead-sugar; and while one cooks the results the other serves them up. We are sorry to spoil the cookery; but in a scientific question of this kind we consider it to be our duty to watch over the interests of the public, although the remarks we feel bound to make may damage an ingenious but dangerous application of chemical science.

After this demonstration of Mr. Warrington's mode of dealing with the *facts* and *figures* of a Chemical report, we think we need hardly dwell upon the other parts of his letter. There is either the same want of fair dealing, or there is blundering throughout. Facts are either misstated or mistaken. In reference to the *Medical* Report we find the following remarks:—

"The report of the medical jurists, Drs. Pereira, A. Taylor, and Carpenter, is of no value whatever, inasmuch as it is based on fallacious grounds; for they state, 'Our investigation practically resolves itself into the inquiry, whether 1·261 grains of metallic lead contained in a salt of lead, capable of becoming dissolved by the juices of the alimentary canal, swallowed weekly for probably many months, are capable of producing serious injury to health?'

"Now there is no evidence whatever in the report to show that the lead stated to be contained in the samples examined, was in such a state as that required by these gentlemen, and on which supposition their report is entirely founded; nay, worse,—they adopt the very extreme result obtained from a sample of treacle which was stated in writing by the manufacturer to be objectionable from a defect in the working of the filtering apparatus; and on this assumption they call in question the chemical accuracy of the patent process."

We think it a somewhat droll proceeding on the part of the patentees to apply to a gentleman known only as a pharmaceutical chemist for his opinion on a subject which presupposes a *medical* education, and requires a knowledge

of physiology, pathology, and toxicology. That they could find no better authority to apply to in reference to a question of chronic poisoning by lead than a gentleman who has probably never seen a case, and whose studies are confined to pharmaceutical chemistry, leads to the inference that the opinions of members of the medical profession must be adverse to them. The result may easily be foreseen. In the *Medical* Report, as we read it, the quantity of lead in the patent sugars is calculated as metal to avoid all quibbling about the actual state of the salt of lead in the sugar, which appears to be just as mysterious to Mr. Warrington and the patentees as to the reporters. We know of no salt of lead which is not more or less soluble in the secretions of the alimentary canal; when dissolved it becomes absorbed, and when absorbed it is liable to affect the system injuriously. Mr. Warrington appears to think that the lead is supposed to be swallowed in a *metallic* state in the sugar; but we do not gather this from any part of the Report. It seems to be left as an open question, whether it be in the state of sulphite, sulphate, carbonate, or undecomposed acetate? To affirm that the three former are not in a state to be dissolved by the juices of the alimentary canal, merely because they are not very soluble in *water*, would display great physiological ignorance, and show that the person was not qualified to give any opinion on the subject.

The objection that the treacle which yielded the maximum of lead was the product of a working with defective filtering apparatus, we have already disposed of. It was the duty of the parties not to have sent for analysis, specimens of sugar or treacle which they considered defective, or to have withdrawn them, for which there was ample time. The Reporters, in conscientiously performing the duty en-

trusted to them by Government, of stating the probable effects of the patent sugars on health, were bound to take *extreme results*. There was nothing to show that the maximum weight of lead might not be frequently present in patent treacle sold to the public, who have no means of ascertaining whether this article of food has or has not been "passed through a bed of charcoal." To have reported only on the probable effects of a *minimum*, or even of an average weight of lead, would have been undoubtedly more favourable to the patentees; but it would have been a breach of duty, in a sanitary point of view, since the patentees could give no satisfactory security that the weight of lead in the treacle should not exceed the average and reach the maximum.

Such is the letter upon which Mr. Warington and his coadjutors would recommend the general use of poisoned sugar. The City-article writer of the *Times*, without taking the trouble to compare the letter with the Report, adopts the whole of Mr. Warington's statements, including the distorted facts and erroneous figures, and thinks that an attempt has been improperly made to stifle a valuable discovery. It is his opinion that a case is made out for further inquiry. We trust that the Government will adopt the suggestion, if only to expose the unfair means which have been resorted to in order to support a bad case.

It is incontestably proved that a salt of lead is liable to be left in the sugar as manufactured. It is quite possible, as the Chemical reporters assert, "that the lead may be removed from the sugar to a point beyond danger, provided the operations are *constantly watched by a person competent to apply the necessary chemical test of purity to the products*." The great practical question, as it affects the public, is not the chemical accuracy of the process as it may be conducted in

a chemical laboratory, but, to use the language of Mr. Wood, as it may be carried on "on a large scale, and by *common workmen*, who may not be sufficiently impressed with the necessity of *entirely freeing the products from any trace of deleterious ingredients*." The syrups are strongly impregnated with a poisonous salt of lead; under proper and skilful superintendence the whole of this lead may be removed from the sugar. But is the health of the public to rest upon a contingency like this? When a general colic has spread throughout the land, and has affected all, or even a large number of the consumers of sugar and treacle, it will be no satisfaction to turn to Mr. Warington's figures, and to find that, according to his calculations, they ought not to have suffered, because *one* patent loaf out of *ten* contained no lead, and because the *minimum* found in patent treacle was less than the *maximum* existing in sugars of the old process!

To banish these fears from the public mind, the patentees say that the sulphite of lead left in the sugar is not a poison; and, in support of this assertion, they quote the opinions of two gentlemen who, so far as we can ascertain, have had no experience whatever on the subject of lead-poisoning—namely, Dr. GREGORY and Mr. WARINGTON. The former gentleman performed a set of experiments on rabbits, for the inconclusive nature of which we refer our readers to some remarks made elsewhere.* Without attempting to determine whether the sulphite of lead is soluble in the *secretions of the stomach*, Dr. Gregory alleges that it is *not poisonous*, owing to its excessive insolubility in *water*. In the next sentence of his letter, the carbonate of lead, which is also remarkable for its excessive insolubility in water, is described

* See MEDICAL GAZETTE, August 30, p. 378.

by him as the "truly poisonous compound of lead!" Mr. Solly has corrected the fallacy contained in this inconsistent statement.* We are also able to supply facts which show that if, according to Dr. Gregory's experiments, rabbits are fattened, dogs are liable to be poisoned by sulphite of lead.† In the Parliamentary papers before us there is a letter from Dr. Gregory, announcing that sulphite of lead is "perfectly innocuous, and as harmless as so much chalk would be."‡ Mr. Warrington has no experience of his own to offer, but he gives his opinion that Dr. Gregory's experiments satisfactorily show that sulphite of lead is an innocuous substance. On this we shall simply remark, *Ne auctores ultra credidas*.

One strange argument which has been brought forward in favour of the lead-sugar process we must briefly notice in bringing this long article to a close. Some sugars of the old process have been found to contain lead in small quantity, arising from the use of leaden pipes and cisterns, as well as moulds painted with white-lead. We have the admission of Messrs. Goodhart, in their letter already quoted, that they, the patent sugar-makers, employ these leaden articles of apparatus in common with other sugar-refiners.§ Hence it

has been inferred that, as lead-poisoning already goes on, to a certain extent, in the consumption of ordinary sugar, it is only reasonable to allow it to go on to a still greater extent by the encouragement of the use of the patent sugar! We demur to this inference, and contend that, so far as it may be practicable, rules should be enforced to prevent the introduction of lead, whether accidentally or designedly, into any description of sugar.

THE CHOLERA IN JAMAICA.

THE most recent intelligence from the West Indies states that the greatest excitement prevailed in Kingston, Jamaica, in consequence of a case of cholera having occurred in that city. At the opposite port, Port Royal, during the few days previous to the steamer's departure, a number of cases had occurred, and about twenty proved fatal. The case in Kingston is described by one of the newspapers as being the "malignant Asiatic cholera;" but it is denied by the medical testimony, which declares that the patient died of a "form of cholera." In Kingston the civic authorities, who had previously been very inactive, had become very vigilant. Meetings of the Board of Health had been held, and a sanitary committee, appointed by the corporation, met every morning. No official returns were made of the cases in Port Royal; but, notwithstanding accounts which reached Kingston were greatly exaggerated, it was an ascertained fact that the cholera had appeared there, and the inhabitants were in the greatest excitement and alarm in consequence. The governor of the island had sanctioned the employment of the convicts in the Penitentiary on the roads of the city, for the purpose of keeping clean the streets and lanes, and the Commissioners of Public Accounts voted £1000 for the purpose of effectually carrying into effect the various sanitary regulations made and to be made. Judging from the form the disease had assumed, and the few cases that had appeared, it was hoped that its ravages would not be great.

* See Pharmaceutical Journal, October, p. 248.
† See in the present number, page 856, an account of the recent experiments of Mr. Edmund Greaves.

‡ Dr. Gregory goes even farther than this, and says—"I should have no objection personally to use the sugar made by Dr. Scoffern's process, even if some sulphite were accidentally mixed with it." We trust that Dr. Gregory will not thus offer himself as a chemical martyr on the altar of science. The fate of the dogs experimented on by Mr. Greaves (see page 856) holds out a wholesome warning.

§ Admitting this to be a correct statement, the patent sugar should contain the amount of lead found in sugars of the old process *plus* that which is introduced by the employment of salts of lead, and not removed by filtration. This affords another very curious illustration of Mr. Warrington's mode of dealing with figures. It will be seen, on reference to his tables, that the patent treacle, instead of containing *more*, actually contains *less* lead; the difference being

0·34 grs. Thus we arrive at this remarkable inference:—By the use of lead-salts in refining patent sugar, you not only get rid of the whole of the lead thus introduced, but actually remove about one-fourth of that which all sugars take up from leaden apparatus, &c.!

Reviews.

Illustrations and Inquiries relating to Mesmerism. Part I. By the Rev. S. R. MAITLAND, D.D., F.R.S., F.S.A., sometime Librarian to the late Archbishop of Canterbury, and Keeper of the MSS. at Lambeth. Pamphlet, 8vo. pp. 82. London: Stephenson.

Mesmerism Tried by the Touchstone of Truth: being a reply to Dr. Ashburner's Remarks on Phrenology, Mesmerism, and Clairvoyance. By GEORGE CORFE, of Middlesex Hospital. Pamphlet, 8vo. pp. 44. London: Houlston and Stoneman.

THE authors of these two pamphlets admit the *facts* of mesmerism—that is to say, the production of certain phenomena known as mesmeric. Thus Dr. Corfe, in his letter to Dr. Ashburner, says:—

"I cannot say that I deny the facts which you adduce. . . . Now if these are facts, then no one can doubt but that there is a power in operation which is not in the possession of every mortal, and that power is supernatural, and beyond finite control. Such I deem to be the fact, unless the whole relation of mesmerism is to be resolved into a tissue of lies and of fraud, which I cannot agree to." (p. 17).

And in like manner Dr. Maitland states his conviction, "that what are called the common phenomena of mesmerism—by which phrase I mean those phenomena (some of them, in one sense of the word, very uncommon) which have been concurrently maintained by mesmerisers in various countries for more than half a century—are realities;" and "that they are, both in themselves and in their indications, very serious matters, and such as demand the deep and earnest attention of all who know their existence" (Preface).

The manner, however, in which the two authors proceed to the inquiry of the nature and tendency of mesmerism, differs widely. Dr. Corfe, the author of a useful practical work on the *Physiognomy of Disease*, denounces the whole of the mesmeric phenomena as the direct work of the Devil, and proceeds to enlarge on its evils as earnestly, as warmly, and as energetically as zeal and the fear of God can urge him. Dr. Maitland, a divine and an ac-

complished scholar, knows to us as the learned author of that most interesting work, "The Church in the Catacombs," proceeds more deliberately to state the facts, and to inquire into their character and tendency. The professions seem here, for the nonce, to have changed places: the physician is found vehemently consigning mesmerism and its professors, on religious grounds alone, to the paternal charge of the Father of Lies; while the clergyman is found dispassionately and logically arguing, from certain known premises, to their legitimate inferences. It is to the work of the latter, on this account, that we shall principally direct our readers' attention.

Dr. Maitland cites a series of cases from the *Zoist* in "illustration" of clairvoyance, without in the least casting any doubt upon their truthfulness, as will be seen by the following extracts:—

"If the reader has made up his mind to believe or disbelieve, without regard to testimony or evidence, either side of a question which has been discussed with disgraceful bitterness, ignorance, and folly, I am not anxious about his approbation; if not, I hope he will acquiesce in the propriety of this course, and will also understand and approve the motives which lead me to take the illustrations for this section (clairvoyance) exclusively from the reports of cases published in the *Zoist*. That periodical work is the principal organ for the diffusion of information on the subject, and it is countenanced and contributed to by the chief patrons and practitioners of mesmerism. It has not yet been seven years in existence, and all the cases to which I refer in this section have occurred within that period, and in our own country. The reader, therefore, who wishes to verify my transcripts, may do it by reference to one English book; and if he likes to go a step farther, and inquire respecting the genuineness and authenticity of the stories themselves, he may in a good many cases, without much trouble, inquire on the spot, and put his questions to the parties actually concerned." (p. 1).

Most of these facts are familiar to our readers: they are offered by the author as "illustrations," not as "proofs." We do not, on our part, doubt the genuineness of the narrative; but we cannot entirely exclude the possibility of deception on the part of the performers in all cases, remembering the exposure of Alexis and others by

DR. FORBES AND SHARPEY. On this point, however, we shall not now press, but shall be content to admit all that Dr. Maitland requires. We pass at once to the consideration of mesmerism "volition," Dr. Maitland's account of which we shall present in the author's own words:—

"That one person should be able, by the simple exercise of his own will, to influence the will, alter the state, and regulate the actions of another—of another, too, who is distant, and altogether unconscious of being the subject of such influence—is certainly very hard to believe, and constitutes one of the most wonderful of the alleged facts of mesmerism.

"It has, however, been asserted, and strenuously maintained, for more than half a century, by persons whose statements are entitled to attention. Whether they are right or wrong in all that they say I am not undertaking to decide; and, indeed, it is not my purpose at present to inquire. My object, and my earnest desire, is to call the attention of sober and reflecting persons to what is a plain and indisputable truth—namely, that this alleged fact is now maintained as a reality by such a weight of character, and such a body of evidence, and is assuming a form of such importance, that it ought to be investigated by those who, though they may have no particular taste for such inquiries, have some fear of God before their eyes, and some concern for the welfare of their fellow-creatures."

This "submission to the overpowering influence of the will of another," Dr. Maitland investigates in the cases recorded by mesmerists. We quote one or two of the shortest of the instances adduced. It appears to us to be an important fact, and one worthy of note, that they are believed by such an inquirer as Dr. Maitland:—

"When urged to look at any thing, she expressed the greatest repugnance to do so; and it was only at the reiterated command (mental, be it observed) "of the mesmerizer that she aroused herself to the necessary effort. At these times her whole deportment was that of a person who wished to rest, yet who, by some external force, was compelled into exertion. No exorcised spirit could have done its work more grudgingly; and, like the enchantress evoked by Odin, she continually entreated to be left to repose." (Townshend's Facts in Mesmerism, p. 46.)

"She responded to the excitation of the phrenological organs, and felt obliged to obey the will of the mesmerizer up to the

point of following him about the room; but if she were commanded to do anything very repulsive to her feelings, she became awake instantly and suddenly." (Ashburner, in *Zoist*, No. xiii. p. 182.)

The next extract we quote is from the pen of the lady in whom the phenomena are said to have occurred:—

"After being thrown into my usual state, Mr. Nixon retreated a few steps from me, when I felt a strange sensation of uneasiness, and my arms stretched out, pointing whichever way Mr. Nixon moved; my mind all the while active and clear, though the power to control my movements was entirely gone, and I felt drawn irresistibly, as the needle by the magnet. After a time my uneasiness increased, and I rose and followed my mesmerizer, my eyes still being closely shut." (*Zoist*, No. xiii. p. 238.)

The same young lady also puts on record the following:—

"I was speedily under the mesmeric influence, my body and senses subdued and under control, but my thoughts, as usual free and clear. . . . This evening I followed my mesmerizer unerringly through the room with closed eyes, and answered correctly to pressure over several of the organs of the head. When an organ was touched over I felt irresistibly impelled to follow the indication, though perfectly aware of what I was doing: for instance, Mr. Nixon, meaning to touch Firmness, happened to touch Veneration, and I fell on my knees, my thoughts turning to God and heaven. When Firmness was really touched I was compelled to draw myself up to my full height, and aspire, as it were, to reach the very ceiling." (*Zoist*, No. xiii. p. 239.)

We cannot quote the whole of this passage; we leave our readers to make their own comments thereon, when we have laid before them Dr. Maitland's views. With reference to the mesmeric states of "attraction" and "attachment," Dr. Maitland quotes from Dr. Elliotson:—

"I have invariably observed, without a single exception, in all my mesmeric experience, from the time of the Okeys, in 1837, to the present day, that the mesmeric state has, even if characterized by affection, and the most intense affection too, apparently nothing sexual in it; but it is of the purest kind, simple friendship, and indeed exactly like the love of a young child to its mother; for it seems characterized by a feeling of safety when with the mesmerizer, and of fear of others. Those who think they have seen anything else must have seen with

the eyes of a prurient impure imagination, unless the unjustifiable experiment of mesmerising Amateurs has been made."

On this Dr. Maitland very properly observes:—

"Could we be sure that persons who have unjustifiable designs would abstain from unjustifiable experiments this would be perfectly satisfactory—but what an 'unless!'"

In a note on this passage Dr. Maitland also remarks:—

"Very probably that 'unless, &c.' is just what alarmed Dr. Mayo, and will alarm others,—not the less, because on this repetition, though the candour and honesty of the author induced him to include the 'unless,' he considered the ' &c.' too obvious to require explanation, or even statement. Certainly that one word from Dr. Elliotson has startled me more, and has more weight, than all that I have heard from the opponents of mesmerism."

Again:—

"One is tempted to ask whether if the clairvoyante of whom we have just read, instead of being told to blow out the candle, had promised to throw herself out of the window, or stab one of the company, she would have found that 'the impulse was irresistible;' and, if so, whether a promise made in private to fire the house at midnight, or become the instrument of any other crime, would have been as punctually performed. . . . Such power, even in the hands of the wise and virtuous, is tremendous; but is it, or can it be, confined to them in the days when science is the idol, and its fervent worship consists in popularising its discoveries? I know, but it does not entirely remove my fears, that no harm is likely to come of even this, unless some of the sedate million should make unjustifiable experiments." (p. 41.)

That these "unjustifiable experiments" have been made is not only not denied, but avowed, with reference to another line of magnetic investigations.*

Dr. Maitland next inquires, respecting his "illustrations,"—Is all this true? And his answer, if we rightly understand him, is to the effect that not only is it all true, but it is not the whole truth; that still more startling facts might be related.

* "Point to Ametivness, and the woman, if her organs are at full, is unable to control her actions; she burns with desire." See note by Dr. Ashburner, in his translation of Reichenbach's *Researches on the Dynamics of Magnetism*, &c.

"Is it supernatural," the author asks, in the next place: we quote his answer:—

"I am content on the present occasion to treat of mesmerism as merely the exercise of a power which belongs naturally to men, or to one man in a given number—a power which (so far as that can be said of any which he possesses) he can use according to his own will, to produce certain effects on other men, independently of all that is external to himself—a power, let us suppose, not more visible, or ponderable, or explicable (but at the same time not more supernatural or miraculous) than the muscular energy and mental volition which enable one man to knock another down. I am content to take this merely as an hypothesis, in the belief that it will save trouble and not injure truth." (p. 47.)

What is it, or what is it like? forms the next inquiry. The answer involves the consideration, at some length, of the ancient divinations, magic, witchcraft, familiar spirits, &c. mentioned in the Scriptures, with which the author identifies its phenomena. We cannot abridge this part of his investigations without damaging the author's argument; we therefore refer our readers to the original pamphlet, where they will be rewarded by the fruits of the author's erudition and biblical knowledge.

We conclude our remarks on Dr. Maitland's pamphlet with his observation on a "most interesting and important question," and one which, observes the author, "if I did not know that it engaged the deep and anxious attention of others as well as myself, it would not be worth asking—Is mesmerism divisible?" Here, again, we prefer giving the author's answer in his own words: and if we seem to occupy more space than mesmerism would lead them to suppose it deserving, we may remark that the tone and the spirit of Dr. Maitland's pamphlets, as well as the conformity of his views in certain respects to our own, demand that, for the interest of truth, we should bring it prominently under their notice.

To state, then, our author's answer:—

"For the convenience of writers, and readers, and talkers, who are supposed to understand one another, a phraseology has been adopted which seems to assume that it is (divisible); and without any one's pretending to draw a strict line of demarcation, we are accustomed to read and speak of the 'higher' and 'lower' phenomena of mesmerism. That the phenomena thus

popularly divided do, in many cases, exist separately there is no doubt. But are the things essentially distinct, or only different parts, or degrees, of the same thing? We might even ask, "What security has the patient when he puts himself in the hands of the mesmerist against such a subjugation as amounts to a voluntary abandonment of the power of reason with which God has endowed and entrusted him? and what security has the mesmeriser (who adopts the Bible) that he is not usurping that very influence over his fellow which God has forbidden him to exercise?" If it be said, "God hath given men these powers, and are we not to suppose that he meant them to be used?"—the plain answer is, He has given men power to commit murder, theft, and a variety of things which he has forbidden; and that, not only in the gross form of the act itself, but as to those courses, practices, and dispositions which lead to it." (p. 68).

"In the meantime, I hope I am not wrong in putting forth a few words of excuse, or at least of deprecation, on behalf of those who hesitate to take an active part in promoting the use of mesmerism on what philosophers may think foolish and superstitious grounds. We do not consider ourselves as more inhuman than those whom science has rendered intelligent, virtuous, and happy. We think that while we are withheld by what others may consider, and what may actually turn out to be, unfounded fears, from actively promoting the practice of mesmerism, it is unjust to hold us up to public odium as persons who 'dislike to see a racking pain removed by it; to see the feverish, sleepless invalid, enjoying a balmy slumber by its aid; to see the nervous, excited patient, restored to comfort and repose.' For myself, at least, I know that I may honestly disclaim such feelings; and I shall be most grateful to any one who may be able to remove the scruples which I feel bound to express, by fairly meeting the question which I have placed at the head of this section, and distinctly showing that therapeutic mesmerism is separable from, and no part of, the curious art of soothsaying and divination which busies itself in the banker's shop or the pawnbroker's cellar" (p. 70).

Dr. Maitland, we consider, has here brought mesmerism to the "touchstone of truth," to the test of the standard of right or wrong. We thank him for this first instalment of his inquiry, and hope that he will not long delay the remaining portions.

We must not, however, conclude without bestowing a few more words on Dr.

Corfé's pamphlet. We honour the author's religious principles, but would fain hear less loudly the sounds of the "drum ecclesiastic." Dr. Corfé's zeal carries him sometimes a little beyond the limits of the apostolical injunction, which says, "let all your things be done with charity," and "let your moderation be known to all men." The following short extracts will justify our censure of Dr. Corfé's vehemence:—

"The spirits of phrenology and mesmerism are twin devils, co-partners, fraternal spirits of uncleanness." (p. 25.)

"You appear as a disciple of Mesmer and of Gall, and an apologist for the Devil, who is the author and abettor of both clairvoyance and of phrenology." (p. 24.)

But we should also, in justice to Dr. Corfé, cite some of his more deliberate and calmer statements with reference to the effects of mesmerism:—

"I firmly assert, that throughout the whole period of nine years, during which mesmerism was more or less practised by yourself and Dr. Wilson, whilst holding the office of physician to this charity, I never once saw or heard of an instance of cure effected through its agency; but, on the contrary, I have witnessed many serious evils result from its employment." (p. 16.)

Among these "serious evils," we quote the following:—

"I never saw nor heard of anything of the sort go forth but what was indecent, disgraceful, and injurious. All the patients have unequivocally expressed themselves worse, where real disease existed; and where nervous disorders only were present, the mind has been worked upon, the lascivious passions have been excited, and the will, unable to control the animal desires, has lost its balance, and the patient has been momentarily stupified by the 300 or 400 passes to and fro before the eyes, but only to awake in a more libidinous state than before: and this has sent them, sooner or later, to wander the streets to gratify their lust, and thus they have become open prostitutes." (p. 21.)

The authors before us, as we have already remarked, unite in admitting the facts of mesmerism: they differ in its source, but they coincide in its uselessness and danger. And surely, in the present utilitarian age, facts which present nothing to attract but their marvellousness, must ere long come to be disregarded and despised; or, having a potency of some kind, but not for good, must be dreaded and shunned by

all who, looking to the issue of their actions, would not lightly tamper with the responsibility and free agency of their fellow men.

The Elements of Materia Medica and Therapeutics. By JONATHAN PEREIRA, M.D., F.R.S., F.L.S., &c. Third Edition. Vol. II., Part I. London: Longman. 1850.

As many inquiries have been made of us respecting the publication of the second volume of this well-known and valuable work, we take an early opportunity of announcing that the volume will be published in two parts, and the first part is now before us. This part is a goodly volume in itself, as it covers 639 pages, and contains 173 illustrations, without including an admirable steel-engraving representing the microscopic characters of different varieties of starch.

The second volume is devoted to the *Organic Bodies of the Materia Medica*, and the present part comprises the history and properties of a large variety of vegetable substances which are extensively employed, either as articles of food, or in medicine. It is unnecessary to enumerate these, as we are now dealing with a standard work in its third edition. We shall only remark that every article bears witness to the industry and indefatigable research of the author. Instead of being merely the elements of materia medica, it constitutes a complete encyclopædia of this important subject. The student of physiology, pathology, chemistry, botany, and natural history, will find herein the most recent facts and discoveries in his favourite branch of study, and the medical practitioner will have in this work a safe guide for the administration and employment of medicines.

The Natural History of the Varieties of Man. By ROBERT GORDON LATHAM, M.D., F.R.S., &c. &c. 8vo. pp. 574. London: Van Voorst. 1850.

THE writings of Dr. Prichard have given an impulse to ethnological researches that has been productive to them of great and, we trust, permanent benefit. Physiologists have learnt that ethnographical distinctions are not to be traced exclusively, or even chiefly, in variations of physical conformation, but that their most correct data are to

be discovered by the philologist. It has hence resulted that a vast mass of new matter has been rapidly accumulating since the date of Dr. Prichard's labours, recent though they be. The Ethnological Society of London has been actively employed in concentrating this later information, and has embodied much thereof in its "Proceedings." We may observe that the author of the work before us is a prominent member of that Society, and has the reputation of being one of the most eminent philologists of the present day.

The present treatise, from the name of the author, may be expected to possess the attribute of originality, so far as the working up of a large amount of material, collected for the most part by the labours of others, may admit of originality. The readers of the book will not be disappointed in their expectations on this point. A different classification to that of his predecessors has been adopted by Dr. R. G. Latham, the primary divisions of the varieties of the human race being limited by him to three—the Mongolian, the African, and the European. The terms by which these are expressed by Dr. Latham are: Mongolids, Atlantids, Japetids. It is not in treating of the primary divisions alone that the particular features of this work consist.

It would carry us beyond the limits which we can afford on the present occasion if we were to follow the author through all his subdivisions; but we may direct attention to the introductory observations, in which Dr. Latham expounds the principle of their formation. Preceding the consideration of each subdivision and variety is a tabular statement of its chief points—e.g., physical conformation, languages, area, locality, political relations, religion, mode of writing, numbers, synonyms, &c. &c., according as each presents peculiar features of importance.

Dr. Latham concludes his volume with a short notice of the general character of the science of ethnology, in a series of general and special apothegms.

The author remarks, that "a subject that a single book, however encyclopædic, can represent, is scarcely worth taking up in earnest." The work before us, although deserving the title of "encyclopædic," as those who peruse it will discover, certainly does not fully repre-

sent the entire subject. Classification is the chief end of this work; and, having thus brought forward his classification, we doubt not that Dr. Latham will prosecute his philological researches to the filling up of the outline which he has herein so ably and distinctly traced. The materials abound; and from one so skilled in languages, and so competent in every respect to the task, we hope that perseverance and industry will in due time produce that which we anticipate—a complete modern work on ethnology. In the meantime the present volume cannot fail to be most acceptable and useful to the students of one of the most interesting branches of knowledge.

Prophylaxis; or, the Mode of Preventing Disease by a due appreciation of the grand Elements of Vitality—Light, Air, and Water: with observations on Intramural Burials. By EDWARD BASCOMBE, M.D., &c. Pamphlet, 8vo pp. 29. London: Higley.

THIS pamphlet was "intended to form part of a work which will be shortly published, entitled 'A History of Epidemic Diseases from the Earliest Ages. 1491 years before the Birth of Our Saviour, to the Present Period, &c.'" The interest excited by the outbreak of cholera at Tooting induced the author to publish at that time "his ideas as to what should form the groundwork—the very basis of all sanitary measures." Dr. Bascome cites from historians many instances of the occurrence of disease and death, from deficiency of the "grand elements of vitality," and enforces the paramount importance of their supply in purity and abundance, as the "basis of all sanitary measures."

Lectures on Inflammation. By JAMES PAGET, Professor of Anatomy and Surgery to the Royal College of Surgeons of England, Assistant-surgeon to St. Bartholomew's Hospital, &c. Pamphlet, 8vo. pp. 57. London.

THESE Lectures have already appeared in our pages. Our readers are, therefore, well acquainted with their contents. They have been extensively transferred to the pages of other periodicals, and are admitted to be among the most valuable of the latest contributions to pathological literature. We now merely announce their publication in a separate form.

Proceedings of Societies.

MEDICAL SOCIETY OF LONDON.

Nov. 1, 1850.

DR. J. R. BENNETT, PRESIDENT.

Health of London during the six months terminating September 28, 1850.

DR. WEBSTER, the author of this paper, stated that the health of London had been recently very satisfactory compared with ordinary seasons, but still more so when contrasted with last year. Thus, during the six months terminating the 28th of September, 22,816 persons have died in London, instead of 40,117 in the parallel season of 1849, being a diminution of 17,301 deaths, or 43.12 per cent. in favour of the present compared with the former period. This great difference is chiefly owing to the very diminished mortality by cholera; only 96 individuals having died from that cause, instead of 13,115 during the same months of last year. Diarrhoea and dysentery have likewise proved less fatal; the deaths by those maladies being 1459, instead of 2946 in the same six months of 1849. Measles, scarlatina, and hooping-cough, have also prevailed less severely than previously. Besides these complaints, typhus, which, in the second and third quarters of 1849, caused death to 1222 individuals, was fatal in only 900 instances during the past six months. Phthisis, pneumonia, and bronchitis, also come within this category; likewise convulsions and erysipelas; 168 patients having died of the latter disease during the recent season, instead of 213 in the same six months in 1849. Notwithstanding the diminished total mortality of London during the period embraced in Dr. Webster's reports, several complaints have exhibited an increased fatality. Amongst these influenza and cancer were mentioned; as also insanity, hepatitis, and ovarian dropsy. By the latter disease 35 females are reported to have died from the 30th of last March to the 28th of September; whereas only 20 fatal cases from the same cause were met with during the parallel six months of 1849. Several maladies, on the other hand, have exhibited nearly the same rate of mortality as in the previous season. Thus, 67 persons died by Bright's disease during the past six months, against 65 in the parallel period of 1849. From puerperal diseases, again, 200 fatal cases were recently recorded, in contradistinction to 210 during the same two quarters of last year. The author then alluded to

apoplexy, hernia, and delirium tremens, by which latter serious complaint 96 persons died recently; whilst the number of deaths by the same cause was 94 in the former season.

Dr. Webster then alluded to the great mortality among children, shown by the fact that, of the 22,816 deaths recorded in the metropolis during the last two quarters, 10,242 were individuals who had not passed their fifteenth year. Violent deaths next occupied the author's attention, by which causes 767 persons are reported to have lost their lives in London; thus making one case in every 30 of the whole mortality. Of these 767 specified violent deaths, 208 were produced by fractures and contusions, 155 were drowned, and 89 died from burns and scalds, besides other casualties not necessary to particularise. After discussing at some length the mortality met with in lunatic asylums, lying-in hospitals, workhouses, and other eleemosynary institutions, the author alluded to the mortality in the London gaols, which recently contained 6110 inmates, 5432 being men, and only 630 women; amongst whom the deaths reported during the last six months amounted to 44 cases, comprising 35 male and 9 female prisoners. Indeed, so healthy have these receptacles of crime and misconduct been of late years, and very different from last century, that, amongst 10,015 individuals committed to Coldbath-fields prison, only 6 deaths have occurred in so large a number of persons. However beneficial confinement in the metropolitan prisons may prove to the bodily health of the residents, it sometimes appears to produce an opposite effect upon their mental condition, particularly in those undergoing solitary or separate punishment. This baneful influence seemed fully established by the fact that from two large London gaols—viz., Pentonville and Millbank—where only convicted criminals are confined, not fewer than 61 prisoners were sent to Bethlem Hospital during the last ten years who had become insane, 47 being men and 14 women, besides four male criminals who came from the hulks, but had previously resided in Pentonville prison. In addition to the above 65 individuals, male and female prisoners had been also admitted from other prisons as lunatics into Bethlem Hospital, although to a much smaller extent; hence showing that the effect of silent and long-continued confinement upon the mental faculties is very decided; and it should be remembered (the author likewise said) that all the cases of insanity recently sent from the two metropolitan prisons, and now reported, were not persons acquitted because

they were insane, but prisoners actually undergoing sentence for previous crimes and misdemeanours.

Hospital mortality.—The eleven general hospitals of the metropolis next occupied the author's attention, respecting which establishments some important remarks were made. From these it appears that 1051 persons were reported to have died in the above charitable institutions during the last six months, which are classified in the subjoined table:—

Statistical Table of Deaths in the eleven General Hospitals of London, during six months, terminating the 28th September, 1850. Compiled by Dr. WEBSTER.

Hospital.	No. of Beds.	Total Deaths.	Ratio of Deaths to Beds.
St. Bartholomew's . . .	580	180	1 to 3 $\frac{1}{4}$
Guy's . . .	580	164	1 to 3 $\frac{1}{2}$
St. Thomas's . .	487	115	1 to 4 $\frac{1}{2}$
London . . .	320	120	1 to 2 $\frac{3}{4}$
St. George's . .	320	106	1 to 3
Middlesex . .	285	86	1 to 3 $\frac{1}{2}$
Westminster . .	175	78	1 to 2 $\frac{1}{4}$
Free . . .	140	22	1 to 6 $\frac{1}{2}$
King's College .	120	66	1 to 1 $\frac{1}{7}$
Charing Cross .	110	36	1 to 3 $\frac{1}{8}$
University College . . .	106	78	1 to 1 $\frac{1}{4}$
Totals . .	3223	1051	1 to 3 $\frac{1}{4}$

Before advertising especially to these statistical data, the author made some general observations respecting the very limited extent of hospital accommodation in London; whilst he also said, that the inmates of these eleemosynary establishments were not always of the indigent classes, but sometimes persons in easy circumstances, or domestic servants from the houses of governors, or even of noblemen; nay, patients who came also from the country. Hence the actual sick poor of the metropolis were not the exclusive participants of the benefits of these institutions. Nevertheless, taking the facts as they appeared, the author deduced the following inferences:—1. One death in every 21 $\frac{7}{8}$ of the total mortality throughout London took place in the above-named charities. 2. One patient died for every three beds, speaking in the aggregate. 3. The highest rate of mortality generally occurred in the smallest hospitals. 4. With one exception, the lowest comparative ratio of deaths was observed in the large, or most popu-

less institutions, 5. The lowest deaths in reference to the number of beds, were reported from the Free Hospital, 6. The largest comparative mortality, according to the same calculation, occurred at University College Hospital. Dr. Webster afterwards alluded to the many advantages which would accrue to the profession, and hence to the public generally, were regular reports of all the "epidemics" in each separate metropolitan hospital periodically published, as such official statements he thought would prove of much interest and practical value. Every charitable institution of the kind throughout the country should adopt this plan; and if some master-mind would then take the whole facts thus supplied, and arrange them in a lucid and instructive manner, the great desideratum, now adverted to, would be effectually supplied. If reports of the kind described were published, they would for the future, to say nothing of the past, become not only of great benefit to all now in practice, or entering the profession, but to posterity. As an illustration of the importance of such documents, Dr. Webster shortly referred to hæmia and fractures, about which considerable difference of opinion sometimes prevails in reference to the treatment. Could the surgeon, the author said, refer with facility, when necessary, to a large array of cases reported by the ablest practitioners, much of the present discrepancy of opinion, on either of those subjects, must give way before the weight of evidence derived from such sources. The treatment likewise of medical diseases, the value of particular remedies, the symptoms characterizing epidemics, the types of even ordinary malades, the rate of mortality observed, and many other interesting questions, would be all greatly elucidated by the system suggested. Consequently, Dr. Webster hoped the parties who had the power would take the subject, now mooted, along with the plan suggested, into their consideration, so as, by a united effort, to remove every difficulty which might retard its realization.

The excess of births over deaths was next noticed; it amounted to 13,532 individuals during the last six months. If to these be added the 10,128 constituting the excess of births over deaths in the two quarters immediately preceding, the total increase of the population from this source would be 23,660 during the past year; which furnishes one cause, amongst others, of the constantly-increasing number of inhabitants throughout this now enormous metropolis. London, in the opinion of the author, is not *per se* insalubrious; but it is the manner of life followed by those who dwell in town which so often proves inimical

to the health of its inhabitants. The various questions bearing upon this important subject were discussed by Dr. Webster, and he also highlighted the rate of mortality met with in the British metropolis, contradistinguished from that observed in various European capitals, all the facts stated proving the great superiority of the former as a salubrious place of residence; London being, in fact, more healthy than Paris, Berlin, Vienna, and even Rome, so famed as a refuge for, but often the grave of, invalids.

The author afterwards proceeded to make a few observations on several circumstances, the judicious or improper application of which materially influences the health of individuals, and hence of communities. These were—1. Food; 2. Clothing; 3. Habits or customs; and 4. Bodily exercise. After investigating some of the above topics, Dr. Webster animadverted strongly upon the present system of tight lacing in females, which fashion sanctions, although reason and experience condemn such practices, seeing they very often prove exceedingly inimical to health. Intemperance and tobacco-smoking likewise received decided condemnation from the author, who further disapproved of the modern custom, now rather prevalent, in the fashionable world especially, of frequently drinking tea on an empty stomach, and previous to meals. If often detrimental to nervous females, this habit is even more so to children and infants, as that beverage excites and renders irritable their physical constitution. Hence he considered the vegetable in question should be put in the same category as opium, tobacco, ardent spirits, or even wine; and it ought never to be allowed to any young person unless as medicine. After some other remarks Dr. Webster concluded his paper by alluding to the great benefits accruing to individuals from properly regulated bodily exercise judiciously employed; he added, however, that the human frame may have too little as well as too much exercise; and on this subject, as in reference to dietary rules, the author strongly urged that extremes should be always avoided; to these questions the well-known saying was most applicable—"In medio tutissimus ibis." Dr. Webster concluded his communication by remarking, that, as the physical constitution of man often wastes from the want of sufficient muscular movement, so will the nervous organization on other occasions become exhausted or worn out by excessive toil, as well as from too great mental excitement.

KING'S COLLEGE MEDICAL SOCIETY.

DR. ARTHUR FARR, F.R.S. PRESIDENT.

THE first meeting of this Society for the session was held in the great dining-hall of the College on the evening of October 17th, Dr. Jeff, the principal of King's College, together with Professors Partridge, Gutz, and Bowman, and numerous students and visitors, attended.

After some preliminary matters, the PRESIDENT arose and expressed his great pleasure at being present in this capacity at the seventeenth annual meeting of this Society, which he announced to be in a most prosperous condition. After recapitulating the advantages which accrue to the student of medicine from enrolling himself as a member, he called upon Mr. Henry Smith to read the introductory address, the substance of which was as follows:—

Mr. President and Gentlemen,—It is but too obvious that in any great seat of learning where numbers are brought together to fit themselves for the prosecution of any particular profession, an interchange of sentiment, and a free discussion on the all-important subjects to which their mental energies are being directed, must be of the utmost service to the students of any science whatever; but under no circumstances, perhaps, are they demanded in a more striking manner than when the study to be engaged in is that of medicine; for here, it must be confessed, there are but few broad and defined principles to guide us: each one holds his particular opinion on the most important doctrines and points of practice; and it will not be going far from the truth in reiterating that which was long ago said of medicine, to a certain extent at least—that it is merely a conjectural art. It becomes, then, a matter of great moment that the future practitioners of medicine, especially those who are studying together in one school, should have the opportunity of obtaining a certain communion of ideas, and of agreeing one with another respecting the great and essential doctrines and practical precepts which are taught them in the lecture-room and in the wards of the hospital. An association like this, which we are now commencing to hold for the present session, will lead to this much-desired object: it will also be a means of joining its members together in the bonds of friendly intercourse, and at the same time they are studying probability, for many ye

that advantage results from the discussions which are carried on here—the student is enabled, if he chooses, to express himself clearly, and to form the habit of so collating and arranging his ideas that he will have no trouble in explaining himself easily and fully to those who may be his auditors on any point with which he is acquainted, and in which he takes interest.

These are some of the advantages which are attainable by those who belong to a society of this description: and I apprehend that they would prove an incentive to those of you who belong to it, to a continuance in it; and to those who have not already enrolled themselves, to do so without delay. You may rest assured that your time will not be mispent by devoting one evening of the week to attendance here. It should be distinctly understood, however, that it is not the idle man, nor he who is not fully impressed with the grave responsibilities attached to his profession, that is likely to do good to himself or to others by joining this Society: it is far better that the idle and careless should have an association of their own: those only who are willing to impart knowledge, and to learn, will either benefit others or themselves by associating themselves in a Society whose only aim is to increase that knowledge which is so difficult to attain, and which is entrusted to us for the relief of the vast mass of human suffering which is incidental to this mortal state.

What is this knowledge, and what are its objects? How great are its pleasures and advantages in more than one sense! It is truly said that "knowledge is power;" but in no case is the truth of this axiom more strongly exemplified than in its connection with the profession of medicine; and in no instance, when we come to look at the matter fairly and conscientiously, do we find that a want of knowledge is weakness, or so likely to lead to consequences ruinous to others and injurious to ourselves. It is true that the student of medicine has many difficulties to encounter in his pursuit after knowledge; but it is equally true that he may overcome them all by the exercise of industry in his various pursuits. Aye, and this is the case even if he possess only moderate talents. It cannot be denied that he who is endowed with a quick perception, a vigorous memory, and a vivid imagination, has the vantage-ground in the study of a calling so truly scientific and difficult as that of medicine; but if he has nothing beyond these, great and inestimable gifts as they are, he will not be able to excel in our vocation. Such a man may be able to speculate finely, and build up theories which are brilliant and dazzling,

but which last only for a time, like "the snow-flake on the river,"

"For a moment white—then gone for ever,"

and which may be succeeded by others equally visionary; but he who is gifted with that precious boon—namely, the power and inclination of exercising an unwearied industry—although he may not possess to an eminent degree those fine qualities of the mind before mentioned, will nevertheless outstrip the clever theoriser in that sound knowledge which is absolutely necessary for the practice of the healing art. In order, however, to get possession of this envied knowledge, his industry must be *constant and unceasing*; and he must not allow any opportunity to slip, by which he may observe the symptoms and treatment of disease, for to the appreciation of symptoms and the treatment of disease all the instruction he is acquiring here finally aims at.

Those only who have actually felt the responsibilities of the profession can fully understand how important it is to store the mind, whilst good opportunities exist, with as many *facts* as possible which relate to the practical part of our professional duties. The most useful and efficient manner of doing this is to study and watch disease by the bed-side—not to be content with merely seeing the patients, but to carry away such a recollection of the cases that they may be called forth to our assistance on any occasion when we shall find the necessity. We cannot be acquainted with too many facts; and therefore, whether as students or practitioners, we should endeavour to see as much of disease as we can, and never to remit in our observations. We shall then most assuredly, in our future career, reap the benefit of our industry; for we shall be able to feel that if life or limb be committed to our care, we have a store of knowledge within us which will guide us in our difficulties and doubts; and we shall feel the pleasure (and great it is) of finding that we have been able to restore to health those who must inevitably have perished, had not the resources of our art been directed in the proper manner. If, on the contrary, opportunities have been neglected, and the practitioner has, when a student, failed to make himself well acquainted with the practical part of his profession, he will find himself in anything but an enviable position when he is called upon to give his opinion, or to act in any emergency. The best are liable to errors in judgment and in action; but he who has not, by continued observation, laid up a good store of facts within his mind, will be *constantly* committing mistakes, thus perilling the

lives of others, and getting himself into disgrace. It is impossible to estimate correctly the evil effects of a want of knowledge of the practical duties of the profession; for, when we know but little, we are too apt to consider ourselves complete storehouses of learning, and thus are led on with an unwarrantable boldness to aim at effecting those things which, in the vanity of our imagination, and in the pride of our ignorance, we deem to be right, but which are in reality mere delusions. And it is only when, by constant industry, careful observation, and some practical experience, we begin to know something in earnest, that we feel what pignies in knowledge we are, and how difficult it is to attain that wisdom which makes us powerful for so much good: but, fortunately, as has been stated with respect to a more substantial but baser article, "*crecitur amor nummi, quantum ipsa pecunia crecitur*," so is it true with respect to the knowledge of our profession;—we love it the more we become acquainted with it; we delight in the steps by which we gain it; we enjoy it when gained; and, finally, we contemplate with pleasure the inestimable blessings which a true knowledge of our noble science brings home to others as well as to ourselves.

Let us, then, Gentlemen—let all amongst us who are either practising or preparing ourselves to practise that which is so difficult to learn, take every opportunity which is presented to us in this noble institution of gaining that instruction which is far above all price; and, as I have before insisted on, one of these opportunities presents itself in belonging to, and taking active part in, our Medical Society. Permit me earnestly to advise those amongst you who have not joined it as yet, or who are new-comers to King's College, to enrol yourselves amongst us, to take part with us in our communion of intercourse and friendship, and to aid us in the acquirement of that knowledge which is so necessary to make us safe and efficient administrators of the healing art. Recollect that almost all the diligent students, and those who have distinguished themselves in this school of learning, or who have gone out into the world, gained the respect of others, and honour to themselves, have been members, and active members, of this Society. I can call to mind several within my own time whose names are well known to you, and whose companionship and friendship it has been a pleasure for me to gain. I can mention the name of George Johnson, who was an active member, and who obtained the prize by his essay on Auscultation. We ought to feel proud of having had such a

man amongst us,—one who, I have no doubt, will some day stand in an enviable position amongst the physicians and pathologists of Europe. Henry Salter, who now holds a prominent position as one of your anatomical teachers, and whose achievements need no eulogy from me, also gained one of the Society's prizes. His brother, James Salter, was one of our active members, and a distinguished student of King's College. We also numbered amongst us Thomas William Neum, whose name, I am sure, will call up pleasant recollections in the minds of those of you who had the good fortune to be under his tuition in the anatomical rooms. This gentleman now holds a responsible position in another school of medicine, and stands high in the estimation of those who know him as a thorough anatomist and a thoughtful and skilful surgeon. I must not forget the name of my valued friend, Samuel Sumner Dyer, who was one of our most active members, and who, on an occasion like the present, read the introductory address. He distinguished himself at this school, and at the hospital, as one of the most efficient house-surgeons who ever held office at that institution; and I was glad to see him, a short time since, in his native town, practising his profession with that skill and success with which he pursued it whilst here, and respected by all who know him. We have amongst us now the names of John Wood, William Farley, and George May, all of whom we are justly proud of.

Finally, Gentlemen, let us bear in mind the extent of that which we have to become acquainted with, and, above all, let us keep and nurture in our bosoms a feeling of the immense dignity and importance of our god-like profession—how incalculable are the benefits which we may one day bestow upon our fellow-creatures if we acquire a sound knowledge of our art, and administer its resources rightly—and how much misery and disgrace we may bring upon others and ourselves if we neglect opportunities, and will not be wise. Let all our proceedings in this Society, be so carried on that they will conduce to our mutual advantage, and assist us in the acquisition of that knowledge with which each of us must be armed. Forearmed by this knowledge, we shall be able, when we enter upon the grave realities of life, to carry on our calling in an able and conscientious manner;—we shall not be deterred nor dismayed by difficulties which we must all encounter; we shall be buoyed up by the hope that perseverance will overcome them, and with the feeling that our best energies are being directed towards relieving the manifold sufferings which we see around us: and

when that time comes when it shall be no longer permitted to us to wield the resources of our art—when the eye waxes dim, and the arm is becoming powerless—may we say, with the great Roman orator, "*Nealubet enim mihi deplorare vitam, quod multi et si docti saepe fecerunt; neque maxime penitet, quoniam ita vixi ut non frustra me natum existimem.*" Or may we be able to say, with the calmness and simplicity with which the illustrious Percival Pott exclaimed to those around his dying bed—"My lamp is nearly out: I hope it has burned for the benefit of others."

Mr. ROBERT DUBUET rose with much pleasure to move a vote of thanks to Mr. Henry Smith for his interesting address. He had no doubt that all present had listened to it with the same gratification which he himself had felt.

Dr. ARLIDGE, in a brief speech, seconded the motion, which was unanimously carried.

Mr. SAMUEL GRIFFITH moved a vote of thanks to the reverend the Principal, the Dean, and the medical professors and visitors who had honoured the Society with their company.

Mr. JOHN WEED had much pleasure in seconding this motion. He was glad to see present some of the professors who had formerly belonged to this Society. He more particularly alluded to the distinguished professor of physiology, Mr. Bowman.

Dr. JELLY, the Principal of King's College, rose and stated his extreme gratification at being present at this meeting, and begged to return his thanks for his brother professors—if they would permit him to term them so—and himself. He had been present at the introductory meeting of this Society last session as well, and he must confess that he was much pleased with what he had heard on both those occasions; and it was with no small feelings of pride that he, although unskilled in the science of medicine, was nevertheless at the head of one of the largest medical schools in the metropolis. He wished the Society well, and had no doubt it would prosper.

Mr. HEADLAND moved a vote of thanks to Dr. FARRE. He was sure that no eulogium was necessary in the case of a professor who was so well known and esteemed. As a student and office-bearer of the Society, he was grateful for the attendance of the eminent man he saw around him this evening.

Mr. HENRY SMITH seconded the motion; and Dr. Farre, having returned thanks, the meeting separated.

PHARMACEUTICAL SOCIETY.

Nov. 13, 1880.

Experiments on the Action of Sulphite of Lead on Dogs. By EDMUND GREAVES, Esq.

At the meeting of the members of this Society, on Wednesday evening last, Mr. Greaves read a paper, showing the results of the action of *Sulphite of Lead* on dogs, when administered with food internally, or applied by inunction externally. The results, as it will be perceived by the two fatal cases recorded, clearly prove that sulphite of lead is not as "harmless as chalk," but on the contrary, that it produces all the effects of lead-poisoning on the animal body. Its detection in the liver proves that it undergoes absorption, and that insolubility in water no more prevents the operation of this substance as a poison, than it prevents the action of the carbonate of lead under similar circumstances. The doses here used were comparatively large, but the object of the experimentalist was to show whether this salt is or is not capable of producing a *noxious* action on the system. This is now clearly established: hence the effect of small doses long continued cannot fail to be injurious, although it may be extended over a longer period of time.

EXPERIMENT NO. 1.—Internal action of Sulphite of Lead.—To a young healthy dog was administered daily twenty grains of carefully prepared sulphite of lead in animal food, commencing on Tuesday, October 23rd inst. No apparent effect was produced until the seventh day, October 30th, when from all appearances he was labouring under a severe attack of colic, which continued for nearly two hours. No further symptoms were manifested until the tenth day, November 2nd, when another violent attack of colic came on, which continued for several hours. On the thirteenth day, November 5th, the appetite, which had hitherto been very good, began to fail, the food being taken with apparent suspicion.

On the fourteenth day, Nov. 6th, the animal took with great reluctance his last dose; at 10 A.M. another fit came on, accompanied with great prostration of strength, and at intervals there was great stiffness of the limbs. The dog would not allow any one to approach him, apparently suffering from excruciating pain, and manifesting the symptoms of poisoning by lead, which continued until death terminated his sufferings, at half past 2 P.M.

The total quantity of sulphite of lead taken was 280 grains. At the commencement of the experiment the dog weighed 16lbs. 12oz.; at the conclusion, 13lbs. 8oz.: thus having lost 3½lbs.

Upon examining the liver, a considerable portion was found to be of a dark colour, and upon treating in the usual method for the detection of lead, unmistakable evidence of its presence was obtained. The brain also gave similar indications, though in a less marked degree.

EXPERIMENT NO. 2.—Internal and external action combined.—To an old dog was administered daily twenty grains of carefully-prepared sulphite of lead in animal food; in addition to which there was rubbed into the skin of the abdomen (the hair having been shaved off) daily one drachm of an ointment composed of equal parts of sulphite of lead and lard. No evident symptoms appeared until the fifteenth day, when the dog refused to take any food, as also on the sixteenth day, when he appeared extremely sleepy and inactive: he would not take his usual dose. On the seventeenth day he took a little milk only, refusing to take the sulphite disguised in various ways: he was evidently ill. On the eighteenth day he was with difficulty induced to take a small quantity of food containing the sulphite. On the nineteenth he reluctantly took a small quantity of food, with the usual dose of sulphite: from four to seven o'clock, P.M., on the same day he barked considerably, appeared more restless and lively than he had previously been for several days: at eleven o'clock, P.M., he was prostrate upon the floor in a senseless state, and foaming at the mouth; at intervals he with difficulty raised himself upright, but failed in attempting to walk. In this state he continued, alternately attempting to move about, and relapsing into a lethargic state, until his death, which occurred about five o'clock, A.M., on the twentieth day—November 11th—the animal having taken 120 grains of sulphite of lead internally, and having had externally applied 600 grains.

On analysing the liver, indications of the presence of lead presented themselves.

This animal weighed, at the commencement of the experiment, 28lbs. 12oz.; at the conclusion, 21lbs., having lost 7½lbs.

ACADEMY OF SCIENCES, PARIS.

Oct. 28, 1880.

Phosphene in its relations to Myopia and Presbyopia.

M. SERRES transmitted another note on this subject, in which he stated that the

phenomenon is more readily and completely produced in the myopic than in the natural state of the eye; the nasal phosphene equalling the temporal in brightness, constancy, and extent. These facts, according to M. Serres, invalidate the opinion of M. Steber, of Strasbourg, that myopia consists in a peculiar condition of the retina, or its complication with amblyopia or incipient amaurosis. When the retina is the seat of disease, phosphene is not produced. The evidence derived from phosphene shows (contrary to general opinion) that the eye most affected with short sight is the weakest, and that the retina of that eye will have more acute perception than the other eye, where the two are unequally affected.

In presbyopia, where both eyes are affected alike, the phosphene is produced in much the same degree as in the normal state.

Composition of the Liquor Amnii and Allantois.

M. STAS communicated in a note the result of his examination of the liquor amnii of the chicken, in which he had uniformly found the bi-urate of ammonia. The uric acid was not met with when it did not also exist in the cloaca even before it was found in the amnion, proving that it had reached that cavity from the kidneys.

M. Stas had not discovered either uric acid or urea in the fluid of the allantois, but a peculiar organic matter, which, for want of sufficient quantity, he had not been able to analyse: besides this, he had detected chlorides, sulphates, and phosphates.

The allantois of the calf contained all the salts that are present in the urine, but neither hippuric nor benzoic acid. Albumen, caseine, and grape sugar, were also found in this fluid.

M. CL. BERNARD claimed priority of the discovery of sugar in the allantoic fluid of the calf.

Origin of Cholera.

M. GUINET addressed a note in which he endeavoured to show that cholera is the result of malarious emanations similar to those which produce ague; that the poison having been once produced, the cholera in one individual is capable of transmission to another; that it operates, by means of atmospheric air, through the pulmonary mucous membrane, and not by cutaneous absorption; and that it is not contagious in the literal meaning of the word.

Hospital and Infirmary Reports.

KING'S COLLEGE HOSPITAL.

Ununited Fracture of the Femur—Excision of the ends of the bones.

By far the greater number of fractures of the bones of the extremities which come under the notice of the surgeon turn out well; and they only require, for the most part, very simple treatment, and but little alteration of it, during the time that the reparative process is being undergone. The surgeon merely places the broken ends of the bone in apposition, and almost entirely leaves to nature the work which is necessary to be done. The apparatus which is in use is so simple that it is neither difficult to apply nor troublesome to readjust; and the main duty of the surgeon is to watch the case with care. It is true that complications may exist or arise which will give some trouble: there may be difficulty in keeping the fractured portions in approximation, either from undue muscular action, or from the restlessness of the patient himself, or a simple fracture may become converted into compound, in consequence of inflammation occurring at the injured spot from some accidental cause, as pressure of bandage or splint. Under such circumstances, even, by a proper amount of care, and by so modifying his treatment as will suit each emergency as it arises, the surgeon will in general find these troublesome cases do well; but, every now and then, in this series of surgical cases, as in others, he will meet with instances in which all ordinary efforts to bring about a cure will fail him, or in which, if a cure is produced, it is such as will be neither useful to his patient nor pleasing to himself. For instance, a fracture of the upper third of the femur may happen to an individual during the time that he is in the raving delirium of fever, or fever may attack him a few days after he has received such an injury. Under such circumstances, it will be with the greatest difficulty that the limb can be kept quiet; and it will happen that the patient recovers with a more or less crooked limb. But the most troublesome, and, fortunately, rare feature in a case of fracture consists in the inability of nature to bring about bony union, or in forming merely a ligamentous bond of junction between the ends of the broken bone. Such a circumstance will occasionally be seen amongst a large number of cases of fracture. The cause of this complication depends upon various circumstances: some-

times it will be evident, from the existence of some particular constitutional taint, producing loss of power in the whole system, such as scurvy or syphilis; or it may happen from some deficiency in the mode of treatment: the surgeon or patient may obviously not be assisting nature in a sufficient manner. The cause being ascertained, it will be easy, for the most part, to select the proper remedy. There are other cases, too, in which the cause of the non-occurrence of union cannot be ascertained until the parts themselves have been examined by dissection. Then it has been discovered; and it has been found out, too, that, had it been ascertained during life, or whilst the limb was on the body, an efficient remedy might have been applied. For instance, Mr. Fergusson is in the habit of referring, in his lectures, to a specimen of fractured leg now in King's College Museum, where non-union occurred, and where it was found on dissection that a portion of muscle lay between the two extremities of the bones. Here exposure of the part would probably have found out the cause and suggested the remedy. There are cases, moreover, where it is impossible to find out the cause of non-union of a fracture, either during life, or after examination of the fractured bone has been made after the limb has been removed or the patient has died. The patient may have been properly treated, and all the functions of the body may have been carried on in a healthy and natural manner; yet union will not take place.

We have to relate an interesting case of this nature which has of late been under the care of Mr. Fergusson in this hospital. The patient was a middle-aged man, who had led a sea-faring life: he was excessively corpulent, weighing about sixteen stone, and a bad subject for any injury or disease. In the month of July, 1849, he was thrown out of a waggon in Nova Scotia, and sustained a simple fracture of the upper part of the right thigh-bone. The limb was set, and he was kept in the military hospital for four months. It appears, however, that not very much attention was given to keeping the limb in proper position. A long splint was applied, and the leg was wrapped in brown paper, and this apparatus was fastened by pieces of tape. This was kept on for seven weeks, when he was allowed to get out of bed; but the bone had not united. He was told that it would get sound in time. He was afterwards brought to England, and transferred to Hasler Hospital, and remained there until October, 1849; but no benefit had accrued to him, although proper treatment had been used; and he was then admitted into King's College Hospital at the end of October, when it was found that there was

not the least attempt at union of the broken bone, although the health of the patient was excellent, and there appeared to be no constitutional taint about him which might act as a cause for the natural process not going on.

Mr. Fergusson determined to put in force some local treatment; and, therefore, in the beginning of November he made several subcutaneous punctures with a long narrow knife, and freely scraped the ends of the bones, as recommended by Professor Miller, of Edinburgh. The limb was then firmly put up with a splint and bandages, and was not disturbed for many weeks, when it appeared that some inflammation had been excited, and that some consolidation of the bones had taken place. Mr. Fergusson again freely scraped the ends of the bones, and ordered the limb to be put up as before. In February it was examined, and callus appeared to have been thrown out, and the limb seemed considerably stronger. More punctures were made, and a dextrine bandage was afterwards applied to exert considerable pressure on the part, and keep the limb very steady. A liberal amount of diet was also ordered. He remained in the hospital until April. He was allowed to sit up, and even to walk about with crutches, his limb being firmly encased in splints; but at this period he got a feverish attack, and his health appeared to suffer from confinement to the hospital; so he was allowed to go out. The limb was examined before he went; but it was discovered that no actual union of the bones had taken place, and Mr. Fergusson determined, with the patient's consent, to adopt more vigorous measures, for the sake of getting union of the bones, when his health had become quite restored.

He returned on the 3d of October last, with his fractured limb as much disunited as ever, but his health in good condition. As all the usual measures had now been tried, it was very evident that it was only by a local operation of a severe nature, or by an amputation of the thigh high up, that any chance of benefit could accrue to the patient. Mr. Fergusson proposed to him the operation of excision of the ends of the bones, to which a ready assent was given, the patient being a most intelligent man, and being well acquainted with the nature of the operation and the dangers he would run: accordingly, on the 6th, this severe and truly difficult operation was performed in the following manner:—

An incision, somewhat curved and carried obliquely across the thigh, was made over the seat of fracture. The soft tissues were dissected up, and the upper fragment of bone, which had been tilted much for-

wards by the action of the powerful muscles attached to it, was exposed; the soft parts were separated from its circumference, and a considerable portion of it was removed by the common saw. An attempt was now made to get at the lower fragment; but this was so much depressed, and drawn inwards under the superior portion, and the thigh was so excessively fleshy, and the femoral vessels must have been so close to it, that it was found to be impossible to get at it with the saw. Portions of its extremity were, however, removed by the cutting pliers, and its surfaces were freely scraped. A considerable bleeding took place during the operation, and it was found necessary to apply several ligatures. The patient was carried to bed, the wound was dressed, and endeavours were made to keep the limb as straight as possible by means of a well-applied splint and bandages.

The patient, although a very bad subject for any operative proceeding, from his great corpulence, did not suffer so much as was to be expected afterwards, and matters went on very well. There was no great shock, no great amount of febrile disturbance, and the local inflammation was not at all severe. The discharge was moderate, and in a few days the wound presented a healthy granulating appearance, and all danger from the effects of the operation was considered to have passed by. Three weeks after operation, however, he had a rigor, which was followed by febrile symptoms, at first of a moderate nature; but they rapidly assumed a grave appearance. The attack commenced on the 25th of October. The ordinary treatment was put in force, and on the 27th he seemed better; but next day the depression was great, the tongue became dry and brown, and a jaundiced appearance of the face—that almost certain indication of approaching death after an operation, or receipt of a severe injury—showed itself. The poor man rapidly became worse, and sunk on the next day, the 29th.

A post-mortem examination was performed the same afternoon, and it was confidently expected that either some purulent deposits within the organs of the body, or some severe inflammation about the wound, would be found; but all these parts were healthy. The wound had nearly healed up, and there was nothing wrong with the veins of the thigh. On examining the ends of the fractured bones it was discovered that they were still much out of place: the upper fragment was tilted forwards, and the lower portion was drawn inwards an inch above the other, and, as it were, imbedded in some of the

fibres of the vastus internus muscle. The surface of the upper fragment was coated with lymph, and, on the surface of the lower, a mass of new bone had been thrown out, sufficient to show that, in all probability, had the patient survived, union would have ultimately taken place. The superficial femoral vessels lay very close alongside of the lower fragment.

Mr. Fergusson, in the course of some remarks which he made in reference to this case, called the attention of the pupils to those points connected with it which were most striking. He told them that, although a case of non-union after fracture was a rare event, nevertheless it behoved them to observe these cases with attention when an opportunity of seeing them occurred; for in no other instance connected with surgery was there likely to be so much difficulty and anxiety, both with reference to the mode of treatment to be pursued, and to the result as well. And, first, as regards the cause of non-union: in this instance, it was impossible to say correctly what it was: there was an enormous development of fat in the body, and some might say that the non-union of the fracture depended upon this tendency of the system to generate fatty tissue, rather than the other more normal structures. With respect to the treatment which he had pursued here, they had seen that patient attempts had been made to bring about union before he had resorted to the last and most severe measure of cutting down upon the ends of the bones; for, finding that at an early period ordinary means had failed, he put in force that mode of treating ununited fractures which had first been recommended strongly by his friend, Professor James Miller, of Edinburgh. It consists of this: in inserting a long and narrow knife over the seat of fracture, and in scraping freely with its point the surfaces of the fractured bones: by this measure an amount of irritation and inflammation is expected to arise which will end in the throwing out of sufficient bone to consolidate the parts. Well, this, as they saw, had been very freely tried here; and great attention had been paid afterwards so as to ensure success; but after the lapse of months the case was as bad as ever. It now became evident that the patient must either lose his limb by amputation, or that some much more severe measure than had already been resorted to must be tried. With the patient's ready concurrence, then, he determined to give him a chance by cutting down upon the ends of the bones. He felt that this was a dangerous and uncertain remedy; but, still he did not consider that he was justified in resorting to amputation of the entire mem-

ber, which in this case would have been equally, or more dangerous, than the excision. The patient had gone on so well for three weeks, that they had reason to expect that matters would go well; but he was, unfortunately, suddenly seized with fever, and carried off. He did not, however, assert that if the operation had not been done he would have still been alive; but yet it could by no means be truly asserted that the operation had killed him. There was no evidence whatever by the post-mortem examination to show this. He wished particularly to call attention to one point connected with the operation, now more fully explained, as they saw the parts displayed. They must have noticed, during the proceeding, the immense difficulty he had in getting at the lower fragment of bone;—in fact, he was unable to remove a portion with the saw, but was obliged to resort contentedly with slipping off pieces by the pliers: as to the upper fragment this had been reached with facility. On looking at the parts now, the lower fragment is drawn considerably upwards and inwards, away from, and, as it were, behind the other. Moreover, close upon it ran the femoral artery and vessels; and this showed how cautious they must ever be in using cutting instruments in such cases. And the knowledge of this proximity of the vessels was that which rendered him so cautious whilst employing the long narrow knife in scraping the end of the lower fragment. It would have been very easy to wound the vessel without such caution. A mass of bone had been thrown out already; and this was enough to show that, in all probability, if the patient had lived, complete union would have taken place.

Hernia.

On the same day the pupils had the opportunity of observing the post-mortem appearances of a case of hernia which had been operated upon by Mr. Ferguson two days previously, and which presented features of very great interest. The case was this:—An old man was brought into the house in the afternoon with strangulated hernia. It appears that he had carried a rupture for several years, but that he had been able to reduce it, when it came down, with comparative ease. At eight, in the morning of the day of admission strangulation took place, and he was unable to get it back. Surgical assistance was immediately obtained; but it was found impossible to reduce the hernia, and all the bad symptoms setting in, the patient was sent to the hospital. The usual means were tried, but the house-surgeon was unable to relieve the strangulation, and therefore sent for Mr. Ferguson, who also tried, in

vain; and the symptoms continuing urgent, an operation was determined on, and put in force at 11 at night, fifteen hours after the hernia was first strangulated. It was a large hernia protruding through the inguinal canal into the scrotum on the right side, and evidently contained a large portion of intestine. Mr. Ferguson made an incision, about three inches in length, over the upper part of the tumor, and by this means divided the integument; the layers of subcutaneous cellular tissue were now divided by touches of the knife, and the cross fibres of the intercolumnar fascia were exposed; a director was placed under them, an incision was made directly upwards, and the structure was at once relieved, and the intestine was passed up without the sac being opened. The sac was large, and the opening into it from the abdomen so capacious, that directly the intestine, which was in large quantity, was reduced, a descent took place: with the object, therefore, of retaining it more securely, and of producing such an amount of irritation as might lead to an obliteration of the sac, Mr. Ferguson pinned up a portion of it, near its neck, passed two threads, and tied them around it: a firm compress was applied.

In the night the house-surgeon was called up to this patient, as bleeding was going on profusely. On removing the dressings it was found that a large quantity of blood had been lost; pressure, however, soon suppressed it. Next day the man was depressed; vomiting had still gone on after the operation; there had been no relief to the bowels, and there was considerable pain in the abdomen. Appropriate remedies were ordered, but he sank on the following night.

There were two interesting matters to be determined by the post-mortem examination. Was the bowel still strangulated, the sac not having been open? And where did the profuse bleeding, which occurred soon after the operation, come from? On dissecting off the skin and cellular tissue from the sac the latter question was at once solved; for a large quantity of blood was infiltrated under these tissues, it evidently having come from some superficial vessel which had been wounded during the operation, and not from the epigastric, as was thought possible. On laying open the abdomen, a large portion of the small intestine was lying in an intensely inflamed condition, the greater part of it being of a deep chocolate colour; and here and there it was in a gangrenous condition. A considerable loop of intestine was still in the sac, but was not at all constricted. The abdomen contained a quantity of bloody serum, the original colour

...of the sac, which had been returned by operation.

Here, then, is a case which must be exceedingly interesting to those surgeons who contend about the advantages and disadvantages of opening or not opening the sac in an operation for hernia. Here is a case which presented every feature favourable to an operation without laying the sac open. The hernia was large, the ring capacious, and—what most readily induced Mr. Ferguson to leave the peritoneum untouched—strangulation had not taken place more than fifteen hours; consequently it was hoped that those morbid changes in the strangulated intestine, which offer themselves as strong objections to the operation of not opening the sac, had not yet taken place. But what are the facts? The operation is quickly and easily done, not a single symptom is relieved, and, after death, a large portion of intestine is found to be in the most acutely inflamed and gangrenous condition; and, as the patient died so soon after the operative process, it is highly probable that these morbid phenomena had occurred within the period at which strangulation first took place—namely, at 8 in the morning, and the hour the stricture was relieved, at 11 at night. Mr. Ferguson pointed this fact out strongly to the pupils, and stated that this was one of those cases which must have ended fatally, from the mere intensity of the inflammatory process before the operation. He moreover referred to the strong argument here adduced against the safety and superior efficacy of operating without opening the sac; it well showed how difficult it is to tell in what condition the gut was, although a short time had only elapsed since strangulation.

ROYAL COLLEGE OF SURGEONS.

THE following gentlemen having undergone the necessary examinations for the diploma, were admitted members of the College at the meeting of the Court of Examiners, on the 8th inst.:—John Sykes, Mile End-road; Charles Clarke M'Mullan, Belfast; Richard Heath, London; Paterson Allen, Waltham Abbey, Essex; Anthony Taylor Preston, Manchester; Thomas Aspinall, Over-Darwen, Lancashire; Arthur Michael Button, Bury St. Edmunds, Suffolk; Trevor Morris, Chepstow, Monmouthshire; Joseph Williams, Portloe, Cornwall; and Samuel Sles Larcombe, Langport, Somerset. At the same meeting of the Court, Messrs. John Ward, and Charles B. passed their examinations for surgeons; these gentlemen had previously been admitted members of the College, and had received their diplomas bearing date May 8th, 1846, and March 1st, 1847.

...in this case would have been the same as in the case of the other two, and so well for the purpose of the present communication.

Correspondence.

Boxes of notes had been sent to the Editor of the Medical Gazette, and had been forwarded to the Editor of the Medical Gazette.

LETTER FROM MR. FRANK.
SIR, I am well, at the present season, at least not being in opposition, and I think, neither uninteresting, nor unprofitable, to place before your junior readers, a few familiar, and practical observations on the pursuit of medical study penned by my lamented friend and preceptor, Mr. Morgan. They are valuable, as they emanated from a gifted mind, which had long the wide field of Guy's Hospital, from which to draw a harvest of professional wisdom. Possibly this communication may be hailed with pleasure by some among your elder readers also, as recalling to their memories the teaching of a man whom few knew but to regard with great respect, and the expression of my regret that it is in my power to send the subjoined fragment only, and not the whole of your esteemed servant's paper on the subject of the Eye Infirmary, and the Lectures on Ophthalmic Surgery, at Guy's Hospital.
41, Finsbury Square,
Oct. 1850.

ADVICE TO MEDICAL STUDENTS IN THE PURSUIT OF THEIR PROFESSIONAL STUDIES. BY THE LATE JOHN MORGAN, M.D., F.R.S., SURGEON OF GUY'S HOSPITAL, &c.

A knowledge of pathology, gentlemen, is of the deepest importance to the scientific surgeon, and demands his most zealous endeavours for its attainment. This is to be gained principally in the wards of our hospital and at the bed-side of our patients; but, gentlemen, you must spare work for yourselves.

I shall endeavour in my lectures, and visits to patients in the wards, to give every assistance and instruction in my power. And, of course, you must endeavour, by consulting the writings of others, to learn what those who have gone before you have done in the prosecution of this study. But recollect that, although without lectures and reading it would be next to impossible for you to obtain a perfect knowledge of your profession, yet these must necessarily have reference to the elementary instruction you require. They will always teach you grammar and the construction of the language; but to be able to write it well, it well, and to speak it well, you must think for yourselves. You must, then, perfectly acquire the different branches of the

which the body is subject, you must see those diseases; and your observation must not be a cursory one. You must endeavour to trace the cause of the disease, to ascertain the time at which it began, the nature of its progress, and its gradual or sudden effect, not only upon the part, but upon the functions of the system generally. Should all known remedies fail, and the case terminate fatally, you will then endeavour to investigate the nature of those organic changes which have been the cause of death. This ought never to be neglected. It is by the study of morbid anatomy that we have been made acquainted with the true nature of many of the most important diseases to which we are liable, and been enabled to administer appropriate remedies.

And here anatomy and physiology come powerfully to our assistance; for, in the first place, who that has not by repeated dissection known the appearance of the perfectly healthy body in all its parts after death, could possibly detect apparently trifling indications of disease? Yet these claim our most serious attention when, by the further aid of physiology, we are enabled to trace the symptoms in the living body to sympathy with such local source of irritation.

After, then, you have made yourselves good general anatomists, let the study of morbid anatomy engage a large share of your attention; for without it, rest assured you can never attain excellence in the knowledge and practice of your profession.

In alluding to the paramount necessity for seeing disease in the living, as well as the morbid appearances which present themselves after death, it is right that I should give you one caution. It has reference more particularly to your conduct as hospital students; but it will be a useful one in after life. Be careful what you say respecting the nature and treatment of a patient's case, either in his presence, in the presence of his relations, friends, or attendants, or, indeed, under any circumstances which may make him or them acquainted with what it is quite unnecessary for them to know: even be careful of eaves-droppers. Now do not mistake me. The truth, and nothing but the truth, ought always to be spoken; but there is a time and a place for all things. It is not *always* necessary to speak the whole truth to patients. Deception is despicable; but, for the welfare of our patients, concealment is sometimes necessary; and the concealment to which I allude is neither criminal nor improper. It consists in avoiding the abuse of that wily member, the tongue.

I will illustrate and explain what I

mean. To the room set apart in this noble Institution for those who are anxious to become our patients, you will all have free access before these persons are seen by the medical officers. Now you may interchange thoughts and opinions in the hearing of those applicants for relief which are not only improper, but injurious to the interests both of them and of yourselves. Often and often has a patient been driven from the taking-in room of a hospital from having heard the tittle-tattle of medical students in reference to him. If the case be an urgent one, is not this injurious to the welfare of the listener?

But, whether urgent or not, we are liable to lose the opportunity of showing you the treatment of disease by the very great imprudence I am alluding to. Is not this, then, injurious to *your* interests also?

Now surgeons are sometimes equally in error with their pupils, and communicate to their patients what those patients have no business to know until they have found it out themselves. There can, for example, be no necessity for telling a subject for operation that you are going to give him pain: common sense will teach him this. Let him know when you are about to commence an operation, and encourage him to bear it firmly; but don't remind him of what he has to undergo; it can do no good, and may possibly unnerve a strength of mind and body wound up previously to the sticking-point. Mr. Cline had once an operation of some importance to perform; and, standing beside his patient on the operating table, commenced—"Now, my good man, I am going to give you a great deal of pain." What was the effect? Why the poor Irishman's resolution failed him at the unwelcome announcement; and, hastily replying "'Faith, but yer honour's mistaken!" he fled in consternation from the theatre, to the mingled chagrin and amusement of its occupants.

Be careful, then, in speaking before your patients: few have had occasion to repent of having said too little—many of having said too much.

Let me give you another piece of advice. Avoid the error which too many of your predecessors have fallen into, by giving their attention almost exclusively to what I may term *monstrous cases*. Those cases where the most difficult, dangerous, and desperate operations are required, certainly and properly form objects of irresistible attraction; but let them not throw into the back-ground other cases of every-day occurrence. For one case of important operation you will, when you get into practice, have fifty of sore leg, or other common disease; yet no case, however

apparently trifling, can be presented to your notice from which you may not gain useful and practical information.

Do not, therefore, turn away your attention from those cases which may now appear to you as trivial, and hardly deserving serious notice; for in future practice, depend upon it, you will regret having done so.

Do not, again, suppose that every sore leg which you see in the hospital is a local disease, requiring merely poultices or strapping. The constitutional treatment, even in these cases, is often more important than the local. There can, I repeat, be no example of disease, whether local or constitutional—whether slight or severe—from which you may not gain instruction; and recollect that you may have rich patients, as well as poor ones, with sore legs.

Let me now, gentlemen, direct attention to one part of your professional studies which comes more particularly within my own province as surgeon to this institution—I mean the study of ophthalmic surgery.

I cannot but believe that the value we all attach to the sense of sight must render it unnecessary for me to dwell upon the importance of those diseases to which the organ of vision is subjected. Do we not all know—do we not all feel, that amongst the various bodily calamities to which a human being is liable, the greatest, the most intolerable, is the loss of sight? And as the continued possession or total destruction of this inestimable blessing must in many cases depend upon the knowledge and experience of the surgeon, you can need no argument of mine to convince you of the necessity for a more than common attention to this particular part of your professional studies.

You have now, gentlemen, an ample opportunity of seeing the appearances and treatment of diseases of the eye in our Eye Infirmary. Let me entreat you not to neglect it.

I will dwell no longer on this subject; for it is well known that for many years I have been most zealous in my endeavours to vindicate the inseparable connection in practice, as well as in a pathological point of view, of ophthalmia with general surgery.

One piece of advice more, gentlemen, before I conclude:—

Beware of losing, even for a day, your present opportunities of acquiring professional knowledge, from the persuasion of friends enforced with one of the most insidious words in the English language—the word *only*. You will in this metropolis have many attractions to divert your thoughts and minds from study. Take care how you begin a course of dissipation;

for when you once begin, you will find it difficult to stop.

For instance:—There is, perhaps, an operation to be performed, or a lecture given, which you are tempted to neglect, just to see the queen going to open Parliament—*only*; or to hear an interesting debate in the House of Commons—*only*; or to see Kean in Hamlet—*only*. Yielding to such allurements, you will find at last that you have *only* a few months left to cram and grind in; and when you get into practice, you will be convinced, too late, that you have *only* yourselves into a professional nothing.

I am not one of those who wish to place a strict limit upon the enjoyment of all rational recreations and amusements: far from it. But I do say that a medical student who is to be seen in Covent Garden Theatre when he ought to be found in the theatre of his own medical school, is very very unwise, to say the least of it. No, gentlemen! business first; amusement afterwards.

In following your professional career, you will, like your predecessors, occasionally meet with difficulties and dangers in your path. But whatever may happen, be not discouraged. Pursue a steady, straightforward course, and your success is certain. Remember that the British surgeon should be distinguished by high character as a man, as a gentleman, and as a Christian. There is but one right and one wrong. Never deceive yourselves by parrying with conscience as to which way you are going; that conscience will tell you at once. To point out, however, and lead you along the single way which leads to excellence, you shall never, gentlemen, be assured, want our assistance so long as, to encounter every danger, and surmount every difficulty, we find you endeavouring to arm yourselves with science in your left hands, and religion in your right.

NEW REMEDY FOR SHORT-SIGHTEDNESS.

SIR,—I beg to call your attention to a newly-discovered remedy in cases of short-sightedness which I have applied with very considerable success.

A few months ago I observed that persons who are short-sighted, when looking at objects at a distance, partially close their eyelids, for the purpose of overcoming the difficulty they find in discerning them. This action is instinctive: it is a natural effort to adjust the eye to an increased sphere of vision. It is well known that short-sightedness depends not only on convexity of the cornea, but also on convexity of the lens; and, having no hope of being

enabled to effect any alteration in the structure of the lens, my attention was directed to the iris, which I found to be in such persons generally much dilated. It then occurred to me that contraction of the iris has the effect of apparently lengthening the convexity of the cornea, which approached a circumscribed plane that permits the rays of light to enter only in a straight line. The effect of this is obvious. The length of vision hereby necessarily becomes increased, and distant objects brought within its range. It therefore struck me that if we could discover any substance which could be so applied as to contract the iris, one cause of the defect of short-sightedness would be remedied.

The result, I am happy to say, has been most satisfactory. In the first instance I applied the extract of ginger, which was rubbed for five or ten minutes over the whole forehead, with the view of acting upon the branches of the fifth pair of nerves. Afterwards I substituted a concentrated tincture of ginger, of the strength of one part of ginger to two parts of spirit of wine decolorized by animal charcoal.

The success of this application was remarkable: in many cases it had the effect of doubling the length of vision. In some persons I found the iris was not much dilated, but very torpid. In these cases I applied the concentrated tincture of pepper, made of the same strength and in the same manner as the tincture of ginger. This I used until I observed that the iris had obtained a greater power of contraction and dilatation; after which I had again recourse to the tincture of ginger. This plan of treatment has been attended with the most signal success, and persons who were extremely short-sighted have very soon become enabled to lay permanently aside their concave glasses. The best method, I may observe, of testing the improvement of the sight during this treatment is, not by taking a printed book, and holding it near, and then at a greater distance from the eyes: this range of vision is much too limited: it is better to fix the attention of the patient upon a distant object, such as the brass key-hole of a door, and by his stepping some paces backwards, so as to place himself at a greater distance from it, he will soon discover the progress he is making.

So important a discovery as this will, I hope, be fairly tested by the members of our profession, who may rely on the success of the treatment I have recommended, if it be only judiciously and carefully carried out.

It is possible that the advantage derived from the tincture as above described may be ascribed to the alkaloidal principle,

piperis, which is held in solution in the tincture of pepper.

I remain, sir,

Your obedient servant,

A. TURBULL, M.D.

16, Manchester Square,
November 9, 1850.

Medical Intelligence.

TESTIMONIAL TO MR. J. F. CLARKE, VICE-PRESIDENT OF THE MEDICAL SOCIETY OF LONDON.

ON Thursday the 7th inst., a very numerous and highly influential meeting of the members of the profession, was held at the house of Dr. Webster, Brook-street, Grosvenor-square, with the view of promoting a subscription for the purpose of presenting a testimonial to Mr. J. F. Clarke, for the services he has rendered to the Medical Society of London, and the profession generally.

Dr. Webster, in stating the objects of the meeting, paid a high compliment to the useful literary labours of Mr. Clarke, and observed, that the assiduity and ability evinced by that gentleman during a rather long career entitled him to the thanks of the profession generally.

The first resolution, proposed by Dr. Chowne, and seconded by Mr. Coulson, was to the following effect:—

That Mr. Clarke, having by his literary exertions during many years, and by the faithful discharge of various important duties, including his valuable reports, materially contributed to extend the fame and usefulness of the London Medical Society, has established for himself strong claims upon the good opinion of the profession generally.

2. It was moved by Dr. Moore, and seconded by Mr. J. B. Brown,

That a subscription be now entered upon to present Mr. Clarke with a suitable testimonial in accordance with the previous resolution.

3. It was moved by Dr. T. Smith, and seconded by Mr. Robert Wade,

That Dr. Clutterbuck and Mr. Coulson be requested to act as treasurers.

It was moved by Dr. Winslow, and seconded by Mr. W. Harvey,

That the following gentlemen be appointed a Committee, with power to add to their number:—Dr. Golding Bird, Dr. Chowne, Dr. Daniell, Professor Erichsen, Professor Fergusson, Mr. Hancock, Mr. Harvey, Mr. Bird, Dr. Moore, Dr. Webster, Dr. Winslow, Mr. Wade, Mr. Stratton, Dr. Sibson,—to carry out the object pro-

posed, and to report at a future meeting of the subscribers—five to be a quorum.

Mr. Wing, and Mr. Borlase Childs, have consented to act as Honorary Secretaries. About one hundred pounds were subscribed in the room.

PROHIBITION OF THE SALE OF WOORARA POISON IN FRANCE.

THE French journals lately drew the attention of Government has been drawn by M. Bernard to the danger arising from the sale of *Woorara* poison. An order has been issued to prohibit its sale by including *Woorara* in the list of poisons which are not allowed to be sold except under certain legal restrictions.

DEATH OF M. BURRAUD-BROFFET, M.D.

THE last number of *D'Union Médicale* announces the death of this gentleman, who was for many years well-known in London as a writer on medical subjects, and the conductor of an Anglo-French periodical. About a year since he emigrated to San Francisco, in California, taking the long sea journey by Cape Horn. He died soon after his arrival in California, from an illness contracted during the voyage.

Selections from Journals.

ADVANTAGES AND DISADVANTAGES OF ETHER AS AN ANÆSTHETIC.

THE *advantages* of sulphuric ether are, according to Dr Hayward, "its entire safety, the ease with which it is administered, and the slight inconvenience which follows its administration. I have already stated that I have never known its inhalation followed by a fatal or alarming effect, and there is reason to doubt whether death has in a single instance been produced by it, when it has been properly administered. One patient is said to have lost his life by its inhalation at the hospital in Auxerre, in France. This took place in August, 1847. The details of the case are not given, with such minuteness as to enable any one to form a satisfactory opinion. It occurred, however, not long after the discovery, before the best mode of exhibiting it was adopted; and the *post-mortem* appearances indicated, as far as any opinion could be formed from them, that death was caused by asphyxia. In a careful examination of some of the leading medical journals of Europe and this country, published during the last fifteen years, I have not been able to find another case in which life was destroyed by the inhalation of sulphuric ether, and there is reason to believe, as I have already inti-

mated, that death would not have taken place in this instance if the lungs had been abundantly supplied with atmospheric air. It is only wonderful that an agent of such power, used as it often has been in the most reckless manner, by unskilful and ignorant persons, should not have caused far more disastrous results than any that have hitherto been made known. It teaches us that though it should be used with caution and confided only to skilful hands, the dangers from its use are far less than our preconceived opinions had led us to believe.

"There are no ill consequences from its use. If it be breathed only for a short time its effects usually pass off in a few minutes. I have never known them to continue for more than an hour; and in this case the patient had been kept under its influence for forty-five minutes. Nausea and vomiting are not frequent, unless it is inhaled soon after food has been taken. I have not seen convulsions follow its exhibition, nor any delirium, except a slight and transitory kind, such as arises from intoxicating liquors. I confess that I was much surprised to learn, by carefully watching its effects, to what a small extent and for how short a time it disturbed the functions of the nervous system, and how rare it was to find headache among the consequences of its inhalation.

"The only objections of which I am aware to sulphuric ether as an anæsthetic agent, are its pungent odour, which is offensive to some persons, and the no inconsiderable degree of irritation which its inhalation occasionally produces in the air passages. This irritation, I am confident, may be in great measure prevented by proper attention to the mode of its exhibition and the quality of the article used. Admitting these objections to be as great as they have been said to be by those who have urged them with the most earnestness, they do not in my opinion counterbalance the advantages, and I have no hesitation in saying that I should give it the preference over any other article with which I am acquainted, that is used for the purpose of producing insensibility."—*American Journal of the Medical Sciences*, July 1850.

POISONING WITH WINE THAT HAD CONTAINED LEAD-SHOT.

THE following is an additional example of the danger of cleaning wine-bottles with lead-shot:—

Eight of the pupils of the Jesuit School at Dole having partaken, with their Superior, of a bottle of wine, were suddenly seized with symptoms of severe colic, and in three hours (?) the Superior died. It was found that the bottle had contained several shot, which had been acted upon

by the acid of the wine.—*Journal de Chimie Médicale*, Octobre 1850.

X

TREATMENT OF CHOREA BY FRICTIONS WITH CHLOROFORM.

M. GASKEZ has published three cases of chorea cured by the topical application of chloroform.

The first was that of a child, seven years of age, in whom the disease was caused by fright. A liniment composed of equal parts of chloroform and oil of sweet almonds was rubbed, night and morning, along the course of the spine. From its first employment the violence of the muscular movements was moderated, and in six days the patient was cured.

The second case was that of a boy, twelve years of age, in whom the disease had appeared two months before as the effect of fright. The spasmodic movements were so violent that he could hold nothing in his hands, nor walk without help. At the end of a fortnight, under chloroform frictions, the disease had disappeared. A relapse, however, occurred, which was cured in two days by a return to the same means.

In a third case, of five months' standing, the result, also, of fright, the symptoms disappeared in seven days under chloroform frictions.—*L'Union Médicale*, Oct. 31, 1850.

X

BOOKS & PERIODICALS RECEIVED

DURING THE WEEK.

A Universal Formulary; containing the Methods of preparing and administering Official and other Medicines, &c. By R. Eglesfield Griffith, M.D. Philadelphia, 1850.

The Elements of Materia Medica and Therapeutics. By Jon. Pereira, M.D. F.R.S. Vol. 2, Part 1.

Influence of Physical Agents on the Development of the Tadpole of the Triton and Frog. By John Higginbottom, F.R.C.S. &c.

Address before the American Medical Association. By John C. Warren, M.D. U.S.

Guy's Hospital Reports. No. 7, October 1850.

Comptes Rendus. Nos. 16, 17, and 18—the 14th to the 26th October. Tables. 1er Semestre. Tom. 90.

Journal de Chimie Médicale. Novembre.

The Philosophy of Spirits in relation to Matter. By C. M. Burnett, M.D.

BIRTHS & DEATHS IN THE METROPOLIS

During the Week ending Saturday, Nov. 9.

BIRTHS.		DEATHS.	
Males....	881	Males....	461
Females..	736	Females..	470
1677		931	

CAUSES OF DEATH.

ALL CAUSES	931
Specified Causes	919
1. Zymotic (or Epidemic, Endemic, Contagious) Diseases....	306
<i>Spurious Diseases, viz.:</i>	
1. Dropsy, Cancer, &c.	27
2. Brain, Spinal Marrow, Nerves, and Senses	101
4. Heart and Bloodvessels.....	47
5. Lungs and organs of Respiration ..	171
6. Stomach, Liver, &c.	58
7. Diseases of the Kidneys, &c.	11
8. Childbirth, Diseases of Uterus, &c.	5
9. Rheumatism, Diseases of Bones, Joints, &c.	8
10. Skin.....	1
11. Premature Birth.....	20
12. Old Age.....	55
13. Sudden Deaths.....	5
14. Violence, Privation, Cold, &c....	17

The following is a selection of the numbers of Deaths from the most important special causes:

Small-pox.....	12	Convulsions.....	26
Measles.....	28	Bronchitis.....	70
Scarlatina.....	33	Pneumonia.....	77
Whooping-cough.....	23	Phthisis.....	93
Diarrhoea.....	16	Lungs.....	2
Cholera.....	1	Teething.....	4
Typhus.....	55	Stomach.....	9
Dropsy.....	15	Liver.....	8
Hydrocephalus.....	22	Childbirth.....	3
Apoplexy.....	21	Uterus.....	2
Paralysis.....	23		

REMARKS.—The total number of deaths was 42 below the average mortality of the 45th week of ten previous years.

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer.....	30.94
" " " Thermometer.....	50.2
Self-registering do.	Max. 59° Min. 28°
From 13 observations daily. 1 Sun.	

RAIN, in inches, .19.—Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was 7° above the mean of the month.

NOTICES TO CORRESPONDENTS.

Dr. Wells.—The paper will be published in the following number. A proof shall be forwarded.

Mr. Hunter's communication next week.

Dr. Snowman's letter will be inserted.

VERAX.—No person is entitled to practise as a General Practitioner in England and Wales (or act as an Apothecary) who has not the license of the Apothecaries' Society. An application should be made to the clerk of the Apothecaries' Society.

Comments.—In Dr. Mayo's paper, in our last number, at page 725, col. 1, line 24, for "life," read "bile."

Lectures.

COURSE OF LECTURES

ON

DISEASES OF THE HEART.

Delivered at St. Vincent's Hospital during the Session 1849-50.

By O'BRYEN BELLINGHAM, M.D.

Fellow of, and Member of the Court of Examiners of, the Royal College of Surgeons in Ireland, and one of the Medical Officers of the Hospital.

LECTURE XIV.

GENERAL SIGNS OF HEART DISEASE.

Indirect or secondary symptoms—Congestion—Causes of—Mode of production of—Organs in which congestion most readily occurs—Effects of congestion upon different organs—Congestion of the lungs—Edema of the pulmonary tissue.

Indirect or Secondary Symptoms of Cardiac Disease.

Congestion.—The secondary, the indirect, or the remote symptoms of cardiac disease, are, in the majority of cases, the result of some impediment to the return of the blood conveyed by the pulmonary veins to the left side of the heart, or to that of the venous blood from the system generally, through the right side of the organ, which in their turn are frequently the result of an obstruction to the free passage of the blood through the chambers of the heart. The blood, being retarded and delayed in the large veins, accumulates in the smaller vessels and capillaries; they become distended, dilated, and tortuous; their healthy state of elasticity or tone is impaired or diminished, and congestion is the result, which may be limited to the lungs and bronchial mucous membrane, or may extend to the liver, spleen, kidneys, and gastro-intestinal mucous membrane, or to the brain, causing more or less derangement of the function of these organs.

When we speak of congestion, therefore, we mean a state in which there is a preternatural accumulation of blood in the minute veins and capillaries, with a weakened, retarded circulation, and a loss or deficiency of tone in the coats of the dilated vessels; in consequence of which they "react imperfectly upon the blood transmitted to them," and are unable to "transmit the force of the current in the propulsion," "vessels which have lost th becoming inelastic and tortuous,

the very stagnancy of the blood in them, opposing an increasing obstacle to its passage through them."

The cause of the motion of the blood in the capillaries is now generally considered to lie in the impulse given to this fluid by the contraction of the ventricles. An experiment which was performed by Dr. Sharpey seems to confirm this. "A syringe, with a hæmadynamometer, to show the amount of pressure used, was adapted to the aorta of a recently dead animal, the vena cava being divided; warm water was then injected, and, with a force which raised the mercury in the hæmadynamometer only three inches, the water passed through the capillaries, and out of the vena cava. When the pressure was increased so as to raise the mercury six inches, the flow was very free; and on adapting another hæmadynamometer to the vein, the pressure in this was found to rise as high as three inches. The pressure thus used in the arteries (six inches of mercury) was not greater than the natural pressure in the arteries of a living animal; and the pressure transmitted to the veins (three inches of mercury) was greater than that in the veins of a living animal,—thus showing that the force of the heart sustained by arterial tension is quite adequate to effect the circulation without other aid."

Hence, when the contractile power of the left ventricle is weakened, owing to dilatation of its cavity, to attenuation of its parietes, or to softening of its tissue, the blood must necessarily pass with greater difficulty through the capillaries, and congestion will be liable to ensue; while, if the circulation through the left side of the heart is impeded, the capillaries of the lungs will become congested in the first instance, followed by general venous congestion and the train of symptoms presently to be described.

It was at one time a very general opinion that cerebral congestion, or apoplexy on the one hand, and hæmoptysis or pulmonary apoplexy on the other, had their cause, the former in hypertrophy of the left ventricle, the latter in hypertrophy of the right; their occurrence was, in fact, looked upon simply as an example of cause and effect, the increased force with which the thickened ventricle transmitted the blood to the brain or lungs being considered sufficient to produce all the signs of congestion of these organs, terminating in serous or sanguineous effusion, or in rupture of the coats of the small vessels.

This theory of the cause of congestion is supported by pathological as the lungs are concerned, be exploded, the congestion in the

result of cardiac disease being much more frequently the result of obstruction to the free passage of the blood through the chambers of the left side of the heart. Because, even though the propulsive power of the ventricles is increased, if there is no impediment to the return of the blood by the veins, the circulation will only be carried on more vigorously, the veins returning the blood as rapidly as it is transmitted to the arteries. Indeed, congestion is much more liable to occur when the force with which the blood is propelled by the ventricles is diminished, as in dilatation and attenuation of the ventricles, or in softening of the tissue of the heart, than when this fluid is propelled with increased force by an hypertrophied ventricle.

Congestion may likewise be the result of long-continued determination of blood to a part, and hence is not unfrequently the effect of a previous attack of inflammation, which, when it has lasted long, or been repeated, leaves behind a state of over-distension of the vessels, and of weakness of their coats, which renders the patient liable to a fresh attack of inflammation in the same part. This appears to be the reason, that a person who has previously been the subject of bronchitis, tonsillitis, or conjunctivitis, is more liable to a return of inflammation in the same part than in any other, after exposure to cold or any other cause capable of giving rise to it.

Over-distension of the minute vessels appears to be not only an effect of congestion, but a cause of the continuance of this state. "The blood-vessels, in their healthy condition," Dr. Williams* observes, "are so constituted as to make the most of the heart's propulsive power, and transfer it through their whole length; but when dilated, tortuous, flaccid, and otherwise altered, they misdirect and exhaust it; it is partly expended in distending and dilating the nearer portion; whilst a sufficiency does not remain for the onward propulsion of the blood, which, therefore, stagnates and accumulates in the congested blood-vessels." "When the circulation is feeble, and the tone of the vessels weak, these causes of congestion operate," he adds, "more readily and more permanently than when the circulation is vigorous; yet these congestive affections, the result of weakness, are often mistaken for inflammation. Many of the pains and ailments of delicate females are of this nature; and, although temporarily relieved by depletory measures, are to be permanently counteracted only by tonic means, which promote the vigour and equality of the circulation."

When congestion is carried to an ex-

treme degree, or has lasted for a considerable time, the overloaded and overdistended vessels relieve themselves by allowing the more watery parts of the blood to transude, and serous effusion occurs, or the blood itself may escape, or rupture of a vessel and extravasation of blood may occur.

Long-continued congestion frequently leads to more or less alteration of the tissue of the congested organ or part; its size is frequently increased; and this may or may not be accompanied by pain, or by tenderness on pressure. This is more frequently witnessed in the liver and spleen than in other organs; and, if the cause continues long in operation, the increase in size may be permanent. Dr. Clendinning* has shown that the lungs acquire increased development and weight under such circumstances. The functions of the organ the seat of the congestion almost always likewise suffer; and, in secreting organs, the congestion may occasion either diminution, suspension, alteration, or increase of the secretion.

In the great majority of the cases which we are considering congestion may be traced to one or other of the following causes—viz., 1st. To feebleness of the heart's action, the result of dilatation of the left ventricle, of attenuation of its parietes, or of softening of its muscular tissue, or to two of them combined; in consequence of which the contractile power of the left ventricle is diminished, and its systole is insufficient to propel the blood through the systemic capillaries; 2ndly. To free regurgitation through the tricuspid orifice, the result of dilatation of this opening, combined with hypertrophy and dilatation of the right ventricle; by which the return of the blood from the venæ cavae to the right auricle is greatly impeded, leading to its accumulation, first in the large venous trunks, and next in the smaller veins and capillaries; and 3dly. To a contracted state of the mitral orifice, impeding the passage of the blood from the left auricle into the left ventricle, or to a state of the mitral valve or orifice which permits free regurgitation through it; owing to which much of the contents of the left ventricle is transmitted backwards into the left auricle at each systole. In the first of these cases the congestion commences in the capillaries of the systemic circulation, and extends to the veins; in the second it commences in the large veins, and extends to the capillaries; in the third it commences in the pulmonary capillaries, and extends to the large veins.

For instance, when the left auriculo-ventricular orifice is much contracted,

* Principles of Medicine.

* Croonian Lectures: MED. GAL.

there will be a permanent impediment to the passage of the blood from the left auricle into the left ventricle; and, as all the blood in the system must pass through this orifice, if it is much diminished in size, the left auricle will become distended, and the return of the blood by the pulmonary veins will be impeded; congestion of the pulmonary tissue, and of the bronchial mucous membrane will follow, which will react upon the right auricle and ventricle; the latter will become dilated, and the former hypertrophied. The entrance of the blood conveyed by the *vena cava* to the right auricle being impeded, these vessels become distended. The hepatic veins and the portal circulation suffer next; congestion of the liver, spleen, kidneys, and gastro-intestinal mucous membrane follows, and, the free return of the blood by the jugular veins being impeded, congestion of the head, face, or of the brain, may ensue.

When the left auriculo-ventricular orifice is dilated, or when the mitral valve is diseased, and incapable of closing the orifice, and free regurgitation into the left auricle occurs at each systole of the ventricle, the same results will follow, but in a somewhat less marked degree than when extreme contraction of the orifice exists. According to Dr. Blakiston,* however, this is the most frequent cause of pulmonary congestion. "Of all the forms of valvular disease, incompleteness of the mitral valves was found," he observes, "to be the most constant and direct cause of pulmonary congestion." "Mitral regurgitation existed in almost every one of the cases where pulmonary congestion formed the leading symptom."

When the parietes of the left ventricle are thinned, and its cavity is dilated, or when the muscular tissue of the ventricle is softened, the weakened organ is unable to propel the blood with sufficient force to pass through the capillaries of the systemic circulation, and they become congested. Eventually, the left ventricle, owing to its inability to empty itself, becomes distended, followed by distension of the left auricle. The pulmonary veins have then a difficulty in returning their blood; and the same train of symptoms may follow as where the valves or orifices of the left side of the heart are diseased, and impede the circulation through it.

When the right auriculo-ventricular orifice is dilated, and much of the contents of the ventricle regurgitates into the auricle at each systole, particularly when the right ventricle is at the same time hypertrophied, the return of the blood by the *vena cava*

to the right auricle will be impeded, which will react upon the venous system throughout the body, and will be followed by congestion of the liver, spleen, and gastro-intestinal mucous membrane, as in the cases where the congestion commences in the pulmonary capillaries. According to Dr. Blakiston, this is the most active cause of general venous congestion. "When regurgitation at this orifice takes place to any extent, it is impossible to conceive," he says, "a more powerful obstruction than is thus offered to the passage of the blood from the veins into the heart by a strong counter-current forced backwards by the systole of the right auricle." Dr. Blakiston seems likewise to be of opinion that regurgitation through the tricuspid orifice, as a cause of congestion, had been overlooked until he called attention to it. In this I cannot agree with him: indeed, so far as I have had the opportunity of observing, dilatation of the tricuspid orifice, with hypertrophy and dilatation of the right ventricle, scarcely ever occurs, except as secondary to valvular disease, at the left side of the heart.

The organs in which congestion the result of cardiac disease most readily occurs are, as a general rule, those which are most vascular—viz., the lungs and bronchial mucous membrane, the liver, spleen, kidneys, and gastro-intestinal mucous membrane, and the brain. Indeed, the secondary or indirect symptoms of cardiac disease depend essentially upon the amount and degree of congestion which it occasions, and they vary according to the organs particularly engaged.

In the slighter forms of congestion, the functions of the organ the seat of the congestion may be little affected: in the advanced stage its functions are frequently seriously impaired. Thus in congestion of the lungs and bronchial mucous membrane we have dyspnoea, passing into orthopnoea, with oppression, cough, and expectoration, which may or may not be followed by hæmoptysis or pulmonary apoplexy; the secretion from the bronchial mucous membrane is often remarkably increased, and this may prove one of the most troublesome symptoms. Congestion of the liver is accompanied by an increase in size of the organ, and by impeded or vitiated secretion, or by arrest of the biliary secretion, frequently with pain or tenderness on pressure. When the gastric mucous membrane is congested the disturbance of function may be indicated by increased or altered secretion, or we may have nausea, or antiperistaltic action of the organ, or pain may be the most prominent symptom, or loss of appetite may be alone complained of. When the spleen is congested the organ sometimes attains a remarkable size:

* Practical Observations on Diseases of the Chest.

this, however, is more frequently observed as an effect of acute than of cardiac disease. When the liver and gastro-intestinal mucous membrane are both congested, we may have pain, deranged digestion, loss of appetite, flatulent distension, vomiting, hepatic derangement, hæmatemesis, diarrhoea, hæmorrhoids, jaundice, or ascites. When the brain is congested its sensibility is sometimes impaired, sometimes increased, and we may have headache, tinnitus aurium, vertigo, sense of weight in the head, confused vision, or disordered volition, or a semi-comatose state, which, in extreme cases, may terminate fatally, with or without the rupture of any vessel. Finally, when the kidneys, and the venous system generally, are congested, we have œdema, slight at first, and commencing in the most dependent parts, but eventually becoming general.

Congestion of the Lungs.

The lungs, from their physiological position between the right and left chambers of the heart, must suffer whenever the circulation through the heart is not free, or when disease to any extent exists in the valves or orifices of the left side of the organ. Thus, when the mitral valve is diseased, and the orifice is contracted, the left auricle, being unable to empty itself, becomes distended, and the blood returning by the pulmonary veins cannot freely enter the auricle; the right ventricle at the same time continuing to transmit the blood by the pulmonary artery, the capillaries of the lungs become overloaded, and the pulmonary tissue and the bronchial mucous membrane congested.

When the mitral valve imperfectly fulfils its functions, and much of the contents of the ventricle is transmitted backwards into the left auricle at each systole of the left ventricle, the left auricle will remain distended, the pulmonary veins cannot empty themselves, and the same results follow as where the mitral orifice is contracted.

When the aortic valves permit free regurgitation into the left ventricle, this reacts upon the left auricle, and the effects are propagated backwards to the lungs, but in a much less injurious degree than when the mitral orifice is much contracted. When the aortic orifice is diseased, so as to obstruct the orifice considerably, the same results sometimes follow as where considerable contraction of the mitral orifice exists. "It appears (Dr. Blakiston observes) that a considerable amount of disease of the aortic orifice may exist for a number of years without seriously affecting the general health, more particularly if hypertrophy of the left ventricle is joined with it; and that, consequently, this affection has little or no direct influence in re-

tarding the venous circulation, and producing engorgement of the vessels of the pulmonary or general circulation."

The congestion of the lungs and bronchial mucous membrane which occurs under such circumstances is always of the passive form, and is always a secondary or consecutive lesion. When it has lasted long, or when the patient has suffered frequent attacks of it, a condition of the part remains in which inflammation, or a state approaching to it, is very liable to be set up, and sometimes by causes which would be incapable of giving rise to it in a healthy condition of the parts: hence we not unfrequently observe in addition a real determination of blood to the lungs, the result of inflammation of the parenchyma of these organs.

The important function which the lungs perform in the conversion of venous into arterial blood must obviously be seriously interfered with when the circulation through the pulmonary capillaries is retarded and impeded, and the minute vessels are considerably engorged. The capillary vessels of the lungs form, we know, a net-work around each vesicle or air-cell: they are exceedingly numerous, and in greater abundance in a given space here than in almost any other part of the body. Now as the blood must all pass through them before it arrives at the commencement of the systemic circulation, it is obvious that any impediment to its passage will be felt throughout the system; while, if the blood is prevented from undergoing the change from venous to arterial in the pulmonary capillaries, the pulmonary veins will convey venous as well as arterial blood to the left auricle. Hence, when the congestion is considerable in degree, and engages much of the pulmonary tissue, and the air-cells of the part scarcely permit of the entrance of atmospheric air during inspiration, the blood will be imperfectly oxygenated, its carbonic acid will be incompletely removed, and the renewal of the fibrine will be imperfectly accomplished; a mixture of venous and arterial blood will then circulate in the arteries, which is unfit for maintaining the functions of the various organs in a healthy condition, or for supplying nutriment to their tissue; the brain, the nervous system, the heart, and the other organs, the maintenance of the function of which depends upon a due supply of arterial blood, suffer; the muscular system receives less fibrine, the animal heat diminishes, and the entire system feels the depressing influence.

When the pulmonary capillaries have been congested for a long time, or to a considerable degree, the overloaded vessels are relieved by the transudation of the watery, saline, albuminous, and sometimes

of the fibrous portions of the blood; or the blood itself may be effused upon the surface of the bronchial mucous membrane, or into the air cells, or rupture of a minute vessel may take place, with extravasation of blood upon the mucous surface, or into the pulmonary tissue; accompanied by œdema of the pulmonary tissue, dyspnoea, which in extreme cases amounts to orthopnoea, cough, and watery expectoration; or hæmoptysis, and sometimes pulmonary apoplexy.

Long-continued congestion not only interferes with the functions of the congested organ, but it also frequently affects its nutrition and structure. "It generally tends," as Dr. Williams observes, "to cause an increased deposit in them, constituting a variety of hypertrophy." "In the lungs, long-continued congestion may," he remarks, "cause hypertrophy of the inter-vesicular and interlobular textures, and in some cases partial consolidation of the vesicular structure itself." "Such changes," he adds, "are frequently met with in connexion with long-standing disease of the heart, and abound most in the posterior parts of the lungs, and near their roots, the most vascular parts."

Œdema of the Lungs.

When the congested capillaries of the lungs have relieved themselves by allowing the serous or thinnest portion of the blood to transude, and when the air-cells, the minute ramifications of the bronchial tubes, and the interlobular cellular tissue, have become infiltrated with this fluid, it constitutes the state known under the name of œdema of the lungs; and, as in œdema of the extremities, the fluid gravitates to the most depending parts, and will be found to occupy the base of the lungs.

Œdema of the pulmonary tissue is necessarily, therefore, a symptom only of the advanced stages of cardiac disease, and the forms of disease with which it is usually associated are those in which a considerable impediment exists to the free passage of the blood through the left chambers of the heart, particularly valvular disease. It is more common in the obstructive lesion of the mitral orifice than in any other; indeed, few cases of this form of disease go through their stages, without this condition being present in a greater or less degree, and it is usually preceded by general anasarca of the cellular tissue.

Œdema of the lungs, although no unfrequent effect of cardiac disease, is by no means limited to diseased states of the heart, but is met with in several other morbid conditions; thus it may accompany general dropsy; it is observed in chronic bronchitis, and pertussis; it follows the resolution of

pneumonia, and it is met with as a sequel of measles and scarlatina.

This condition of the pulmonary tissue is always accompanied by more or less dyspnoea, by cough and expectoration, frequently of a thin mucous or serous fluid, as well as by certain physical signs,—the most remarkable of which are dullness on percussion, and a moist subcrepitant rûle on auscultation of the base of the lungs,—but upon which it is not necessary to delay here.

[To be continued.]

THE PERIOD AT WHICH IODINE APPEARS IN THE URINE.

DR. DALTON states that he observed the appearance of iodine in the urine in seven cases, and in many of them it was detected at the same time in the saliva. In order to determine the time which is necessary for iodine to appear in the urine, the following experiments were instituted. On the 21st of February, at five P.M., I took half a drachm of the syrup of the iodide of iron, and examined the urine afterwards, at intervals of ten minutes. The portions passed at ten and twenty minutes past five showed no indication of iodine; but at half-past five a strong purple colour was produced by the addition of starch and nitric acid. This reaction continued throughout the evening, and urine passed at seven the next morning struck a deep indigo with the same reagents. At nine A.M. the colour was less marked; and from this time it continued to grow fainter and fainter, till forty minutes past four P.M., nearly twenty-four hours from the time of taking the medicine, after which no further reaction was perceptible.

The next observation was made with regard to the accumulation of iodine in the system, and the time necessary for eliminating a large quantity. Two patients in the Massachusetts General Hospital had been taking iodide of potassium, one for six, the other for eight weeks. During the greater part of this time, they both took the medicine to the amount of one drachm per day. On the 24th of February, the medicine was omitted for each. The urine of both patients at this time gave a nearly black colour with starch and nitric acid. It was then examined every twelve hours, soon after being passed. In seventy-two hours the colour produced was very faint, so that in one case it was doubtful whether it existed or not; and in the other it was distinct on first dropping in the nitric acid, but disappeared by agitation. In twelve hours more, all trace of iodine had disappeared from both specimens.—*American Journal of Medical Sciences.*

Original Communications.

ON THE
TREATMENT OF OBSTRUCTION OF
THE BOWELS.BY EDWARD WELLS, M.D., OXON.,
F.R.C.P.*(Read before the Reading Pathological
Society.)*

In venturing to make the following brief remarks upon the treatment of obstruction of the bowels, I do so principally with a view of causing a discussion upon this subject, and of not only recording my own experience, but also of eliciting that of others.

It cannot, I think, be denied, that of late years the treatment of diseases in general has taken a right direction, and made a rapid improvement. Both in the domain of medicine and of surgery the preservation of life and limb has largely ensued from the adoption of a conservative policy. Putting aside the increased amount of good we are enabled to effect at the present day, it is no small proof of our progress in therapeutics that we do much less injury to our patients than was done by our predecessors.

This improvement in practice is perhaps evidenced in no disease more strongly than in that of obstruction of the bowels. Instead of the old custom of reiterating cathartics upon cathartics, of hunting the Pharmacopœia for purgatives still stronger than those already administered, of pouring in quicksilver as if the intestinal tube had a continuous descent, the practitioner of the present day wisely attempts to soothe rather than to excite the already irritated intestine. He knows that the mere inaction of the bowels is of itself of very secondary importance, that there are cases, in which individuals pass weeks and weeks without having a motion, and feel none the worse, and he is rather anxious to remedy the cause, knowing that when this is removed the effect will follow.

In considering on the present occasion the treatment of obstruction, I do not intend to refer to those cases which arise on the one hand from causes acting

externally to the intestinal tube, such as tumors, either malignant or otherwise, which by pressure mechanically occlude the channel, or, on the other, to those which arise from causes acting *internally* to the intestinal tube, such as hardened feces or calculi, such as originate from the use of magnesia. Neither do I refer to those cases which depend upon the strangulation of a hernia. These cases each require their peculiar mode of treatment, and the last-mentioned belongs to the province of surgery.

The cases which I have in view at present are those in which there is no demonstrable cause of the obstruction, and in which the point of obstruction is situated somewhere in the small intestines.

I will now suppose an instance of such a case:—

You are called to a patient, who informs you that he has had no proper relief from the bowels for the last seven or eight days, that he has been to the druggist, and taken black dose upon black dose, pill upon pill, and that they are all in him, and he wants to know what he is to do next. He tells you further that it is true he has been to stool once or twice, or perhaps even oftener during the time, that he has perhaps on each occasion passed something, but he is sure it is not what he ought to have passed. In short, to use his own expression, although he has occasionally had a scanty evacuation, he is convinced that "*nothing has gone through him.*" Upon examining the abdomen, you find some distension around the umbilicus, with a degree of tenderness on pressure. This last symptom varies from that slight shade in which the patient can hardly say whether the pressure relieves his pain or not, up to decided tenderness on the least touch. In mild cases the patient will tell you he feels very well, excepting the obstruction, but the knowledge of its existence makes him very uncomfortable. In other cases there is some degree of sickness conjoined, merely perhaps occasioned by the purgative draughts. In severer cases the sickness is more permanent, mucus or bile being rejected from the stomach. In such instances we should expect the tenderness on pressure over the bowels to be greater, though still not in any degree approaching to what usually

occurs in peritonitis. There will also be a rumbling of flatus in the intestines, and the patient will say he feels the wind pass downwards to a certain point and then stop. All this time the pulse is not perhaps accelerated, it is generally weak; the tongue is moist and often clean; the urine, provided the obstruction be not situated high up in the bowels, is not necessarily affected, though generally high coloured.

Under these circumstances, and especially in the milder cases, the first thing perhaps that you do is to order a large enema to be thrown up. It is found to traverse the large intestine easily; the patient assures you that he feels it go as far as the ilio-cæcal valve, and after a short time it returns without any tinge of fecal matter. The obstruction is not in any part of the colon, but somewhere in the small intestine.

What treatment should, then, be adopted? In the severer cases, where there is pain upon pressure, distension of a portion of the intestine, a rumbling of flatus, and frequent vomiting, it will be said that the line of treatment is easily chalked out; that, whatever the cause of the obstruction, we have inflammation superadded; and that our treatment must be directed to subdue the latter. This is quite true; and in such well-marked cases I do not think there would be much chance of the case being misunderstood. But we must remember that these severe instances of the disease are only the consequence of a continuation and aggravation of the symptoms of its milder forms. We must not forget that the most simple case of obstruction is liable to run on into a fatal form, if, with the view of obtaining an action of the bowels, we are incautious in the prolonged use of irritating medicines. Finding that the patient's chief discomfort arises from the fact of the bowels not acting, that he professes himself as feeling otherwise well, we are, perhaps, rather too liable to fall in with his own fancies, and just give him one more dose.

Now, in these cases what ought we to do? In the first place, abstain entirely from all purgative medicines. It will be much better to err in not giving sufficient aperients, than to err in giving too much. The first thing to do is to compose the patient's mind by informing him that there is no hurry

for the bowels to act; that if he waits patiently, they will be sure to act in time; to tell him instances of persons who have gone a long time without any action of the bowels, and have done well.

Next, in these cases of obstinate obstruction I have great faith in the lancet, where it can with safety be used. It has seemed that a slight degree of faintness produced by blood-letting has acted very beneficially in removing the exciting causes of the obstruction, probably by the general relaxation which the faintness itself occasions. By putting the patient in an upright position, and bleeding him until he begins to feel slightly faint, I think we are quite safe not to do him any harm. If he is of a weak, nervous temperament, a very few ounces will produce the desired effect. If he be strong, he will afford to lose more. Where, however, the debility of the patient forbids the use of the lancet, it will be as well to apply leeches around the umbilicus. These act, probably, by relieving the local congestion, which is either the cause or the effect of the obstruction.

These measures premised, the safest plan is, I think, to put the patient upon repeated doses of calomel and opium. Even if inflammation be totally absent, the exhibition of these two drugs is likely to be attended with the best effects. The opium soothes the bowels already irritated by the repeated cathartics: it allays the over-excited peristaltic action; it relaxes any contingent spasm, and quiets the patient's mind. To effect these objects, it must be administered in sufficient doses—such as gr. ʒ to gr. j. every four hours. The calomel, by improving the secretions, and exciting the action of the liver, tends to remove the cause of the obstruction. And if this happen to depend upon a partial enteritis, the combined action of these two medicines would hold out the best hopes of a successful treatment. If the calomel be sufficiently guarded by opium, there is not, I think, any fear of its producing any serious irritation of the bowels.

While using these remedies I should be in no hurry to accelerate the action of the bowels by aperients. I should rather wait until they begin to act of themselves, as they generally will; and then, provided no inflammatory symp-

toes were present, there would be no objection to administer a dose of castor-oil to aid their propulsive efforts. In these cases it is also better to delay the administration of aperient enemata until the bowels are acting of themselves. Previously to this they appear to add rather to the patient's discomfort, probably by the distension they occasion in the large intestine, which reacts upon the parts already distended by the obstruction.

When there is no tendency to sickness it is better to allow the patient to take food, in the shape of gruel, by the mouth. It prevents that sense of sinking which he often experiences, and it probably acts in some degree mechanically in propelling the contents of the intestinal tube.

In those severer cases, where there is frequent sickness, with pain in the bowels, and a rumbling of flatus, the above measures will be still further indicated. But there will also be other things which it will then be necessary to attend to. In these cases it is of great importance to abstain from giving any food by the mouth for some days. A tea-spoonful of cold water should be put into the mouth from time to time to allay the patient's thirst. His support should be entirely entrusted to beef-tea injections. It is proved that these are sufficient to maintain the strength for some time—at any rate, for a period sufficient to allay the irritating symptoms, which forbid the exhibition of food by the mouth. This part of the treatment I am inclined to consider as of the highest importance; for as long as food is continued to be administered by the mouth, and is rejected by vomiting, there will be little chance of arresting the inversion of the peristaltic action of the intestinal tube. The nutritive enemata should be of small bulk, not exceeding at the outside a quarter of a pint; otherwise they will not only not be retained, but they will add to the patient's sufferings. They should be administered at regular intervals of four hours. When there is much rumbling of the intestines, or when there is a difficulty as to the retention of the injections, it is advisable to add to them a certain proportion of laudanum.

I have confined my remarks on this occasion to the treatment of these cases

of obstruction: I have said nothing of their *pathology*. I shall probably be told that the severer cases which I have described are nothing but cases of enteritis; and this is probably true to a certain extent. But what I have been most anxious to discuss is the treatment of the milder cases. Whether they also are not most frequently cases of partial and slight enteritis I will not take upon myself to say, but I feel convinced that the safest way of treating them is to suppose that they are such, and to treat them accordingly. It may be said that some of these cases arise from intussusception or internal hernie. There is no doubt that this is sometimes the case, though, I suspect, less frequently than is generally supposed; but, if it were so, the treatment I have recommended would answer better than any other to this state of things also.

ON THE COINCIDENCE OF ECLAMPSIA AND ALBUMINURIA.

M. HIPPOLYTE BLOT submits the following conclusions on this subject:—

1. Albuminuria is frequent in pregnant women.
2. In almost all cases it is the result of a simple functional hyperæmia of the kidney.
3. It is remarkable and inexplicable, that *primiparity* seems to be a predisposing cause.
4. Besides the presence of albumen in the urine, other signs of this condition may be observed,—such as œdema and lumbar pains.
5. The albuminuria of pregnancy is almost always free from febrile excitement.
6. In nearly every instance the albuminuria disappears a few hours after labour.
7. This condition of pregnancy is free from danger when uncomplicated with cerebral congestion; it has no influence on the course or termination of gestation, on the development of the fœtus, on the duration of the labour, nor on the subsequent natural puerperal state.
8. Every case of eclampsia seen by M. Blot has been accompanied with albuminuria.
9. The relation between the two is obscure; they may depend upon sanguineous congestion occurring at the same time in the kidneys and cerebro-spinal centre.—*L'Union Médicale*, Oct. 10, 1850. x

ON
CARDIAC AND AORTIC DISEASES.

By R. H. A. HUNTER, Esq.,
Staff Surgeon.

HAVING already written so much of these diseases in India (to wit, between 1834 and 1843, in the "India Journal of Medical Science," and "Transactions of the Bombay Medical and Physical Society"), it will be impossible altogether to avoid repetition, and particularly as since that period we have added little to our store of observations; not, however, because we have not met with cases since, both in the East and West Indies, and also in England, but because we have had less favourable opportunities for watching their progress, and proving afterwards or otherwise the diagnosis by post-mortem examinations. Nor would we have ventured, perhaps, to resume the subject, had we not, when in the West Indies only two years ago, noticed a paper in the "MEDICAL GAZETTE" setting forth the rarity of such diseases in the East; for though the error was subsequently corrected by Dr. Morehead, formerly secretary to the Bombay Society, it may not be so generally known that they are there even more frequent and fatal, at least among the European soldiery, than in England; and here we may have the less hesitation, as it is to our own former writings Dr. Morehead has directly appealed. In our remarks we propose the following order:—First, to consider the character of these diseases as they occur in the East Indies; second, the diagnosis; third, the causes; and fourth, the prognosis and treatment.

First, of the character:—In the East Indies there are at least three varieties of cardiac disease ordinarily met with—the pericarditic, the simple hypertrophy, and dilatation—and that which is the immediate subject of these remarks, the cardiac and aortic. Of the first we met with three cases in the 2nd or Queen's The subjects of all of them were delicate growing lads, from fourteen to eighteen years of age, sons of soldiers of the regiment, and for the most part brought up in India. Two were published in the Bombay Transactions, No. 2; but the third was first seen only a few months before we left. The second variety is

much more numerous: it is met with among recruits of the first and second years, and, like that mentioned by Dr. Hope as being common among the young servant-girls from the country in London, seems to depend upon an excess of nervous excitability, coupled with some undue proportion of the organ. In these, the climate, together with the duties of the soldier in India, so increase the inordinate action, and particularly if treated for that or other diseases by depletion (fevers especially, in malarious districts), that they are now very generally invalidated. The third, however, is the chief variety, and also the cause of the mortality under this head among the European soldiery. It is usually met with from twenty-eight to thirty years of age upwards; but by far the greater number with us have been from thirty to thirty-five. In all of this variety there is a marked similarity in the post-mortem appearances, differing principally in degree, but in some measure also in site,—that is, in some of the cases the valves are found more especially affected with chronic thickening, induration, and occasionally perforation; in others, the base of the aorta with dilatation, irregularity, and induration, occasionally with aneurism of the secondary or sacculated class; and all more or less associated with hypertrophy and dilatation of the left ventricle at least. Sacculated aneurisms are common also without cardiac complication about the arch, and again where the aorta enters the abdomen; but not a single case we remember in any of the secondary classes of arteries, the innominate excepted, and only one of the popliteal. This last occurred in Afghanistan, in a man forty years of age, as he supposed, from leaping over the water cuts, or other obstructions (there being no such thing as roads, even of the rudest description, in that country), and was operated upon afterwards with success at Kurachee, by taking up the femoral.

Second, of the diagnosis:—This, also, we fancied we had pretty well exhausted in India (Bombay Transactions, Nos. 1 and 5), that is, as far as our own personal observations extend; but since we returned to England, and have become better acquainted with the writings of others, we find the morbid murmur, indicative of mitral disease, which we have everywhere associated with the

second or diastolic sound, is here universally referred to the first, as is also the prolonged sawing at the base. We certainly claim priority in directing attention to the true site of the mitral murmur (Case of Madden in "The India Journal," 1835, and "Bombay Transactions" of 1836-7); but it may not at all times be so easy to determine to which of the true sounds it does actually belong, even with the aid of the finger on the pulse;—a corrective we have never omitted. But if ordinarily a regurgitant murmur, why should it not be more audible at the base in the direction of the current, where Dr. Hope (now evidently upon theoretical grounds) originally assigned it, rather than at the apex? And as to the prolonged sawing or rasping at the base, we have long been impressed with the belief that it is due rather to a churning in that situation from the reflux caused by the resiliency of the arteries. At all events, any palpable imperfection of the semilunar valves is by no means necessary. In morbid states of the base of the aorta, accompanied with hypertrophy and dilatation of the left ventricle, whether with or without valvular disease, we have always found this sound double,—to wit, a shorter, synchronous with the abrupt bellows, over the clavicle, and a longer, occupying not only the period of the diastole, but also of the normal interval. These are points, however, which will come better to be discussed afterwards.

In forming the diagnosis, we have hitherto pursued the following system:—that is, first to write down carefully the history as given by the patient himself, so as to avoid his statements being influenced by any crude opinions or suggestions of our own, then the results by "inspection," "palpation," and "percussion;" and not till all these have been carefully noted do we have recourse to the stethoscope. The abnormal murmurs, including all the varieties of "blowing," "sawing," and "rasping," we divide into two classes—the quick, or abrupt, and the prolonged, always of a sawing character. The first, according to the site, we associate with mitral or aortic disease, including aneurism; the other, with dilatation or other morbid states of the base of the aorta, or of the semilunar valves. If the first be more audible, or, as it usually is, confined to the region of the apex, it is in-

dicative of mitral disease; if at the base, upper part of the sternum, or in the course of the arteries (if indicative of disease at all, determining this by the character of the sound, &c.), it is aortic or aneurismal. In dilatation and other morbid states of the base of the aorta, as already mentioned, these sounds are double; the first, which is brief or abrupt, becoming bellows over the clavicles; the other, prolonged, is sometimes extremely loud, and, as it were, superficial over all the sternum. In sacculated aneurisms, also near the base, we have noticed the first sound somewhat prolonged, and of a hissing character; more squirting, loud, or forcible, however, than the diastolic murmurs. In exploring for deep aneurisms, whether for the sound or the impulse, we prefer without the stopper. Those most difficult to detect have been about the "innominata," or in the abdomen,—parts, in fact, not naturally sonorous. Two at the root of the neck, and several in the abdomen, have eluded our search altogether, even when directed to the very place by the symptoms. One of these, lodged between the upper portion of the sternum and the trachea, from the loud (steam-engine like) respiration, was mistaken for "tracheitis;" the other, at the root of the neck, from the persistent aphonia, for chronic disease of the larynx. In fact, this patient (an officer of the 68rd regiment) had been sent out to India (Madras) on this very account. He had only once been explored, however, and that very cursorily, at an Invaliding Board; nor was the aneurism ultimately proved; for no autopsy could be obtained: but the symptoms and history, together with the manner of death, left no doubt of the fact. Some weeks after this, he went to bed in his usual state, and was found dead in the morning. Of the abdominal, one eventually broke into the left side of the thorax, and yet remained undetected till revealed by the post-mortem examination. Another was mistaken for spinal disease,—bathing when over-heated with marching assigned as the cause, helped to mislead: and another for simple lumbago. Nevertheless, a more satisfactory diagnosis we have scarcely made than in abdominal aneurism. Bellows murmurs are so common, however, resembling the aneurismal, in the epigastrium, and also along the course of the

aorta, with strong pulsation, if not impulsive, that they materially interfere; though we cannot agree with those who think the murmur of pregnancy may not be distinguished from every other by its peculiar long rolling or buzzing character.

Cases.—Here we would avoid the too common error of taking our own little village to be the type of the great city, Rome; and the better to do so, may be permitted to advert to the localities and climates, in which these diseases more especially occur. In 1829, having been sent in charge of invalids from Ceylon to the Cape of Good Hope, we were there as a temporary measure attached to the 72nd, then in Cape Town, and were thus led to inquire as to the nature of the diseases peculiar to the climate.

It was said the climate is extremely healthy, and particularly on the frontier; only there diseases of the heart and blood-vessels are common, which is attributed to some peculiarity of the water. Again, at Barbadoes, in 1830, where we had gone a short time previously from England with detachments, we were in orders to proceed in medical charge of a wing of the 19th to St. Vincent, whereupon Mr. Poole, who had been there a year or two as Assistant-surgeon of the 27th regiment, remarked, St. Vincent is famous for diseases of the heart and blood-vessels, owing to the long steep ascent from the town, where the principal duties are. It is true, we were unable to prove this from the records, when at St. Vincent during the years 1848-9, or to make out, even at the period referred to by Mr. Poole, more than 1 fatal case, or 1.5 per 1000 of strength, but as these cases, at that time, seldom proved fatal in hospital, the records may not be the best place to find them, and now the strength is too small, and nearly all the town guards are withdrawn, not to speak of the comparatively short tour regiments have in the West Indies. Next we went out to Bombay, and again diseases of the heart and blood-vessels assailed us, but here it was the "abominable arrack," drunk in the Bazaars, or smuggled in by the natives. It was not, however, till at Poona, four or five years after that, that these diseases reached their acme in the 2nd or Queen's, nor were they at that time confined to the 2nd, or even to the Infantry. The fact is, they were only just then, and chiefly through the zeal of Deputy-

Inspector Collier, beginning to be fully recognised during the life of the individual. Nor must we too hastily conclude, because of their greater frequency in the East, that such is the fact absolutely. An European regiment in India is seldom under 800 strong, and almost always undivided, whereas on the Cape frontier, or even in the West Indies, Barbadoes excepted, there are scarcely 300 together anywhere. One fatal case in 200, the usual strength, would be equal to four or five in a regiment in India, and 5.5 is the highest we have known per 1000 of strength. The average, aneurisms inclusive, used not to exceed three, and now that any tendency to disease of the heart is looked for as a valid objection in the examination of recruits, a considerable diminution may be expected. It may be true, the Cape frontier, both in climate and locality, resembles many of the stations in the interior of India, but are there not other and more palpable causes, and particularly as at Poona, where the mortality from these diseases reached, the third year of the Queen's, the extraordinary ratio of 5.5 per 1,000? Poona rarely has a force of less than 4,000 men, and the scope being ample, field exercise, during the drill or cool season, are both frequent and severe; it was so at least at the period referred to, the ordinary routine being three field days a week, in addition to the usual regimental drills and parades; and these field days are much more trying to the organs of circulation than even continued but regular marching, as may be easily understood when it is considered at one time they are at "the double"—that is, running; at another, still or inactive; now half suffocated in column, and again in line, open and free to the breeze. Add to this the woollen jacket closely buttoned up, and the thick leather stock, and we can scarcely be at a loss, reasoning from analogy, to account for diseases of the heart and blood-vessels in that or any other hot climate, without the aid of peculiarities of water, long steep ascents, or abominable arrack, or even of rheumatism, as in civil life.

Prognosis.—It is rarely, as already mentioned, that cardiac and aortic diseases are traceable, among the European soldiery in India, to rheumatism; nor do they usually come under treatment at a very early period of their course. The greater number of the

cardiac prove fatal in one or two months from admission, where the valves of aorta are manifestly implicated. One or two we have known to prove fatal in one or two days; but in these venesection had been rather liberally employed perhaps, with a view to reduce the inordinate impulse. Some, on the other hand, have extended over a period of one or two years; whilst a few, having left as invalids or otherwise, have been lost sight of altogether. The most unfavourable prognostic has been the prolonged sawing murmur at the base, or abrupt and hoarse bellows at the apex; and when to either of these has been superadded an inordinate impulse, death has, in the majority of instances, been nearly at hand. The longest period we have known a sacculated aneurism of the thoracic aorta to a certainty diagnosed, was about fifteen months. This was situated near the origin of the left subclavian, and till death was only appreciable by the physical signs: pneumonia was the disease originally inferred from the symptoms alone. Three cases in particular of cardiac or aortic disease were discovered accidentally, when the patients were in hospital for other and unconnected diseases; nor had they previously complained, or even been aware, of any symptom of such. One of these was that of a man of the 17th Regiment, soon after his return from the campaign in Afghanistan. There was then no inordinate impulse, and, altogether, it appeared to be a case of very recent origin; yet hypertrophy and dilatation soon supervened, and proved fatal within the six months.

From cardiac and aortic disease we have known no case of recovery when once the prolonged sawing murmur was manifest at the base; nor have we known a single patient to be alive, except one of the growing boys already alluded to, at the end of two years; and therefore have little to recommend in the way of treatment. Venesection, however, is especially to be avoided, and all remedies of a directly lowering nature. Temperance in all things, quiet in the most extensive sense of the word, and loose clothing, will be readily suggested to every one; and, if to these means be added a few leeches occasionally, and such other moderate remedies as may from time to time become necessary for the relief of urgent symptoms, or of secondary diseases, as hepatitis,

pneumonia, or dropsy, it is the utmost, perhaps, medicine can effect. Mercury, as in the rheumatic disease, if at an early stage of its course, might possibly do good, or sarsaparilla with the iodide of potass.

Bristol, Oct. 11, 1850.

N.B.—It is not meant that the above diseases are confined to the non-commissioned, nor can we say absolutely that they are even more frequent in that class; for, owing to the comparatively small number of officers in a regiment (30 to 35 actually present in India) it is not easy to determine the point. Many officers, too, have an aversion to being stethoscoped, as it is called, when labouring under any cardiac symptoms, whether it be functional or organic. One in particular (no doubt of the former class) could not even see the instrument approach the region of the heart without becoming sick or faint (the same thing we have seen in dyspepsia on pointing the finger at the stomach); and another, the late Assistant-surgeon of the 6th, or Royal Warwickshire, would not, because (as he said) he was sure it was organic. This was at Poona, in 1840; and, by the newspapers, we observed he fell suddenly dead at Chatham the following year, a few days after he reached England with his regiment.

In none of this class either in India could the disease be traced to rheumatism. Only one admitted "lumbago," a short time before the cardiac symptoms commenced. In two, however, it was the simple hypertrophy, loaded with fat, with enlarged liver, &c.; and one perhaps was congenital; but neither would this submit to an exploration, and at last he died dropsical away from head-quarters.

NEW TEST FOR THE DETECTION OF SULPHATE OF QUININE. BY M. VOGEL, JUN., OF MUNICH.

WHEN a concentrated solution of ferrocyanuret of potassium and a few drops of chlorine are added to an alcoholic solution of quinine, the liquid assumes a clear red colour. If the solution of prussiate be not concentrated, the addition of a few drops of ammonia will produce the tint. The same test is applicable to dry quinine. No other organic base produces the same effect, according to M. Vogel: this test may therefore be relied upon for the presence of quinine.—*Journal de Chimie Médicale*. x

ON THE
OCCURRENCE OF STARCH GRA-
NULES IN THE URINE,
AND ON THE MICROSCOPIC EXAMINATION
OF THIS SECRETION.

BY THOMAS INMAN, M.D.
Liverpool.

MR. BALMAN, in a recent paper in the *MEDICAL GAZETTE*, p. 799, draws attention to the fact that starch granules are often found in urine, and mentions one case where they were abundant enough to form a distinct deposit visible to the naked eye.

I can corroborate his statements respecting the frequency of the presence of granules, and his observation that they consist usually of wheat starch. The fact is one of considerable interest. It is, I think, almost self-evident that they must exist in the urine accidentally, and must be introduced into it subsequent to its passage from the bladder.

We may imagine many ways in which they may be so introduced; the most common is probably the following:—Bread being an universal article of diet, its crumbs are to be found in every house. Falling on the floor they harden, get trodden to dust or powder. When the floor is swept or the foot passes over, the dust rises, and part gets deposited on the internal surface of all the chamber utensils, and of every unclosed bottle. Urine is voided into its usual receptacle, and poured thence into a bottle for examination, and is then found to contain wheat starch.

That such is occasionally the case I may illustrate by the following observation:—

The urine of a young man who had signs of a morbid growth in the bladder was given to me for microscopic examination. In it I detected a very minute white mass, like a flake of lymph. It consisted apparently of a number of indistinct reticulations, and a number of cells which closely resembled in their general aspect some animal formations. Alas! I saw one which had an appearance as like boiled wheat starch that I applied the test of iodine. I then found that all were turned blue, and that the fragment was therefore a piece of bread or biscuit accidentally introduced.

When the quantity of starch is as great as that described by Mr. Balman in the case he narrates, we must suppose that the patient introduces it in some way, either accidentally or with the intention to deceive.

I may add, in corroboration of the idea that in no case can the starch be a urinary product, that this fluid will entirely dissolve when added to a hot solution. It would equally prevent the formation of the granules.

While on the subject of the urine, I may mention a curious observation I made a short time ago on a specimen sent me for examination. When first seen it contained nothing beyond cubical crystals of oxalate of lime, which existed in immense numbers. Next day more than half of these had disappeared, and their place was taken by stellar and prismatic crystals of the triple phosphate.

As the phosphatic and oxalic diathesis are supposed to be closely allied, the apparent conversion of one deposit into the other out of the body is particularly interesting.

I scarcely need add that the power used was a $\frac{1}{4}$ inch of Powell's, that there was no mistaking the one set of crystals for the other, nor was there any opportunity for any practical joker to change the contents of the bottle.

Liverpool, Nov. 1850.

UNIVERSITY OF LONDON.

SECOND EXAMINATION FOR THE DEGREE
OF M.B.—1850.

First Division.

Bristowe, J. S., St. Thomas's Hospital.
Growse, R., Guy's Hospital.
Hewitt, W. M. G., University College.
Lewis, R. B., Leeds School of Medicine.
Manley, E., Royal Manchester School of Medicine.
Slater, H. H., King's College.
Shearman, C. J., University College.
Thornton, W. H., Royal Manchester School of Medicine.

Second Division.

Filliter, W., University College.
Heale, J. N., St. Thomas's Hospital.
Jones, J., Royal College of Surgeons of Ireland.
Robertson, J., St. Thomas's Hospital.

INDURATION OF THE STERNO-CLAVO-MASTOIDEUS MUSCLE IN YOUNG INFANTS.

Communicated by T. B. CUMMINGS, ESQ.,
Surgeon to the London Hospital.

WHILST I had charge of the out-patients at the London Hospital, infants a few months old were occasionally brought to me on account of stiffness of the neck, which I found to proceed from a remarkable induration of the sterno-clavido-mastoides muscle on one side. The muscle was in all cases well defined, and felt rigid, and almost of cartilaginous hardness. I have preserved notes of four cases, in all of which the muscle affected was the left. The affection did not appear connected with any derangement in health, or to produce any inconvenience beyond the stiffness alluded to; but this, coupled with the hardness, was sufficient to excite some alarm in the minds of the parents. I do not recollect having met with any instance of it later than ten weeks after birth, and generally the infant was about three or four weeks old. In the last case that I saw, which was sent to me by my colleague Dr. Frampton, the child was aged seven weeks. The induration invariably subsided very gradually. The treatment resorted to was the application of a mild liniment and a grain or two of the Hydrargyrum c. creta.

I have had no opportunity of ascertaining the structural character of this curious muscular affection, nor of determining its cause; but, from the early age at which it occurs, and ultimate subsidence without impairment of the part, I suspect that the induration was consequent upon some injury to the muscle at birth, producing a chronic deposition of lymph between the muscular fibres. I have inquired of several experienced accoucheurs, but I found they were unacquainted with the affection, and unable to account for it.

NATURAL HISTORY OF THE CINCHONAS— THEIR GEOGRAPHICAL DISTRIBUTION.

THE geographical distribution of the Cinchonas, according to M. Wedell, extends from 19° south lat. to 10° north lat., and forms a large arc of a circle, having its convexity turned towards the west, its most western point being about 4° S. lat. and 80° W. long. (merid. Paris), its northern

extremity being in 69° W. long., and its southern extremity towards 65° W. long. The width of the zone varies in different parts, and gradually diminishes towards each extremity. It was observed that beyond a certain altitude it became confounded with the belt of forest, and ceased with it.

The Cinchonas abound most on the eastern slopes of the Andes, the western sides of which are almost without wood. The streams which water the regions of the Cinchonas are, with small exceptions, the tributaries of the Amazons, with some of those of the Orinoco. According to preceding travellers, the height at which the Cinchonas vegetate is from 3956-00 to 10725-60 feet above the level of the sea, and the mean height of their limit is between 5248-00 and 7872-00 feet.*

M. Wedell having closely watched the growth of the Cinchona, and the process of development of the bark, divides the latter into two kinds—the outer, or more fibrous portion, and an inner, or cellular portion. The latter he states is full of quinine, the former in cinchona. M. Wedell makes, however, this distinction,—that it is not in proportion to the amount of cellular tissue that the quinine is abundant, but that the bark is rich in quinine in proportion as the cellular and fibrous tissue is intermingled. The most favourable combination appears to be a certain distribution of the fibres among the surrounding cells, which are filled with resinous matter, whereby each fibre is, as it were, isolated, or they are separated by thin laminae of this cellular structure. The fracture of a specimen indicates the character: when the cellular tissue predominates the fracture resembles that of cork; while in the fibrous variety the fracture is characterized by numerous points and filaments.

M. Wedell states, also, that he has every reason to believe that the root possesses the same properties as the bark.

M. Wedell has reduced the number of species of Cinchonas from twenty-four, the usually received number, to eleven. To these, however, eight hitherto unknown species must be added.

M. Wedell attributes the varieties of Cinchona met with in commerce to differences in their age, exposure, &c., as they may all be produced from one tree.—*L'Union Médicale.* X

* The highest of the Andes,—the Nevada de Sorata, in Bolivia or Upper Peru, is said to be 25,350 feet, or nearly five miles, above the level of the sea. The next highest of these mountains is Illimani, in Peru, the summit of which exceeds 24,500 feet.

The line of perpetual snow in the equatorial regions of South America is at the height of 18,500 feet.

MEDICAL GAZETTE.

FRIDAY, NOVEMBER 23, 1850.

Among the Blue Books which have been lately issued, there is one which possesses considerable interest in a medical point of view. We allude to the Report of the Select Committee of the House of Commons on Prison Discipline. The facts and opinions therein contained, having reference to the effects of separate imprisonment on the mental and bodily health, are so important, and in some respects so unexpected, that they demand, and will no doubt receive, serious attention.

It is a commonly-received opinion with the public, as well as among members of the medical profession, that separate confinement cannot be inflicted upon prisoners without inducing greater evils to body and mind than are consistent with justice, or than are counterbalanced by the good which may result, in a reformatory point of view, from so severe a punishment.

An examination of this Report will, we think, serve to remove this impression by showing that separate imprisonment is perfectly compatible with good health, and that it is even attended with an immunity from disease not to be found in many honest callings. It demonstrates, moreover, that separation is a system more favourable to, and more positively productive of reformation, than any other mode of punishment. It is not, however, our purpose to intrude upon the province of the moralist or the jurist: * we shall

* For information upon the punitive and reformatory fruits of the "separate system," we must refer our readers to the facts stated in evidence before the Committee; to the yearly Report of the Commissioners of Penitentiary Prison to the letters received from convicts in the Colonies; to a work entitled, "Prisons and

confine our observations to those parts of the Report which have a medical bearing. These may be arranged under three heads:

1. *Opinions in which the separate system is regarded as unattended with excessive injury to the mental and bodily health.*

2. *Opinions in which it is considered to be inevitably attended with excessive injury to body and mind.*

3. *Facts by which the separate system is shown to be free from excess of danger to body and mind, and thus a perfectly safe form of punishment.*

1. *Opinions in which the separate system is regarded as unattended with excessive injury to mental and bodily health.*—These are, to be found, repeatedly and confidently expressed by statesmen of the highest integrity; by legislators who have devoted time and earnest attention to the subject of prison discipline in all its varied forms; by governors of prisons, both upon the separate and under other systems; by chaplains, who declare that they find it to be the only machinery that holds out a prospect of reformation; and, lastly, by medical officers, who have watched its working, and have not only not detected any injury to the minds and bodies of prisoners as its result; but have even found it to be in some cases beneficial to the health.

The verification of all these statements would carry us beyond the space which we can spare to the consideration of this first division of our subject; but proofs will continually appear in the facts to be subsequently brought forward. We observe, however, that at a period when the merits of the separate system were only beginning to be developed, the late Sir Robert Peel thus

expressed," written in
Chambers to Penit

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old.

embodied the result of his investigations on this subject, in the preamble to a bill which he introduced into Parliament in 1843:—"Due classification, inspection, separation, regular labour and employment, religious and moral instruction, are essential to the discipline of a prison, and for the reformation of offenders."

Most politicians have arrived at the same conclusions;* and the last authoritative exposition of the kind comes from the Committee whose Report is before us, and which we here quote:—

Resolved,—“That this Committee concurs in the opinion that entire separation, except during the hours of labour and of religious worship and instruction, is *absolutely necessary* for preventing contamination, and for securing a proper system of prison discipline.”

Up to the end of the year 1847, the Reports of the Medical Commissioners of Pentonville Prison had confirmed the opinion that the health of prisoners suffered no damage, or that any evil possibly consequent thereon was counterbalanced by its alleviations. These reports being confirmed by those of the surgeons of convict ships, who removed the prisoners to the colonies, are entitled to some respect, but they are still more deserving of notice, when it is considered that they are the results of Sir Benjamin Brodie's observations for six years, of Dr. Fergusson's also for six years, of Dr. Bisset Hawkins' for a shorter time, of Mr. Green's for two and a half years, and of Dr. George Owen Rees's daily superintendence and visiting during a period of seven years.

Mr. Hampton, surgeon superintendent of the “Sir George Seymour,” who took out 345 prisoners to the colonies, speaks in high terms of commendation

of the men, stating that their bodily health was good, and “their intellect in a more vigorous and healthy condition than in any prisoners he had previously observed.”

Mr. Baker, surgeon superintendent of the Stratheden, remarked, that the prisoners from Pentonville “were in excellent health, more so than the sailors of the ship, and that in his opinion they were healthier than the same number of men out of a man-of-war would have been: they were as active and intelligent men as ever he saw.”

Abundant additional testimony to the same effect might be adduced from similar sources. This testimony is accessible in the Report with which we are now concerned, and in the yearly Reports of the Pentonville Commissioners.

2. Opinions which regard separate imprisonment as inevitably attended with excess of injury to body and mind.—In the present volume of evidence these do not bear any proportion to those of an opposite character. Even among most of those adverse to the system, their conclusions have reference rather to the time during which it can be safely borne, than that it is altogether unendurable. This view of the question will be shown in our subsequent observations to have been also definitely settled; and, moreover, it will be shown from the facts elicited at Pentonville Prison (which we may here state we shall assume as the basis of our comparisons), that the question of the time for which the system may be applied is the most important of any involved in the discussion.

We take the Pentonville Prison as a basis, because it is the “Model” prison in which the “great experiment” (as it has been justly designated by Sir James Graham) of modified separation has been reduced to practice; and we shall show that the experiment has been

* They have been embodied in a Report by a Committee of the Lords on Prison Discipline in 1835.

successful, or at all events that it was so in the earlier years of the trial.

It may here be noticed that the separate system as exhibited in the Model Prison at Pentonville, must be kept distinct in the mind from the solitary system as at first adopted and practised in America; and from the separation which (with less efficient alleviations than at Pentonville) is put in force for short periods in some other prisons of England.

It is obvious that the comparison of the effects of separate confinement on body and mind must be different among prisoners confined for a like period, part of whom are undergoing the punishment as the first term of a long period of transportation, and a similar number undergoing the same period as their entire punishment.

It must also be borne in mind, that before the conclusion can be arrived at that the discipline of any particular prison is productive of ill effects to the health, over and above that of any other prisons, it is highly necessary that all conditions be ascertained to be similar. For instance, it must be known whether the diet be equally good, whether the situation be equally salubrious, and whether the physical condition of the prisoners be the same; thus the results obtained from the observation of prisoners consisting of agricultural labourers cannot be the same as those compared with a prison full of low London or Manchester thieves and pickpockets. Much difference will be found in the two cases; but this is explicable on other grounds than the effects of the system.

The following extract from an article in the Quarterly Review, for December 1847, well expresses the light in which the system should be viewed:—

“What are the effects of such a system of discipline? Quite innocuous,

say some; madness or premature disease, say others. Both opinions are partial. It is hardly to be expected that an individual can fail to suffer when he is at once imprisoned in body and constrained in mind. On the other hand, those who have designated prisons on the separate system as ‘manufactories of madness’ have probably confounded the solitary with the separate system. In France, Esquirol, and other high authorities on mental diseases, have asserted that the latter system has no tendency to deteriorate the mind; and, as far as a five years’ experience of the working of discipline at Pentonville has gone, close observers coincide with them.”

We would refer those of our readers who may be disposed to take an interest in this subject, to this article in the “Quarterly,” as being a masterly exposition of the whole question, attributed to the pen of one well qualified, by his thorough knowledge of the subject, to offer a trustworthy opinion. As a review of the results of the trial of the earlier system of long periods, this article supplies valuable evidence to the effect that separation is unattended with injury to bodily and mental health. It is shown from statistics that the prisoner in Pentonville suffers no more, or that his mortality is no higher, than that of the soldier in the choicest spots of the Mediterranean, or in the bracing climate of Canada. We quote again on this point:—

While the annual mortality at Pentonville is (in 1847) 15·70 per 1000, that of the Foot Guards is 21·0; nay, the mortality from consumption alone in the Guards is nearly as high (14·1 per 1000) as the total mortality of Pentonville prisoners—men, be it observed, of about the same age, on an average, as soldiers.

“At Pentonville, then, the daily ratio is 14 sick in 423, or about 33 in 1000. In the Prussian army the daily average is 44 per 1000. Among the British Dragoons, and Dragoon Guards, 44 per 1000.”

We find three different modes adopted at Pentonville in calculating the mortality of the prisoners:—1. The actual mortality—i. e., the number of deaths. 2. The actual mortality, to which are added three-fifths of the medical pardons as a fair proportion to be allowed for the deaths that would have ensued from the diseases which formed the ground of pardon. 3. The mortality, including the *whole* number of medical pardons. This will give too high an estimate, as many pardoned on medical grounds have been known to recover. In the first case we find the mortality to be, in round numbers, about 6 per 1000; in the second, about 8 per 1000; in the last, about 14 per 1000. The entire prison population showed a mortality (from 1838 to 1842) of about 19·62 per 1000, according to the calculations of Dr. Baly, physician to the Millbank Penitentiary.

We have noticed, in going over the evidence in this Report, that the witnesses who expressed themselves most decidedly on the injurious influence of separation did not support their opinions either by positive or negative facts.

Thus Dr. Baly, in answer to the question whether separation shows itself more injuriously at the commencement than at a more lengthened period, answers—"I cannot lay down any rule as to the period at which the mind becomes affected by imprisonment. As regards the bodily health, the longer the imprisonment is continued, as far as the end of three years, the greater is the effect; but as regards the mind there seems to be no precise rule. Many prisoners suffer in their minds at an early period; others, and I think more, at a later period; but there is no great difference in the proportion at the two periods."

Now we cannot but suppose that, if an uniform cause be in operation, the

effect will be uniform, and that there will be a "precise rule" discoverable:—with regard to Pentonville, this has been shown in the tables we shall subsequently quote.

Again, Dr. Baly states—"A great many prisoners going to Millbank from separate imprisonment have suffered in their health; some as invalids, some as incorrigible, some in bad bodily health, others disordered in their minds." But at the same time the relative periods for which they had previously undergone rigid imprisonment under the separate, and under the other systems, are not stated. It cannot be doubted that facts, in the shape of the actual numbers thus asserted to have suffered, would have been most desirable.

Captain Groves *thought* that prisoners under separation were in worse health than those in association; but adduced no facts in support of his opinion.

Captain Macdonochie, allowed that "separation is a very good servant, but a very bad master;" but that "it is very desirable to have a separate apparatus." His objections to the separate system, as a mode of punishment, were founded on its trial by himself on twelve prisoners for the longest period (in one case only) of six months. Of the moral conditions under which this experiment was made we are left in ignorance.

The evidence of Captain Whitty, Governor of Portland Convict Prison (to which the Pentonville prisoners are removed) was not more precise. We quote portions thereof:—

Q. "In what state, mentally and physically, do you generally find those prisoners that are brought under your care when they come from separate confinement?"

A. "Latterly they have been much improved in that respect. In the first instance, when the prison was established, I think many came to us from

separate confinement in a very reduced state from some of the prisoners."

This witness then deposes that the prisoners recover their health and spirits after removal to Portland—"all of them improve in health and cheerfulness." On this topic the following examination ensues:—

Q. "Then you find that, though in 1849 the mental and physical depression of the prisoners was not so great as it was during the early period of your government, still all the prisoners do come to you mentally and physically depressed?"

A. "I have observed that, to a certain extent, the prisoners are in a state of physical depression when they come, but I should not like to undertake to say that they are in an actual state of mental depression."

Q. "You stated, in answer to a former question, that you observed a change in the prisoners in regard to their cheerfulness, after they have been under your care for some time?"

A. "Yes."

Q. "Do you infer from that fact that there was a degree of mental depression at the time that the prisoners came under your charge?"

A. "Yes: a degree. That is what I meant to say just now."

Few persons, we conceive, would give to the above evidence a higher rank than that of mere loose opinion, the result of casual observation, and not of close examination on the point referred to. We cannot attach a more definite character to this witness's statements in reference to the bodily and mental effects of separation.

The influence of the separate system in producing or developing phthisis has been much dwelt upon. That an excess of phthisis did appear in some years at Pentonville prison was admitted by Dr. Rees, who traced it to the dusty trades taught to the men in their cells. On the adoption of proper measures the mortality from this cause, as we have observed in a former number of our journal, suddenly abated

We shall take an early opportunity of returning to this subject.

CONCLUSIONS REGARDING THE ELIMINATION OF IODINE BY THE KIDNEYS.

DR. DALTON, of Boston, has drawn the following conclusions from his researches on this subject:—1. After a single moderate dose, iodine may appear in the urine in so short a time as thirty minutes, and continue to appear for nearly twenty-four hours afterwards. 2. After a much larger dose, taken habitually for a long period, the time necessary for complete elimination is not proportionally increased. 3. It is therefore probable that iodine, or at least iodide of potassium, does not accumulate in the system in any considerable quantity; and it is questionable whether a large dose, taken habitually, produces any more constitutional effect than a moderate one, since the superfluity is constantly removed by the kidneys. 4. The colour, produced by the reaction of starch with urine containing iodine, varies, according to circumstances, from a light purple, slaty-blue, or French gray, to deep indigo, or absolute black. 5. A solution of iodide of potassium in distilled water, in the proportion of one-eighth of a grain to the ounce, produces, with starch and nitric acid, an opaque black colour, as strong as is ever observed in urine. A solution in the proportion of one-sixteenth of a grain to the ounce produces a strong purple, moderately translucent. 6. It is therefore probable that iodide is often excreted in the urine in about as large a quantity as one-tenth of a grain to the ounce; since one-eighth of a grain of iodide of potassium contains nearly one-tenth of a grain of iodine.

Iodine is usually perceptible in the saliva at the same time that it appears in the urine; but it does not always exist in these two secretions in the same proportions. Thus, in one instance, the urine showed only a faint purplish colour by the reagents, while the saliva exhibited a strong blue. The same or even greater irregularity was observed by Dr. O'Shaughnessy in the case of the dog above referred to, since the poison was abundant in the saliva on the third day, but could not be detected in the urine: on the fourth and fifth days, however, it again appeared in the latter excretion. Becquerel (p. 128) speaks particularly of these occasional variations, and acknowledges that we cannot, at present, refer them to any fixed law.—*American Journal of Medical Sciences* for July, 1850.

Reviews.

Contributions to Mental Pathology: with Introductory Observations: containing the Past and Present State of the Insane at Ceylon, &c. By JAMES GEORGE DAVEY, Licentiate of the Royal College of Physicians of London, &c. &c. 8vo. pp. 276. London: Churchill. 1850.

NEARLY one-third of this volume consists of a narrative of the annoyances to which the author was exposed in the performance of duties for which he was sent out to Ceylon by Lord Stanley. Dr. Davey seems to have been placed in a false position, and, according to his own account, to have been extremely ill used; but in this, as in all quarrels, something may probably be said on both sides: we shall therefore not dwell on this point.

The remainder of this work comprises three reports on the State of the Lunatic Asylums in Ceylon, presented to the Colonial Government. These reports, unfortunately, also include much of the correspondence between their author and the Colonial authorities with respect to the measures which it was indispensably necessary should be adopted for the amelioration of the condition of the insane in Ceylon, which was as bad as bad could be.

A few extracts will put our readers in possession of the author's pathological views with reference to insanity.

"We have employed the words 'mania,' 'melancholia,' 'monomania,' &c. &c.; and we have done so, not because we consider their use in the least degree calculated to elucidate the various abnormal phenomena of the mind, but for this reason—the want of a correct and classical nomenclature of mental diseases. No person, unless he be a phrenologist—that is, unless he is well acquainted with the functions of the brain in a state of health—can possibly be a good judge of the indications of an unsound mind" (p. 90).

On this dictum of the author it is possible that there may be two opinions. For our own part, we altogether decline accepting the proposition otherwise than as an assertion the *onus probandi* of which lies with Dr. Davey.

We do not consider that the case given below furnishes conclusive evidence of

the therapeutic action of Mesmerism. Our readers do not require to be informed that insanity, epilepsy, and apoplexy, are not unfrequently combined in one subject, and that the symptoms of either form of disease may precede the other, and subside to give place to those of a fatal character.

"We feel we should be guilty of great and unpardonable neglect, and moreover incur, and deservedly too, the censure of the man of science and the philanthropist, if we were to allow this opportunity to pass without insisting on the remedial efficacy of animal magnetism, or mesmerism, in cases of epilepsy combined with mental disease. Since our connection with Heddelle, we have succeeded in restoring one poor native lad to perfect health who, for a period of, we learn, seven years, was afflicted with epilepsy of the most distressing character. He used to have from two to five fits per day, and each one was commonly so severe as to leave him for some time more or less insensible. His appearance when first mesmerised was exactly that which belongs to the last stage of phthisis, so emaciated was he. Three months after we commenced our operations, not only had the fits entirely left him, but he then presented the appearance of robust health, and had become stout and really muscular. The case created a great sensation among the servants, whose remarks upon it were not a little curious. When on the eve of obtaining his discharge he was attacked with apoplexy, and died about thirty hours after" (p. 98).

It is a question that may fairly be asked, Why Dr. Davey, being a believer in mesmerism, did not ascertain beforehand, by the means of clairvoyance, whether there existed in Ceylon any obstacles to the fulfilment of his mission such as he has put on record in this volume.

The following severe criticism on the opinions of some of the highest legal authorities of the present day seems to suggest the possibility that the manner as much as the object of Dr. Davey's performance of his duties in Ceylon may have given offence to his professional brethren holding high offices in that island:—

"The judges of the present day, like those of the olden time, persist in declaring all persons to be *sane* and *responsible* who can distinguish *right* from *wrong*. Lord Hale's dictum continues yet to influence both our legislators and lawyers. The bar and the bench alike continue to grovel

in the mire and obscurity which characterizes the public acts of such men as Mansfield, Erskine, Denman, Abinger, and Follitt, as shown on the trials of Bellingham, Hatfield, Oxford, Blacklesley, and M'Naughten, to which the reader is here referred as an illustration of the false reasoning employed by the legal profession in matters relating to the question of insanity.

"I would beg leave to introduce to the reader's attention the following extracts from my medico-legal reflections on the trial of Daniel M'Naughten" (p. 120).

The last paragraph stands in unhappy proximity to the unqualified charge of ignorance fastened on the judges of the land by the author. Dr. Davey would have the responsibility of lunatics determined by phrenology, and observes, "that the sooner the LAW OF LUNACY is blotted from out the statute-book of the empire, so much the better for the law and lawyers; and if not for it and them, for Ransom and Henshaw's sake" (p. 120).

But, with all due deference to Dr. Davey, we may repeat what has been said on another occasion:—

"It is justly supposed, from the experience and knowledge which the members of the bar must acquire before they are promoted to the judicial bench, that there are no men better qualified than our judges to interpret the laws of this country."

Beyond the author's phrenological and meager opinions, a good account of the treatment adopted at Hanwell, and the topics of local interest to which we have already alluded, we have found but little in this volume to justify our recommendation of it to the notice of our readers.

An Introductory Lecture, delivered at the Massachusetts Medical College. By HENRY J. BIGELOW, M.D., Professor of Surgery in Harvard University. Pamphlet, 8vo. pp. 52. Boston, U.S.

An interesting discourse, pointing out the mode in which surgery should be studied, and the errors of surgeons avoided. The fashionable quackeries of the day are noticed, and their sources traced. In the exposition of all the varied topics which are embraced in this Lecture, a thorough knowledge of surgical science is seen to be combined with great command of language and extensive literary acquirements.

* See editorial observations on the case of *Nottingham versus Bigelow*, *Mass. Gaz.*, N. S., vol. ix. p. 1061.

The Unity of Nature. By CHARLES BLAND RADCLIFFE, M.R., &c., Lecturer on Vegetable Physiology and Botany at the Westminster Hospital, &c. 8vo. pp. 150. London: Churchill. 1859.

THE analogies observed between the varied forms of organization have afforded to men of speculative turn of mind the data whence they have deduced the laws of the unity of nature, and the facts on which they found the science of transcendental anatomy. An extensive acquaintance with the structures both of animal and vegetable life is demanded for the successful pursuit of this branch of science, and is doubtless possessed by those whose names stand foremost among its cultivators: we need only mention those of Owen, Macleise, Oken, and Cuvier, with whom we think may now be joined that of Radcliffe.

Dr. Radcliffe has arranged his subject under the two chapters of *unity of form*, and *unity of force*. In the first, the unity of form to be observed in the general plan upon which the several organs of plants and animals are constructed is treated; then the uniformity to be observed in the entire organism. Unity of form is next traced between the plant and animal. By transferring the author's words on this point to our pages we shall afford our readers an opportunity of judging of the general character of the work before us.

"1. In the perfect plant, the arrangement of parts in a spiral, branching, or radiate manner, together with the absence of the power of spontaneous movement, may be considered as general and universal features of organization. The presence of such characters, indeed, is involved in the very idea of a perfect plant.

"2. In the animal, also, we may always discern branching, radiate, and spiral tendencies in the plan of organization. The power of spontaneous movement, which is the attribute of the higher forms of animal life, may seem, indeed, a fatal objection to the analogy of the plant and animal; but this objection is only apparent. Such a power cannot be deemed characteristic of every animal form: indeed, in the earlier periods of the history of the nobler creatures,—which epochs are typical of vegetable life,—and in myriads of forms belonging to the radiate and molluscous provinces of nature, where the plant-phases of the embryonic state of the higher animal, are permanent conditions of being, not only

is there an absence of the power of spontaneous movement, but the animal is actually a prisoner like the plant. . . . The parallelism of the animal to the plant, in the general characters of the organic plan, is seen, however, more distinctly, if we travel successively through the several ranks of being." (p. 47.)

If our readers have any love of the science of anatomy and physiology, they may profitably follow the author.

The points of resemblance which can be discovered in the general features of organic and inorganic nature are fully set forth by the author.

Under "Unity of Force," which constitutes the second division of his subject, Dr. Radcliffe touches upon Mr. Grove's exposition of the "Correlation of Physical Forces," and quotes several of Mr. Grove's illustrations. In speaking of "the phenomena in vital force," the author dwells upon the nature of man, his original character, his present fallen condition, and his future destiny. We think Dr. Radcliffe ventures on dangerous ground in attempting a definition of the humanity of Christ:—

"If we take the highest possible degree of life, as manifested in Christ, we may understand that there is no absolute distinction between the material and sensible humanity and the supersensuous principle." (p. 145.)

We most cordially accept the following declaration of the possession of the best founded principles of belief:—

"I am unhappy if I give the readers of these pages any wrong impression of my opinion in these matters. Far be it from me to wish to degrade any sacred and sublime mystery into a mere philosophical abstraction; and though I would hold forth as a truth essential to the interpretation of the phenomena of vitality, the fallen position of man, yet I would never forget that the Godhead of Christ is absolute and infinite, and that to this, our own being, though congeneric in essence, can never hold a higher relation than that of finite to infinite, or of created to Creator." (p. 147.)

As in the writings of Professor Owen, so in Dr. Radcliffe's work, we find, as we have shown, an avowal of a believing submission to the sacred record of creative power, in all points that are beyond the reach of finite human knowledge;—and in this subject we would claim for the writings of our countrymen a higher character of simplicity

and intelligibility than is possessed by those of our continental brethren. Dr. Radcliffe's explanation (above quoted) is certainly more intelligible than the strange and complicated formulæ of Oken, the most striking characteristics of which are their obscurity and irreverence. We may here cite a few short passages in support of our position. Oken, in his "Elements of Physico-Philosophy," states—

"God is a rotating globe." . . . "The world is God rotating." . . . "God is = + O —. Man = + ∞ O — ∞. The animal = + m N — n."

This may be, physico-philosophy; but it strikes us as very like what, by an inspired writer, was termed "philosophy falsely so called."

We can truly commend Dr. Radcliffe's essay as full of interest, sound in its inferences, and calculated to enlarge our ideas of the vastness and simplicity of the schema of creation, while at the same time it tends to increase our reverent admiration of the Omnipotence and Omniscience which, amidst such apparent incongruity, has established harmony, and has so marvellously combined unity of plan with endless variety of details.

On the Management of Infancy; with Remarks on the Influence of Diet and Regimen, &c. By CHARLES HOGG, M.R.C.S., L.A.C., &c. 8vo. pp. 132. London: Churchill. 1850.

ALTHOUGH the observations contained in this work must be admitted to be judicious, and calculated to be useful to junior practitioners, it certainly has but small claims to originality: in fact, it consists, to a very considerable extent, of extracts from numerous writers. In addition to the management, diet, and regimen of infants, the subjects of cutaneous eruptions, struma, and hepatic disorders, with their treatment, occupy more than half of the volume. In looking over the cases we do not perceive that their treatment varies much from that which the recognised principles of medicine would dictate. From the style of the preface, and of the work itself, we should infer that this work is rather intended to meet the public than the professional eye.

* Translated for the Bay Society, pp. 132-133.

Address to a Medical Student. Second Edition. 16mo. pp. 107. Oxford: Parker. London: Churchill, 1880.

It is not long since we had the gratification of bringing under the notice of our readers part of a series of Biographies of Eminent Christian Physicians, edited by Dr. Greenhill, in which the truth of the adage, "example is better than precept," was strongly corroborated. This proverb, however true, does not lead to the inference that precepts are unnecessary. That the reverse is the fact is sufficiently attested by the copious body of precepts composed and collected by the wisest of men, who taught therein that "He that hearkeneth unto counsel is wise" (Proverbs xii. 15). Having, then, on that occasion spoken of examples, we now take occasion to draw attention to this small volume of "precepts."

The "Address to a Medical Student," which is deservedly dedicated to Dr. P. M. Latham, is published at an opportune time, although the first of October for 1880 has passed away. The term of *Studentship* has only just commenced to many, and been renewed to others with the present season. The first of October has been called, by a popular writer, "a great day with the doctors;" but that writer does not tell his readers how great it is, and that, in truth, it is really very little greater than any other day which follows it to all who undertake the responsibilities, the trials, and the anxieties of a medical life. The Address before us does this in the clearest manner; and, in the most earnest words, it reminds him that, whether he will hear or whether he will forbear, the medical student enters upon no trifling or easy undertaking at this time. This little book sets before him "a plain and simple view of the profession upon which he is entering, its difficulties and its dangers, no less than its pleasures and its advantages, and reminds him by WHOSE aid alone he can be enabled to keep himself unhurt by the temptations which will assail him."

We would advise attentive perusal of the introductory observations, instead of a cursory glance thereat, according to common fashion. The second chapter treats of the "choosing a profession,"—one of the most important actions in a young man's life. Under this head the author offers suggestions to assist the student in answering the only ques-

tion that he should put to himself with reference thereto—"Is this the state of life unto which it has pleased God to call me?" It would be better for the profession if this question were deliberately weighed by students in every aspect in which the author places it. There would be less of disappointment, and a more conscientious performance of our obligations, if it were so. If entered upon in the proper spirit, and under the influence of a conviction that the physician should regard himself as God's servant, having before him the example of our Saviour, who, when in the "form of a servant," took the physician's character,—if sincerely entered upon, and carried out with this feeling, "how welcome would be the toil, the carefulness, the want of ease, the broken rest, of the doctor's life!"

The following passage in reference to particular branches of the profession deserves attention in these days of class contentions:—

"I would only beg you to remember, that, though the physician is by law at the head of the profession, yet, in God's sight, he enjoys no such superiority of rank; for each branch of the profession is equally useful and necessary to mankind; and we may believe that in His eyes there is no distinction, except 'between the righteous and the wicked, between him that serveth God, and him that serveth *Him* not.' In fact, both in a scientific and a religious point of view, it seems to me that a *perfect* specimen of a general practitioner would be the noblest member of the whole profession. In a scientific point of view, from the fact of their combining both medical and surgical practice, and from other circumstances, the members of this branch of the profession enjoy several advantages over both physicians and surgeons, which I suppose nothing but their (in too many instances) inferior education prevents their turning to better account."

There can be but little difference of opinion on this view of the case, except from the difficulty in the present day of defining a "*general practitioner*." We must quote also the following passage, to which we would draw the especial notice of practitioners as well of students, as, from our experimental knowledge, we cordially testify to its truthfulness:—

"When you have determined on the branch of the profession to which you will belong, you may perhaps have another im-

portant question, to decide—viz. whether your medical career shall be exclusively practical, or whether it shall be entirely scientific or literary (but still professional), or whether you shall endeavour to combine in some measure both these characters . . . only I would say, that, as our intellectual faculties ought to be cultivated to the utmost extent, *that man seems to me to be most highly favoured by God who is allowed to find time not only for active employment in healing disease, but also for the cultivation of his own mind by study; perhaps, too, for imparting the results of his learning and experience to the young, or for writing some work that may be useful to his own or to future generations; or, in other words, that he approaches more nearly to the idea of a perfect physician who combines literary or scientific knowledge with practical skill, than he who gives himself up to the exclusive pursuit of either.*"*

With a reasonable amount of energy, and a will so to do, the way may in every case be found to create the combination here spoken of. The busiest man has spare minutes and quarters of hours during his rounds, or in his leisure, during which he may pursue his own or others' literary improvement if he be so determined.

The hospital, the dissecting-room, the lecture-room, daily life, and the examination, each furnish topics of exhortation, advice, and of encouragement from Dr. Greenhill to the medical student. Not only, we may observe, to the student, but also to the advanced practitioner, who may here hold up the mirror to himself, and view therein his past shortcomings, and from the self-reproachful retrospect derive lessons of future improvement.

This little work is indeed a compendium of the best advice, most judicious caution, most excellent inducements, and highest motives. The author's only object "has been so to direct the studies of the younger members of the profession as to make them Christ's true servants—is short, *good* physicians, in the highest and best sense of the words."

In concluding our notice of this interesting and eloquent address, we would here state that Dr. Greenhill has, at his own cost, undertaken the editing of a series of works touching the ethical and religious relationship of the profession; and we cannot but hope that, for the

honour of God, for the sake of the charities to which the profits of their sale are appropriated, for the sake of Dr. Greenhill's own resources, which alone meet the expenses and risk of publication, and for the lasting benefit of our profession, that these works will be sold extensively and studied diligently. Lastly, we trust that we may ere long have to announce the publication of a complete code of medical ethics from the pen of one so able to compose it in conformity with the morality of Scripture and the requirements of the profession, as Dr. Greenhill.

Instruction in Chemical Analysis (Qualitative). By Dr. C. F. GREENHILL. Edited by J. L. BULLOCK, Fellow of the Chemical Society, &c. 3d edition. pp. 384, 8vo. London: Churchill. 1860.

THAT this excellent treatise on Qualitative Analysis has already reached a third edition is a sufficient proof of its merits, and furnishes a striking indication of the extent to which practical chemistry is now cultivated in England. This may be regarded as the prototype of the greater number of the smaller essays on the same subject which have of late years issued from the press. It is a treatise complete in itself, and with which it is impossible to dispense in a working laboratory. The descriptions of the chemical processes are clear and precise, and such as may be easily mastered by a student of ordinary industry and capacity.

We observe that, since the first edition, which was noticed in this journal, there have been numerous additions, and the size of the work has been increased by fifty pages; in fact, both author and editor have striven to make it as perfect as the present state of science will admit. Comparative tables of foreign weights and measures might be added with advantage in an appendix.

Of the second part of this work, *Quantitative Analysis*, we are unable to express an opinion, as we have not seen it; but we can strongly recommend the part before us, *Qualitative Analysis*, as a most serviceable and safe guide to the chemical student.

* The italics are our own.

Elements of Chemistry, including the Application of the Sciences to the Arts. By THOMAS GRAHAM, F.R.S., L. & E., &c. 2d edition. Part IV. London: Baillière. 1852.

We are glad to be able to announce the completion of the first volume of this excellent treatise on chemistry. The fourth part is now before us. It contains the chemical history of the *Alkali-genuous metals* and their compounds, with an account of the metallic bases of the *Earths*. The salts of these bases are duly noticed, and the mode of preparing those which are manufactured on a large scale duly illustrated by wood-engravings. We have examined this part, and find that it contains what might have been anticipated from the well-known reputation of the author—a lucid account of the most important improvements and discoveries which have been made in the science since the publication of the first edition. We must also observe that the volume now completed is an admirable specimen of typography, and creditable to printer and publisher for the care with which it has been got up.

Des Kramphaste Asthma der Erwachsenen. Von Dr. J. BERGSON. 8vo. pp. 142. Nordhausen: Bücking. 1850.

On the Spasmodic Asthma of Adults. By Dr. BERGSON.

In the year 1845, the Royal Society of Sciences at Göttingen announced the above subject for the prize which they offered. Dr. Bergson directed his attention to its consideration, and obtained the award for the essay now before us, the contents of which we here briefly enumerate.

The author introduces his investigations with an extensive historical sketch, with true German industry and lore bringing under notice the opinions of every author of importance from the earliest ages of medicine down to the present time. In the next place, Dr. Bergson inquires into the real nature of spasmodic asthma, and concludes that it is a distinct affection, separate from other forms of dyspnoea with which it has been confounded by many modern writers, and that it is an essentially nervous disorder. In order to establish this point, the author draws a parallel between this and other nervous in the several aspects

of ætiology, prognosis, and therapeutics: in this portion of his treatise the author touches upon the essential point of the inquiry, and fully describes the diagnosis of the disease.

The course of the disease is then traced by the author, and its anatomical characters are described. It is shown that the muscular fibres of the bronchi, and the motor influence of the vagus nerve, are in abnormal conditions, that the disease thence consists in a spasm of the muscular apparatus of the lungs.

On the view thus taken by the author, he divides spasmodic asthma, with reference to its causes, into cerebral and spinal, and the latter into centric and eccentric. The work concludes with a very full diagnosis of spasmodic asthma from other diseases of the chest. This is arranged in a tabular form, the characteristic symptoms of the affections in each case being arranged side by side, so as to present their prominent features in obvious juxtaposition.

We can speak highly of this essay, and can recommend it to every reader of German medical literature.

The Brain the sole Centre of the Human Nervous System. By EDWIN LEE, Member of the Principal European Medical and Chirurgial Societies. Pamphlet, 8vo. pp. 14. Edinburgh. 1848.

In this paper the author endeavours to establish the position that the conclusions drawn by Dr. Marshall Hall and others, from their experiments on cold-blooded animals, are applicable only to those classes, and—by quotations from various authors compared with and added to his own observations—to prove that the phenomena of the spinal system, even when manifested separately from the cerebral influence, are owing to the persistence of that influence in the fibres of the cord. In his view the grey matter of the cord and sympathetic ganglia are reservoirs of power, and the brain alone is the nervous centre, controlling and modifying the functions of other parts of the system. Such is an outline of the contents of this pamphlet, to which we must refer our readers for the reasons adduced by the author in support of his views.

A Manual of Qualitative Analysis. By ROBERT GALLOWAY, F.C.S. Small 8vo. pp. 119. London: Churchill. 1850.

MR. GALLOWAY was formerly Chemical Assistant in the Museum of Practical Geology. Although there was no deficiency of works on practical chemistry, he considered that the majority of them were better adapted for the advanced student than for the beginner. With this view he has prepared a compendium of the general rules to be pursued in the qualitative analysis of different substances. Such is the history of the book before us. We have looked over these rules, and find them to be adapted for the purpose intended. They will guide the beginner, and lead him to look upon larger treatises with less fear of mastering their contents. The essay is strictly confined to practical manipulation in the laboratory.

The Physiology of Digestion. By ANDREW COMBE, M.D., &c. Ninth Edition. Edited by JAMES COXE, M.D. 8vo., pp. 191. Edinburgh: MacLachlan. London: Simpkin and Co. 1849.

THE publication of a ninth edition, as well as the sale of more than 16,000 copies of this work within ten years, is sufficient to prove that it is well known to, and, we believe, extensively read, both by the public and the profession. The present edition, by the author's nephew, has been carefully revised, and additions have been made where the present state of science had rendered them necessary.

A Treatise on the Gout; its Nature and Treatment. By H. B. C. HILLIER, M.D. Pamphlet, 8vo. pp. 23. London: Longmans.

DR. HILLIER'S views may be gathered from the following extracts relative to gout:—

"It is hereditary: it does not occur till after puberty: it attacks men oftener than women: it seldom attacks those who live on vegetable diet: it appears to exist in persons of a peculiar diathesis, which diathesis appears to be altered after every fit of the disease: it is seldom fatal: it is assimilated with rheumatism: the treatment which relieves the one does not cure the other; the pain which characterises this disease is peculiar to the gout: it is not met with in every climate." (p. 15).

"I am not inclined to believe that the cause of this disease is in the blood alone, but that it is engendered by and dependent upon unhealthy chyme." (p. 19).

The treatment founded upon these views consists in the use of "the mixed alkalies combined with magnesia and henbane, in frequently repeated doses." (p. 20.)

The subject is treated very fully, and the author displays considerable acquaintance with the views of ancient medical writers.

The Eighteenth Annual Report of the Trustees of the Perkins Institution, and Massachusetts Institution for the Blind. Pamphlet. 8vo. 1850.

THIS Report constitutes a valuable document relative to the management and instruction of the blind. From some experience in the direction of a similar institution, we can speak highly of the remarks and suggestions of Dr. Howe, the writer of the Report. Beyond these considerations, however, the pamphlet before us has deep interest in the subsequent narrative, by Dr. Howe, of the case of Laura Bridgman, whose history has gained a world-wide celebrity from the graphic pens of Washington Irving and Charles Dickens. The Report is no mere record of dry matter-of-fact details, but a stirring and instructive appeal on behalf of the most helpless of our afflicted brethren, which we hope may not be without its answer both on that and on this side of the Atlantic.

The Baths of Rhenish Germany: with Notices of adjacent Towns. By EDWIN LEE. 8vo. pp. 134. London: Churchill. 1850.

THIS work consists of an abridgment of a portion of the author's larger work on "The Baths of Germany." Mr. Lee has been induced to publish this smaller volume, "in accordance with the opinion of the foreign booksellers, for the accommodation of visitors to the Rhine." It will be found useful to those for whom it is intended.

Pestilence; its Source and Suppression. Pamphlet. 8vo. pp. 23. Isle of Man: Quiggin. London: Simpkin and Co. 1850.

THIS medley of scientific words, and extracts from Holy Writ is utterly unintelligible to us.

Proceedings of Societies.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Nov. 12,

Dr. ADDISON, PRESIDENT, in the Chair.

History of a Successful Case of Ovariotomy; by W. E. DUFFIN, Surgeon. With a Description of the Morbid Anatomy of the Sac; by ROBERT LEE, M.D., F.R.S.

A WOMAN, 38 years of age, having her abdomen enlarged to the size of the eighth month of pregnancy, this size having been attained in 7 or 8 months, her general health appearing to be good, applied to the author for relief by operation. The rapid growth of the tumor, which appeared to be connected with the left ovary, had been accompanied by neuralgic pains in the right thigh, in consequence of pressure on the sciatic nerve of that side. The tumor was very moveable, indicating the absence of adhesions. The patient was very urgent in her desire for the operation, and very confident that it would be successful, and it was consequently performed, on the 27th of August last, by the author, assisted by Messrs. Fergusson, Ure, and Henry Smith, chloroform having been previously administered by Dr. Snow. The incision, at first of only sufficient size to admit the forefinger for exploration, afterwards enlarged to three inches, was made in the linea alba, midway between the umbilicus and the pubes. The absence of adhesions having been ascertained, the sac was punctured by a trocar, and 130 ounces of a viscid, ropy fluid, of a light brown colour, were let out. The collapsed cyst, containing a smaller cyst the size of an orange, was drawn through the incision, and its pedicle secured by ligatures. The tumor was then separated, and the pedicle and ligatures were prevented from receding into the cavity of the abdomen by other ligatures attaching them to the wound. The ligatures came away on the 15th day, and the wound was healed on the 22d day, the abdomen resuming its natural shape and size.

The patient was kept on a light diet, took opium in sufficient doses for six successive nights, and the bowels were opened by enemata. On the 18th day she was able to get about a little. Her recovery has been complete, and she has been enabled to resume the occupation of dressmaker.

Dissection of the Cyst, by Robert Lee,

M.D., F.R.S.—The walls of the cyst are composed of three distinct coats or layers. First, a peritoneal covering; secondly, a middle fibrous coat; and thirdly, a dense membranous sac, in which the fluid was contained. In the divided pedicle are seen the cut ends of three large arteries, and of one large vein, and of the fallopian tube. The peritoneal coat is thin, and loosely attached at the root of the cyst; but beyond this it is thick, opaque, and firmly adherent to the tissues below. The middle coat is thick at the root, and contains numerous branches of arteries, and of nerves with ganglionic enlargements. It becomes thinner as it is expanded over the cyst, is of a dense fibrous structure, and adheres firmly by both its surfaces. The internal membrane is firm and thick, and its inner surface is rough, irregularly puckered, and in some spots of a brown or yellowish colour. It consists of two distinct layers, like the coats of a Graafian vesicle. A smaller cyst is situated near the root of the larger, imbedded in its middle coat, projecting into its cavity, and invested by its lining membrane, which it has pushed before it. The lining membrane of this smaller cyst also consists of two distinct layers, like that of the large cyst.

A group of small multilocular cysts is contained in the middle fibrous coat of the great cyst, and between the outer surface of the smaller cyst and the peritoneum. They have the same structure, contain a similar fluid, and bear the same relation to one another, as the two cysts above described. The author remarks, that "the walls of this ovarian cyst contain all the elementary structures which enter into the composition of the human ovary in the healthy condition—peritoneum, steoma, and Graafian vesicles, with blood-vessels and ganglionic nerves. Whether all multilocular cysts are formed in the same manner, future observation must determine."

An Analysis of 108 Cases of Ovariotomy which have occurred in Great Britain.

By ROBERT LEE, M.D., F.R.S., Fellow of the Royal College of Physicians, Physician to the British Lying-in Hospital, Physician Accoucheur to the St. Marylebone Infirmary, and Lecturer on Midwifery, and the Diseases of Women and Children, at St. George's Hospital.

The author commences with the history of a case of ovarian dropsy successfully removed by a surgical operation, and reported in the Edinburgh Medical and Surgical Journal, for 1822, by Dr. Nathan Smith, Professor of Physic and Surgery in Yale College, Connecticut. He then gives an account of an operation performed by Mr. John Lizars, of Edinburgh, in 1823,

when an ovarian disease was found after the abdomen had been opened; and of three other operations by the same surgeon, when the diagnosis was more accurate, in 1826 and 1826; and of two operations by Dr. Granville, in 1826 and 1827. He then refers to an operation performed, in 1833, by Mr. Jeaffreson; and to three operations by Mr. King, in 1834 and 1834.

During the last 37 years the operation has been attempted or performed more than 130 times in Great Britain, of 106 cases of which authentic reports have been received. In 37 cases either no ovarian cyst or tumor to remove existed, or there were present ovarian cysts and tumors, the removal of which was found to be impracticable, and the operation was abandoned. Of these 37 cases 14 died. The analysis given of 106 cases of ovariectomy shows that in 37, or about one-third of the whole number, it was impossible to determine *previously* whether ovarian disease actually existed, or, when it did exist, whether its removal was practicable. Of the 71 cases in which ovarian cysts and tumors were removed, 24 proved fatal, in 14 of which the operation could not be completed.

In the tabular analysis of the 106 cases appended to the paper, it appears that 45 cases are reported by Dr. Clay, and 11 by Mr. Lane; and to both of these gentlemen the author expresses his thanks for the manner in which they have communicated to him the entire results of their experience of the operation.

PATHOLOGICAL SOCIETY OF LONDON.

Nov. 5, 1850.

DR. LATEAM, PRESIDENT.

MR. COULSON exhibited a heart taken from a man aged 50, who had suffered for many years from chronic bronchitis and dyspnoea. The dyspnoea became very urgent for 30 hours before he died. On examination there was general serous infiltration into the tissues, not amounting to anasarca, considerable effusion into the right pleural cavity, and the left pleura pulmonalis was intensely adherent. The pericardium contained 3 oz. of sanguineo-serous fluid; the liver was congested, and the kidneys large and mottled. The weight of the heart was 28 oz. avoirdupois, and the aortic valves were very extensively diseased, one of them being partly separated from its attachments, so as to allow a portion of the curtain to be retroverted. The other valves

were thickened, and studded with vegetations, and a large part of the free fold of the mitral valve was covered by a thick and irregular layer of recent lymph. The left ventricle was greatly hypertrophied and dilated.

DR. PRACOCK remarked, that the heart exhibited afforded a very characteristic example of obstructive and regurgitant aortic valvular disease. The sigmoid valves were all much thickened and indurated, and must thus, for a long time, have formed an obstruction to the flow of blood from the ventricle into the aorta. When the attachments of the right valve, which was the most extensively diseased, gave way, and allowed its curtain to be retroverted, an aperture had been created, by which, during the diastole of the heart, the blood must have freely regurgitated from the aorta into the ventricle. To the operation of these two causes the large amount of hypertrophy and dilatation of the left ventricle was to be ascribed. The patch of lymph on the free fold of the mitral valve lay directly in the course of the regurgitant current, and was probably a simple deposition from the blood. The specimen further afforded an example of what had been termed "aneurism of the valves." In the curtain of the posterior sigmoid valve there was found a small sac, which opened into the corresponding sinus of Valsalva; and, again, in the free fold of the mitral valve, there was a similar, though smaller sac, which communicated with the ventricle.

MR. TOYNER exhibited a specimen of *Neuroma of the Auditory Nerve,*

which was removed from a man aged 60. The only symptom indicative of its presence during life was a diminution of the power of hearing. The disease consists of a white tumor, about the size of a small bean, which occupies the internal auditory meatus, and projects about a line beyond its posterior border. The shape of the tumor is conical, the small extremity being situated at the anterior part of the meatus: its texture is firm posteriorly; and, upon being cut into, presents but little cellular tissue: anteriorly it is softer. The tumor is attached to a portion of the auditory nerve supplying the vestibule, the fibres of which enter its middle part; and they emerge at its anterior extremity, entering the vestibule with filaments from the healthy portion of the nerve. The nerve supplying the cochlea is unaffected by the tumor; the portio dura lies on its upper surface. Upon examination with the microscope, the tumor, at its anterior part, was found to be composed of distinct nerve tubes, which assumed the varicose form

upon the slightest pressure, and of others about the same size, but more opaque, presenting in their walls distinct nucleated cells, and having the appearance of nerve tubes in the process of development, as delineated by Schwann; these two kinds of nerve-tubes were about equally distributed through the anterior part of the tumor, and between them rounded cells were interspersed. The posterior part of the tumor was observed to consist of fibres running parallel to each other: they are not so large as the tubes in the anterior part; some of them present distinct nucleated cells.

Mr. CANTON exhibited a specimen of
Ossification of the Pericardium.

Master E. B—, *et. 11 years*, in Feb. 1844, suffered from a severe attack of acute rheumatism. A fortnight after its commencement pericarditis supervened, and was treated by leeching, blisters, mercury to ptyalism, colchicum, &c. The following May the medical attendance ceased. Auscultation gave evidence of fibrinous deposit on the pericardium, and there was turbulent cardiac action, with dyspnea. In all other respects he enjoyed tolerable health until November 1845, when pain in the region of the heart required antiphlogistic treatment and blistering for a short time. In April 1846 he became the subject of a severe attack of hamoptysis, which, being subdued, was followed by ascites: this latter was with difficulty removed by diuretics in June. In June 1847 ascites reappeared, but was again removed by the same plan of treatment. In September 1848 he was again dropsical, and continued so for several weeks. The ascites recurred at the close of 1849, accompanied by a severe attack of hamoptysis, which for several days threatened his existence, but was ultimately stayed by gallic acid. His sufferings during the last three years had been very severe, and from this date (Nov. 1849) were still augmented, and obliged him to pass several successive nights supported in a chair. There was great abdominal distension, attended at times with severe pain in the epigastric region; oedema of the lower extremities and of the scrotum. A more formidable attack of hamoptysis again took place in July, and lasted ten days. It now became manifest that the long-continued sufferings of the patient were drawing to a close: the dyspnea became painfully urgent; the ascites was no longer amenable to treatment; and the violent action of the heart prevented him sleeping. Repeated auscultation led to the conclusion that extensive adhesion of the pericardium was present, with hypertrophy of the heart: some disease of the valves

was suspected. During the whole of his illness the pulse maintained a strong, full, and rapid beat, and lessened the suspicion of valvular disease. The carotid and temporal arteries pulsated violently: a dry cough repeatedly harassed him, especially towards the close of his existence.

Notwithstanding his disease and suffering, he grew in stature, and was at the time of his death six feet in height. He died rather suddenly on the 11th September.

Examination.—The anterior mediastinum was filled with old tenacious lymph; the pericardium firmly united to the heart; and in the adhesions, bony matter, forming a ring, was deposited, encircling the organ between the auricles and ventricles; the ring, in the chief part of its circumference, was an inch in breadth; the heart much enlarged; valves on the right side healthy, on the left somewhat thickened.

Abdomen.—The whole surface of the peritoneum overlaid with firmly adherent lymph, obscuring and agglutinating the viscera. Liver enlarged, and kidneys healthy; ascites to a small extent, with a little recent fibrin on the small intestines.

Dr. O. WARD exhibited a

Portion of a Gall-Bladder discharged externally.

Miss T., *et. 48*, has had very severe cough and purulent expectoration, attended with signs of a cavity in the left lung, and other symptoms of phthisis, for more than 20 years. About eight years ago she had an attack of vomiting and diarrhoea, with faintness, when a tumor was discovered on the right side of the abdomen, but apparently unconnected with the liver. As it increased in size the tumor passed more into the centre of the abdomen, leaving a marked sulcus between it and the liver. For a twelvemonth it had descended towards the right iliac region, when it pointed and burst, in April last, a little lower than the edge of the transversalis muscle, and midway between the spine of the ilium and the pubes. The discharge at first was very offensive pus, followed in a few days by numbers of hydatids of various sizes, from a pea to a turkey's egg, and some very minute ones in clusters, with pedicles like a bunch of grapes, and striated externally. In about five weeks the hydatids were all expelled, and the pus became healthy and less offensive. The opening continued to discharge till August 11th, when she had another bilious attack, and the discharge ceased; the abdomen became tense and tender over the right side, with occasional rigors. On the 15th the orifice reopened, discharging pure bile, which continued to flow, to the amount of about a pint a day, till the 24th, when a

alough came away, which, on examination, proved to be a portion of the gall-bladder, having small calcareous plates imbedded in it, similar to some that had passed occasionally from the wound. The bile has continued to pass, in greater or less quantity, almost uninterruptedly to the present time, when there is about a drachm passed in 24 hours. The patient's health has varied considerably, as she is sometimes able to go about the house, and, again, has been reduced so low as not to be able to rise up in bed without support.

Dr. GARROD exhibited, for Mr. DENNY, a
Piece of Bone and Teeth,

which were dissected from between two layers of membrane forming the walls of an ovarian cyst, taken from a girl after death.

Elisa F—, aged 22 years, single, was admitted at the Stoke Newington Dispensary, under Dr. Duesbury, August 9th, 1860, with dropsical swelling, distending so uniformly the abdominal parietes that it was difficult to diagnose whether it was ovarian disease or ascites. She complained of weakness, and inconvenience from the weight and size of the body. She stated that she had been ailing for three years and upwards, during which period she had suffered much from pain in the left side of the abdomen, but had not perceived her body enlarging more than two years; that it had increased much more rapidly of late. She attended at the Dispensary, and slightly improved in health under tonics and diuretics until a fortnight previous to her death, when she was attacked with peritonitis, and died on the 7th of October.

Mr. Price attended her, for a considerable period, for what he considered to be an ovarian tumor; and four months previous to her being admitted a patient at the Dispensary (in consequence of the lady with whom she lived as servant having suspicions of her being pregnant) he sent her to Dr. Lee, who pronounced the uterus to be impregnated, and confirmed the previous diagnosis as to the existence of an ovarian tumor.

On opening the abdomen, the left ovary was found to have been converted into a cyst, occupying the greater portion of that cavity, the viscera of which it was compressing in every direction. A few recently deposited threads of lymph extended from its anterior surface to the peritoneum lining the abdominal parietes; otherwise it was perfectly detached except by its peduncle, which was not longer than the little finger, and consisted merely of the obliterated fallopian tube, enlarged vessels, and peritoneum. The cyst, together with the uterus and its appendages,

were removed; and, on opening the cyst, from three to four gallons of a cream-coloured fluid escaped, suspended in which were numerous flakes of fatty matter: it also contained a large conglomerate mass, as large as the fetal head at six months, consisting of hair (4 to 5 inches long) and this fatty substance matted together, and many smaller pieces of the same substance.

The cyst was divided by transverse bands, and also into pouches, or smaller cysts opening into the common cavity. Adhering to the walls in places were considerable quantities of the fatty matter and attached hair, similar to that in the mass; and between two layers of the membrane forming the wall of the cyst on the left side was the specimen reduced. The teeth are in every respect perfect, with their fangs inserted into processes of the bone resembling the alveolar processes of the jaw.

The right ovary was rather larger than natural, congested, and contained several smaller cysts the size of a pea, filled with fatty matter.

The uterus presented the appearance in all respects of that of a virgin, and healthy, with the exception of slight congestion, which might be merely post-mortem.

Dr. Baillie mentions similar fatty matter, hair, bone, and the rudiments of teeth (but without fangs), having been found in ovarian tumors, and under circumstances leaving little doubt that they had been formed independently of impregnation, one case of which occurred in his own practice. Also a case published in the Philosophical Transactions, where this change in the ovary was found in a child whose age did not exceed 12 or 13 years, with the hymen perfect, the uterus not increased in bulk as is usual at puberty, together with the other signs of puberty wanting.

From the fact of the uterus being totally devoid of all those changes that follow impregnation, whether the fetus be developed within its cavity, or as an extra-uterine foetation within the ovary or fallopian tube, from the respectability of the girl's parents, together with the good character she herself had borne, and her anxious desire that her body should be opened for the satisfaction of her friends, in consequence of the suspicion that her appearance had excited, Mr. Denny was led to believe that in this case there had been no impregnation.

The consideration of this case suggests whether an ovum which the stimulus of the catamenial period caused to burst from the Graafian vesicle, and which was prevented from passing off with the menstrual discharge by the impermeability of the

fallopian tube, might not become adherent to the lining membrane of the ovary or fallopian tube, and thus obtain nourishment, setting in action a power, which may be inherent in it of developing the structures of the body, but in an incoherent manner, without the power to regulate the formation, or to produce a circulating system or nervous centre, necessary to the perfect development and independent life of the fetus. The latter power may emanate from the male, exerting an influence in some way analogous to the electric influence excited in crystallization, of attracting and arranging the particles, so as to give a definite form, and thus to characterize the individuality of the salt.

The statistical information furnished by the record of these cases, especially with evidences of existing virginity, may, by affording data for physiologists, enable them, at some future period, to throw light upon the functions of the ovary, and upon the elements supplied towards the formation, and the power exercised over the development of the fetus by the sexes individually, and may also prove of practical importance in removing the odium which a supposition of the necessity of impregnation might cast upon the fair fame of the innocent.

MEDICAL SOCIETY OF LONDON.

Nov. 9, 1850.

DR. J. R. BENNETT, PRESIDENT.

Mortality in Hospitals.

DR. HARE, of University College, made some observations on certain points of Dr. Webster's statement last week, which, without some explanation, might tend to mislead, and to give rise to very erroneous inferences. The subject alluded to was the statement with reference to the number of beds, and the relative mortality in the different metropolitan hospitals. Thus it was stated that the beds in University College Hospital amounted only to 106; while the fact was, that the beds were now, and had long been, 122. On the other hand, the number of beds in some of the other hospitals, as stated in Dr. Webster's paper, was overrated. Dr. Hare then proceeded to show that the number of beds in an hospital was no certain criterion of the number of beds occupied by patients, and based his arguments upon a paper published by the Registrar-general on the 29th of June last. An account was there given of the number of patients in the different public institutions on March 31, 1850; and this was the most recent authentic account

which had been published. It appeared, then, that on that day the Royal Free Hospital, which, in Dr. Webster's Report, was put down as having 140 beds, had only 52 inmates; that King's College Hospital, which was put down for 120 beds, had only 96 inmates; while University College Hospital, which, according to the Report, only contains 106 beds, had actually 118 inmates. A short time since, every bed in the hospital was full (122); and on inquiry that day (Nov. 9), Dr. Hare found that the number of in-patients was 112. He then pointed out that, supposing the table had been quite correct as to the number of beds, or even the number of beds occupied—nay, even the number of admissions—in each hospital, still no conclusions or inferences whatever should be drawn from the relative amount of mortality they present, seeing that that is constantly fluctuating, and that fortuitous circumstances so much influence the result. If, however, the number of deaths be compared with the average number of patients in the hospitals (which is a somewhat more accurate method than that of taking the number of beds, even if these be correctly ascertained), it would appear, according to the Registrar-general's document, already referred to, that instead of having the largest percentage of mortality, as stated in Dr. Webster's paper, the mortality in University College Hospital was actually less than the average; for while, in the document alluded to, the average annual mortality of all the London general hospitals was 82 per cent., that of University College Hospital was only 79 per cent. Dr. Hare entered into some further details illustrative of the subject, and pointed out how the much larger relative mortality, mentioned by the Registrar-general as occurring in the London Hospital, and in King's College Hospital (116 per cent. in the former, and 158 per cent. in the latter), might be accounted for without supposing that there occurred in these institutions any larger number of deaths, relatively to the severity of the cases admitted, than in any other hospital of London.

Dr. SIMSON made some remarks with the view of showing that the actual number of patients in an hospital, and the actual number of deaths, did not convey in all cases a fair estimate of the mortality of the institution, when compared with others. Thus the previous condition of the patients, the situation of the hospital, the nature of the maladies under which the inmates laboured, and other elements of inquiry, were necessary before a right conclusion could be arrived at.

Dr. WEBSTER, after stating that he had availed himself of every possible means of

making his inquiries accurate, said that he should be most happy to have any of his statistics corrected. With reference to the number of beds for patients in University College Hospital, he could find no published statement, and he believed there was none. He had, however, the written authority of a medical officer of that institution for stating that "there are 106 beds laid out at University College Hospital," and Dr. Hare now acknowledged the mortality was correctly reported. Respecting the other subjects alluded to by previous speakers, as they formed no part of his (Dr. Webster's) paper, or of the abstract recently published, it seemed unnecessary to advert thereon, or to the quotations just read from the Registrar-general's Report bearing upon the mortality in hospitals, although such remarks were of the highest value, and the opinions now expressed deserved respect. The discussion that evening, however, clearly indicated the advantage of having regular statements from all the metropolitan hospitals, and how useful they would be to the profession, as he (Dr. Webster) had suggested in his communication.

SURGICAL SOCIETY OF PARIS.

Nov. 4, 1886.

Serious and multiple Injuries occurring to an Old Man—Amputation of the Arm—Ligature of the Axillary Artery—Cure.

A MAN, aged 70 years, fell from a heavily laden vehicle, one of the wheels of which passing over him, produced fractures of both thigh-bones, comminuted fracture of the radius on the right side, and compound fracture of the humerus. Amputation was considered requisite. M. CHASSAIGNAC performed it at the level of the insertion of the deltoid. An external flap was formed, so that the extent of injury to the bone could be ascertained. Secondary hæmorrhage occurred repeatedly, and rendered ligature of the axillary artery indispensable: this was effected beneath the clavicle.

With reference to a discussion which had taken place in the Society on the question whether the ligature, in becoming separated from an artery, carried with it a portion of its structure, M. CHASSAIGNAC stated that he had directed his observation to this point in the present instance. On the fifteenth day the ligature came away, carrying with it the debris of a portion of artery, equal in length to the width of the ligature, presenting the calibre of the artery and the two cup-like depressions on each

side of the constriction. This fact, M. CHASSAIGNAC observed, is exceptional, as ordinarily, when a portion of the vessel comes off with the ligature, it is extremely small, and consists solely of the portion compressed by the ligature. In the present instance the ligature had been double.

M. LENOX considered the result here stated as unfavourable, and attended with risk of hæmorrhage. He entertained no doubt but that the portion of artery could always be found with the ligature if properly sought for.

M. HUGUET had observed, that with a single ligature, the internal and middle coat having sloughed away, and the external coat having divided, no remains of the vessel comes away with a ligature.

MM. MAISONNEUVE and LARREY concurred in the last opinion.

M. CHASSAIGNAC replied that he had merely stated this fact as having occurred to him, irrespective of any theoretical views; it was an exceptional case; he had never, in other cases of ligature of large vessels, found any trace of their structure; and he suggested it as probable that the age of the patient might have contributed to this unusual result.

A report was read on M. Blot's Essay on the Albuminuria in Pregnant Women. (We have elsewhere given an abstract of this essay.)

Painful Tumor of the Face.

M. CHASSAIGNAC presented a patient, a man 30 years of age, of a good constitution, in whom a tumor had been developed on the inferior maxilla, in the site of, and consequent upon the extraction of a molar tooth. It had attained the size of a hen's egg, and was productive of severe lancinating pains. The skin over it was unaltered; to the feel it was elastic, and presented a doubtful sense of fluctuation: it was adherent to the bone, and an explorative puncture had proved it to be solid. M. CHASSAIGNAC regarded this growth as being of a cancerous nature, but hesitated to remove it, as he feared that the bone itself might be diseased.

M. FOREST was disposed to regard the tumor as of a fibrous character, and suggested its removal, with a portion of the bone, to secure against its return.

APOTHECARIES' HALL.

NAMES of gentlemen who passed their examination in the science and practice of medicine, and received certificates to practice, on Thursday, 14th November, 1886:—William Draper, New Zealand.—Charles Hirst, Morley, Yorkshire.—George Willis, Florence Court, Ireland.

Medical Trials and Inquests.

COURT OF ASSIZES, DARMSTADT.

March 11th—April 11th, 1850.

Trial of John Stauff for the murder of the Countess of Goerlitz—Alleged spontaneous combustion of the body—Report of the Hessian Medical College on the case.

[We are indebted to an eminent Scotch medical jurist for the abstract of the following remarkable case.]

The abstract of the instructive trial which follows is taken from the two medico-legal periodicals whose titles will be found below.* The mode in which the crime had been gone about, which had nearly defeated the ends of justice, and even baffled suspicion, the number of scientific witnesses employed, and the nature of their investigations, embracing as they do the difficult and hitherto unsettled question of the so termed "spontaneous combustion" of the human body, will we think justify us in giving a pretty full outline of the particulars of this remarkable case, from the documents before us.

From the report of Dr. Graff, of Darmstadt, it appears that the late Countess of Goerlitz, *et. 46*, a person of active, industrious, and somewhat parsimonious habits, and in the enjoyment of good health, was frequently known to retire to her own apartments, and shut herself up there for half a day at a time. On the 13th of June, 1847, between 3 and 4 P.M., on her husband going out to dinner, she had retired to her room to arrange some household matters. Her servants had obtained permission to absent themselves for the evening, leaving only the prisoner Stauff in charge of the house. The Countess's private apartments consisted of an ante-chamber, and a parlour (*Wohnzimmer*) inside of this, into which opened a closet (*Kabinete*), only large enough to contain an ottoman (*divan*), on which she commonly took her siesta. In one corner of the parlour was a Russian stove, and in another her writing-desk (*schreibpult*). On his return in the evening the Count knocked at the door of the anteroom, but received no answer, when he went out again. This

was about seven o'clock by 9 A.M. became back. During his second absence a bright light, which speedily disappeared, had been noticed at the closet window, which looked to the south, and also a thick smoke from a chimney which corresponded with the stove in the parlour. At 9 P.M. the servants, who had returned, were sent in search of their mistress, but not obtaining entrance, workmen were sent for to break up the doors of the outer rooms, which were locked. The keys were not afterwards to be found. The smoke which issued from the rooms prevented them from being entered till one of the windows was forced. On this being done flames burst out simultaneously from the hangings, the writing-desk, and the floor underneath it. The dead body of the countess was found, a foot from the writing-desk, with the feet towards the middle of the room, and the head towards the window. There was no appearance of fire about it, or at the part of the inlaid wooden floor on which it rested. On its removal a few embers (*cinder*, = 15·12 imperial gallons) of water sufficed to extinguish the fire. On this being effected, a rush of smoke was noticed to issue from the open door of the closet, which was found to proceed from the ottoman being on fire. No other object in this apartment was touched, and in front of the ottoman lay one of the Countess's slippers uninjured.

When called in officially on the following morning (14th of June), Dr. Graff found the whole of the Countess's private apartments in the state of disorder usual after the extinction of a fire. The writing-desk had been mostly consumed, and the papers it had contained, partly burned, lay scattered about the room. A mirror on the parlour wall, 16 feet (15 f, 5·84 inch. Eng.) from the writing-desk, was cracked, and two stearine candles which had stood on a chiffonier, 9 feet (9 f, 3·23 inch. Eng.) from it were melted. The ottoman in the closet was displaced, and nearly in its centre was an almost oval hole, caused by the combustion of the hair mattress and stuffing.

On proceeding to view the body, Dr. Graff observed the remaining slipper on one of the feet, and uninjured. The dress on the upper part of the body was almost wholly consumed. The head exhibited the form of a nearly shapeless black mass, in which the mouth was imperfectly distinguishable, with the charred tongue protruding from it. The body lay on its left side, the head and chest retracted, the neck everywhere blackened and charred, as were the skin and muscles on the fore and upper parts of the chest, the former being thus affected to within an inch of the pit of the

* Henke's Zeitschrift für die Staatsarzneikunde, 2 V. H. 1850. Bd. 40. Ergänzungsheft. Bd. 3 V. H. 1850. Annales d'hygiène publique et de Médecine légale, 37 and 38 No. Juillet, Octobre, 1850.

stomach. The marks of the action of fire did not extend quite so far along the back part of the trunk. The joints of both upper extremities were flexed; and their surfaces charred, except at the hands. The left shoulder and the right elbow-joints were laid open. From the former of these the blackened humerus, and from the latter the heads of the radius and ulna, protruded. The skin in the vicinity of the left knee was slightly acted on. The body exhaled an empyreumatic odour. The further examination of it was objected to.

Considering (1) that the deceased had been in full bodily vigour; (2) that she had been seen by her servants the evening before in good health; (3) that she had not gone to bed, but must have been occupied at her writing-desk; (4) that in case of the fire having been accidental, she might have escaped from it, or at least have called for assistance, of which there was no indication, though she was close to a window; and (5), that as the traces of the fire and the carbonization of the body were chiefly about the head, and that the open mouth and protruded tongue were indicative of suffocation and impending asphyxia, Dr. Graff reports it as highly probable that this had been one of the rare instances of what is termed *spontaneous combustion*; a supposition, in his opinion, which alone could explain the circumstances that the deceased had been unable to call for assistance, or to save herself, as she must have done had the light on the writing-desk caught her hair or head-dress.

This opinion Dr. Graff so far qualified next day, by farther reporting that his conclusion in regard to the high probability of death by spontaneous combustion, in this instance, was an hypothesis only admissible in the absence of indications of violence on the body, the abstract possibility of which he was not prepared to deny, or that the burns, particularly those about the head and neck, might not have been resorted to to mask such violence; in which case the protrusion of the tongue, and the open mouth, would give support to the presumption of strangulation.

Report of Dr. Stegmayer, 27th December, 1847.—Dr. Stegmayer, who seems to have been the fatally medical attendant, could not say whether or not there had been anything in the mode of life, the constitution, or the usual state of health of the Countess, which would give support to the hypothesis of spontaneous combustion in her case. He had not himself observed her using spirituous liquors, nor witnessed anything which could lead him to conclude that she was addicted to the abuse of them. There was nothing in the state of the dead body, as seen by him, or as spoken

to by the servants, which could indicate that it had been exposed to the action of fire during life.

Report of Dr. von Siebold, 12th April, 1848.—In this document Dr. von Siebold seeks to corroborate the opinion previously adduced by him in his *visu vœce* examination, to the effect that the Countess had perished by spontaneous combustion. In proof of the occasional occurrence of death in this way, he points to the instances adduced by Kopp, Friedreich, Henke, and Devergie. He enumerates the leading features of the cases collected by Devergie in the article on "Spontaneous Human Combustion," in the *Diet. de Méd. et de Chir. Pract.*

These may be left out here, as they will be given below in our notice of Dr. Graff's second report. We may also here omit that portion of his summary of the facts elicited regarding the death of the Countess already given from Dr. Graff's first report, confining ourselves to a few points omitted in this last document.

He viewed the body (along with Dr. Stegmayer) at 11 P.M. on the night of the fire. The surface of the head and neck had a shining fatty appearance, as if covered with a coat of varnish. There were no marks of fire on the clothes anywhere beyond the margins of the burns on the body. On the 26th of November, 1847, it was intimated by the Count that an inquest would be held respecting the fate of the deceased. Next day the cook detected a quantity of greenish matter (*verdigris*) in a sauce intended for the Count. About the same time suspicion was awakened against Stauff, from the discovery of some jewels, which had belonged to the Countess, in the possession of one of his relatives at a distance. A few days after the fire several boxes full of charred matches were found below a small Russian stove in Stauff's room. The bell-pull in the Countess's room was torn away, and lay on the floor at the spot where it had been attached. When the body was found one of the knees was exposed, the lower part of the chemise and the other clothes being turned up. The writing-desk retained its shape; the top and sides of it had only partially suffered: the folding-board (*klappe*) and the drawers, from top to bottom, were entirely consumed, as was the floor beneath and in front of the writing-desk, over a space of a foot and a half. The mirror, and an oil-painting above a sofa in the parlour, were obscured by a coating of soft deep reddish matter, containing black points visible in it, with or without the aid of a lens. The feet of a chair placed near the writing-desk were slightly charred.

From the facts of the case Dr. V. Siebold considered that the burning of the Countess's body could not have been the consequence of accident. The combustion of the desk and the portion of the floor could not have done it; the corpse was found out of the reach of these, and on a part of the floor to which the fire had not extended. The doors and windows were closed, and no flames were observed till the room was forced open; so that the desk and floor were rather charred than burned. The disproportion, besides, between the extent of the burns on the body and the quantity of the combustibles consumed, was too great to allow him to admit that they had been caused by the burning of the furniture. He considered it equally impossible that the burning of the body had been the result of design, and the employment of additional combustible matter. After explaining away the facts which favour the supposition of design, he contends that no murderer could have chosen a mode of concealing his crime so difficult and complicated a kind, which demanded time for its execution, endangered his discovery while putting it in force, required the collection of abundant materials, was likely to leave its traces behind it, and which, in short, none but a madman in the pursuit of some fixed idea was capable of carrying successfully into effect.

He further thinks that the portion of the body burned could not have been introduced at once or separately into the stove; that the coating on the mirror showed that the combustion had taken place in the room; and that the state of the apartment indicated that no particular arrangements had been made for this purpose.

He contends, in addition, that spontaneous combustion alone can satisfactorily explain the circumstances of the case; such as the limitation of the burning to the upper part of the body and of the dress, while it had extended to the upper extremities; the coating on the mirror and the oil painting, and the appearance of the burned parts. Supposing that the combustion had begun at the head, and that the eyes had only been partially involved in it, the Countess might at the moment have been in a state to run to the bell-pull; and, in her agony, to pull it till it gave way. The flame seen from a house opposite the closet window at 8 o'clock showed that she had been lying on the ottoman when the head had taken fire, which accounted for the hole burned on it, and for the disfigurement of the mirror and the oil painting, as well as the finding of the slipper in this apartment. He conjectures that the Countess was in the act of run-

ning to the window to call for assistance when she had fallen before the writing-desk and set it on fire.

Report of H. Merck, 16th April, 1848.
—H. Merck carefully examined the brownish coating on the fragments of the mirror. On washing these with distilled water the liquid was brownish and clouded; with potash it gave out ammoniacal fumes: it was feebly acid: distillation, and the addition of concentrated sulphuric acid, showed that it contained acetic acid. The residuum was soluble in alcohol, and the alcoholic solution had a burned and fatty taste. Hence he concludes:—1. That the coating on the mirror had been produced by the slow and imperfect combustion (carbonisation) of organic matters. 2. That it contained ammonia and acetic acid. 3. That the substances from which it had been derived were of a vegetable and animal nature, but not necessarily derived from the combustion of the body of the Countess, since woollen stuffs, feathers, &c., might equally have yielded an ammoniacal product.

Reference to the Medical College of the Grand Duchy of Hesse, 21st July, 1848.*

—On the 23d of June, 1848, the following queries were forwarded to the members of this College for their decision—viz.:

1. Whether, and in how far, it could with certainty be determined either that the death of the Countess had been the result of spontaneous combustion, or that this mode of death was in any case admissible? And in case of a negative answer to both these questions:—

2. Whether, and in how far, grounds of probability existed in this case for or against the likelihood of this mode of death?

3. Whether, and in how far, the employment of the sauce drugged with verdigris, or a portion of it, would have endangered the life or health of the person partaking of it?

It was further intimated to them that, should the disinterment of the body be deemed by them necessary or desirable, in order to the elucidation of the first and second questions, leave should be asked for that purpose.

In reply, the Medical College refer to the annexed document by one of their colleagues, to whose conclusions they give their formal assent.

Report (2d) of Dr. Graff, 14th July, 1848.—After a few preliminary remarks, and a recapitulation of the principal facts already detailed regarding the death of the Countess, and the appearances which her

* The members of the College were Drs. Büchner, Hohenchild, Leidhecker, V. Siebold, Rieger, Graff, and H. Merck.

body presented at the time, Dr. Graff proceeds to compare these last with the results deduced from an analysis of the cases of spontaneous combustion recorded by authors, and especially with those collected in the "Encyclopädisches Wörterbuch der Medicinischen Wissenschaften," amounting in all to about forty cases. In these:—1. The proportion of females to males was as 4 to 1. 2. The parties were mostly of very advanced age, chiefly from 50 to 80 years, namely 59. 3. Their habits were most frequently sedentary and inactive. 4. They were in almost every instance stout and very fat. 5. Almost all had been for years addicted to the excessive use of spirituous liquors. 6. The combustion almost always immediately followed such excesses. 7. It always happened in the night-time, and in winter. 8. Calls for assistance were never heard from the parties. 9. In the majority of the cases, though not in all, there was some burning body in their vicinity. 10. The combustion was excessively rapid, occupying mostly but a few minutes, sometimes only a few seconds. 11. It was attended with flame, and hardly admitted of being extinguished by water. 12. Even very inflammable objects in the person's vicinity often escaped injury from it. 13 and 14. In every instance the combustion involved part of the trunk of the body; and, with few exceptions, this part was always converted into carbon (kohle) and ashes. 15. In the majority of the cases, portions of the head and limbs escaped the action of the fire. 16. The extremities were also severed at the joints, and were covered with vesications. 17. The combustion had always a fatal termination. 18. The charcoal (kohle) which was left mostly retained the form of the part burned, was very porous, and fell into powder on the slightest touch. 19. The ashes were almost constantly intermingled with a yellowish, oily, glutinous liquid, which also covered the floor, and gave out a penetrating empyreumatic odour. 20. The whole chamber was filled with a thick smoke, and the walls and furniture were invested with a dark soot.

Applying the criteria of death from spontaneous combustion thus obtained to the case of the Countess of Goerlitz, Dr. Graff finds that, excepting in the particulars noted above in §§ 1, 8, 19, and 20, the two present no points of agreement; that these points of resemblance besides refer only to generalities, some of which—such as those in §§ 19 and 20—prove nothing, and could all have been caused by the burning of the body under ordinary circumstances; that one other of the two remaining circumstances—that in § 8—

does not admit of satisfactory determination one way or another; and that, in this, particularly if we take into view the proposition in § 9, we have an absolute negative put upon the possibility of spontaneous combustion, in the failure of its most essential conditions, as well as, not only the highest probability, but likewise the absolute certainty that, in this instance, death had not happened from this cause.

Dr. Graff conceives Dr. V. Siebold's hypothesis, as to the train of circumstances attending the Countess's death, to be opposed to the facts stated under §§ 19, 14, and 15. The probabilities, he contends, were all against her having been able to ring for assistance, or to call from her parlour window. He cannot see how his suggestion that she might have rested her hand on the top of her writing-desk, could have set it on fire; or, if so, why the floor was not also kindled at the spot on which her body had fallen (vide § 11). He relates an instance, known to himself, in which the head of a corpse was reduced to a shapeless charred mass, from the accidental combustion of some wood-shavings, and the subsequent spread of the flames to the top of a table above them, on which the body rested, as a proof of the extent of injury which a dead body may suffer from the burning of a limited supply of combustible matter. He states that, although, when first called to view the Countess's body, he had hesitated between spontaneous combustion and murder, he cannot now admit the possibility of the former mode of death in her case, but considers it to have been undoubtedly the work of an assassin. He supposes that an inspection at the time would have shown that there had been not only strangulation in the present instance, but also fracture of the skull; and that even yet, provided the cranial bones had not been completely consumed, the verification of the previous existence of fractures or fissures was not impossible. On these grounds the disinterment of the body is advised.

In reply to the 3d question, Gr. Graff states that the sauce was found to contain 15½ grains of verdigris, a dose of from one to two grains of which was capable of producing dangerous symptoms, and four grains might cause fatal effects.

Disinterment and inspection of the body.

—The body of the Countess was inspected by Drs. Graff, Büchner, and Rieger, and Freniard, surgeon, on the 11th of August, 1848, after upwards of fourteen months' sepulture. It exhaled a moderately strong mouldy putrid odour.

The grave-clothes in contact with the body were moist: they were every where

stained of a more or less deep brown colour: larvae and pupae were found within their folds, and a countless host of small flies, both living and dead, covered most parts of them.

More than two-thirds of the cranial vault were entirely wanting, the greatest deficiency being at its upper and left side. The remaining portions of the skull appeared blackened and charred; but, on removing the blackened masses which adhered to it, the charring and discolouration was found to be but partial, the bones being firm, smooth, and giving out when struck their usual normal sound. A fissure, about half an inch in length, extended downwards from the upper border of the right temporal bone, which was otherwise nearly entire. The remains of the encephalon, in the form of a blackish-brown, half-softened, and adherent mass, of about the 6th or 7th part of its natural bulk, was found at the base of the skull. The lower jaw was in part deficient, and two and a half inches apart from the upper. The cervical vertebrae, the clavicles, and the ribs, were partly blackened, but their texture was unimpaired. The cavity of the chest contained a black fetid pulp, in which only a portion of the heart was distinguishable. The contents of the abdomen were in a putrid state. The right upper extremity had suffered more than the left. The head of the left humerus was charred, and part of it gone. The bones of the forearm were fractured, and portions of their surfaces charred, but their texture was otherwise unaltered, and they gave out their normal sound when struck. On both arms portions of the soft parts remained as a blackened greasy mass. The integuments of the abdomen and thighs had a leathery feel, and a cheesy-looking appearance. The femoral muscles were pretty fresh.

From the data thus obtained, the examiners concluded:—1. That the flames which had acted on the body of the Countess had been chiefly on the left side of it; and 2. In a direction mainly towards the head. 3. That the injuries it had suffered from the fire could not have been produced by spontaneous combustion. 4. That the unusual separation of the remains of the lower from the upper jaw was in accordance with, and merely corroborative of, the position of the mouth previously noticed. 5. That the putrefactive destruction of the peritoneal viscera, in contrast to the putrefactive softening of the abdominal contents, was connected with the penetration of the conflagration within the chest; and 6. That the fissure in the right temporal bone, though possibly caused by the action of fire, may, with at least equal probability, be attributable to me-

chanical violence directed against the skull from without.

Report of the Hessien Medical College, 16th August, 1848.—The members of this body report that the result of the above inspection had corroborated their former opinion (vide Dr. Graff's Second Report, ante) that *the death of the Countess of Geortitz had not been the result of spontaneous combustion.* After enumerating the leading points revealed by the inspection, and premising that though the two cases are not strictly parallel, they conceive they are fairly entitled to draw a comparison between the condition of the bodies of those who had perished by spontaneous combustion, and that of the body of the Countess on its disinterment.

In their previous Report they had shown—1. That in the instances recorded of death by spontaneous combustion the trunk of the body had always, with few exceptions, been reduced to coals and ashes, leaving only small vestiges of the head and limbs; and also, 2. That the coal which remained behind mostly still retained the form of the parts of the body destroyed, was very porous, and on the slightest disturbance crumbled down into dust. What, they argue, do they meet with in the present instance? 1. Instead of a heap of dust and ashes, a body, with the exception of the head, still retaining the human form. 2. Instead of the crumbling down of the burned and charred soft parts, a black, pulpy, greasy, and putrid mass. 3. Instead of the transmutation of the black burned bones into dust and ash, these still retaining their full firmness of texture, their usual hardness, and normal sound. 4. Instead of mouldering integuments and muscles, a compact, firm, dry, leathery skin, and a muscular tissue but little affected with putrefaction. 5. Instead of the falling of the charred fragments of bone into dust on being disturbed, a hard, firm charcoal, with all the qualities of common bone-charcoal.

The members of the college assent to the opinion of the examiners regarding the fissure in the right temporal bone, considering a decided cause for its production impossible to be fixed on, owing to the mutilated state of the skull.

[To be continued.]

EPIDEMIOLOGICAL SOCIETY.

THE meetings will be held on the first Monday evening in the month, from December to August inclusive. For this purpose the Council of the Royal Medical and Chirurgical Society have kindly granted the use of their house in Berners Street until the new society shall have secured the means of obtaining a permanent habitation.

Correspondence.

REVIEW OF CORFE AND MAITLAND ON MEESMERISM.

SIR,—Allow me to correct a slight error into which the reviewer of Maitland and Corfe on Mesmerism in your last number has been led, by the common nomenclature of the doctors who *preach* and the doctors who *practise*. The author of the "Church in the Catacombs," is Dr. Charles Maitland, M.D., the author whose work on Mesmerism is quoted is Dr. Samuel R. Maitland, D.D., of Lambeth. The latter is well-known as the author of "Eruvin," "The Dark Ages," "Letters on the Voluntary System," and sundry critical, bibliographical, and prophetic works.

Yours obediently,

T. K. CHAMBERS.

HHH Street, Berkeley Square.
Nov. 18, 1850.

LIFE ASSURANCE OFFICES AND MEDICAL REFEREES.

SIR,—One of our Medical Officers kindly referred me to a recent number of the MEDICAL GAZETTE, containing a list of Assurance Offices recognising the principle of compensating the private Medical Referees of Life Proposers, in which the name of this Society is omitted.

I shall, therefore, feel greatly obliged by your supplying the omission as soon as convenient.

Your obedient servant,

J. H. JAMES,
Actuary.

English and Cambrian Assurance Society,
for Fire, Life, Annuities, and Loans,
9, New Bridge Street, Blackfriars.

London, Nov. 15, 1850.

*** Mr. James's object will be answered by the insertion of this note.

ANALYSIS OF THE ASHES OF BLOOD. BY M. G. ROSE.

THE author has analysed the ashes of blood which had been previously washed in water, both hot and cold, in alcohol, and in ether. These ashes were found to contain a larger proportion of iron than those not thus treated, which contain a larger proportion of phosphoric acid. The ashes of the lixiviated blood did not contain a sufficient quantity of this acid to form the tribasic phosphate of lime and magnesia. M. Rose concludes from these results that the iron of the blood does not exist in a state of combination with phosphoric acid.—*Journal de Chimie Médicale*, Octobre 1850. x

Medical Intelligence.

ADDRESS TO THE QUEEN FROM THE PRESIDENT AND FELLOWS OF THE ROYAL COLLEGE OF PHYSICIANS.

THE following Address has been delivered to the Home Secretary for presentation to Her Majesty:—

To the Queen's Most Excellent Majesty.

We, Your Majesty's most loyal and dutiful subjects, the President and Fellows of the Royal College of Physicians in London, desire to approach your Majesty with a fervent expression of our devotion to our Sovereign, and of our unalterable attachment to our constitution in Church and State.

We deem it our duty to offer this expression of our principles and loyalty, in consequence of the aggressive measures of the Roman Pontiff,—measures unprecedented in the darkest ages of Papal dominion,—whereby he has assailed the supremacy of the British Crown, and has virtually denied the very existence of a Church in this country, or of Christian faith in Your Majesty's Protestant subjects.

As members of a liberal and highly educated profession, we feel ourselves especially called upon to protest against the moral and intellectual tyranny too often exercised by the Church of Rome. Too often has she endeavoured to fetter the human mind with the chains of ignorance. And, looking to the recent interference of the Pope with secular education in Ireland, we see plainly what would inevitably ensue in England also, if he should be allowed to re-establish here his spiritual power.

While we value and respect the rights of conscience, and desire that no civil disability should be imposed on account of religious opinions, we yet demand that your Majesty's authority, and the rights of the Church, of which your Majesty is the lawful Head, should be maintained inviolate; nor can we endure that these should be prostrated before the intolerant pretensions of a foreign power, and of a Church that will not accept of toleration, but claims an absolute and universal sway over the minds and consciences of men.

We remember that your Majesty's illustrious ancestors of the House of Brunswick were called to the sovereignty of these realms for the express purpose of defending the Protestant faith; and therefore we consider a blow aimed at the Royal supremacy as directed against the foundation of your Majesty's Throne. Although that foundation is laid too deep in the hearts of the

people to be shaken by any such attack, yet we humbly pray your Majesty to adopt such measures as to your Majesty shall seem best for the purpose of repelling the aggressions of which we indignantly complain, of preventing their repetition, and of vindicating, in the eyes of the world, the rights of the Sovereign and of the people of this Protestant land.

MEDICAL STATISTICS OF THE BOROUGH OF PLYMOUTH FOR THE QUARTER ENDING THE 30TH SEPTEMBER, 1850. BY DR. W. HAMILTON.

The population of the borough continues slowly to recover from the formidable reduction effected in its numbers by cholera during the preceding year. Among the various causes which might be assigned for the slowness with which society repairs the breaches which have been made in its numbers, there is one which it is right to notice more especially, as it is one which must of necessity continue to operate yet for a considerably longer period—namely, the havoc which cholera committed among females at ages between the marriageable standard

and that at which child-bearing usually terminates. From an examination of the materials kindly furnished by our registrars, I find that out of 10,169 females who were living, between the ages of 18 and 50, at the commencement of cholera, no fewer than 167, or 1·64 per cent., were carried off; and of these, as I have already shown, in a paper in the MEDICAL GAZETTE of the 4th inst., page 568, at least 98, or above 59·6 per cent., appear to have died between the third and ninth months of gestation; and, as I remark in the place to which I have referred, “that it is no visionary opinion which rests only on the unsubstantial basis of theoretical calculation, appears to be corroborated by a fact, communicated to me upon what I regard as unquestionable authority, of parturition having actually taken place while the mother was labouring under cholera, and this in more than one instance, although I have been unable to ascertain the precise number.”

The following tables exhibit the quarterly movements of the population, as deduced from the returns kindly furnished by the registrars—

Monthly Births and Deaths in the Borough of Plymouth during the Quarter which ended on the 30th of September, 1850:—

	DISTRICT OF ST. ANDREW.						DISTRICT OF CHARLES.					
	1849.			1850.			1849.			1850.		
	Bir.	Dea.	Diff.	Bir.	Dea.	Diff.	Bir.	Dea.	Diff.	Bir.	Dea.	Diff.
July	84	145	61	94	47	47	42	38	4	39	20	19
August	61	279	218	79	57	22	48	78	30	54	27	27
September	67	251	184	91	69	22	52	199	147	51	34	17
Sum	212	675	463	264	173	91	142	315	173	144	81	63
Mean	71	225	154	88	58	30	47	105	58	48	27	21

Borough.

	1849.			1850.			Movement of Population.	
	Births.	Deaths.	Diff.	Births.	Deaths.	Diff.	1849.	1850.
July	126	188	57	133	67	66	39,514	39,331
August	109	357	248	133	84	49	39,266	39,380
September	119	450	331	142	103	39	38,935	39,419
Sum	354	990	636	408	254	154	117,715	118,130
Mean	118	330	212	136	85	51	39,238	39,343

Thus, while during the corresponding quarter of last year population retrograded, under the malignant influence of cholera,

from 39,571 on the 1st of July, to 38,935 on the last day of September, or at the rate of above one and a half per cent., it has

advanced in the present year from 89,905 on the 1st of July, to 89,419 on the last day of September, or 144, being at the rate of 0.286 per cent. In the quarter immediately preceding the eruption of cholera, the increase was from 89,605 on the 1st of April, to 89,571 on the last day of June, being an increase of 166, or at the centesimal rate of about 0.43 per cent.,—nearly one-third more rapid. Should not a third invasion of cholera—an event by no means improbable—take place, we may anticipate a rapid ascent of population to its former level at a progressively increasing rate. In the meantime, we cannot too strenuously urge upon our municipal authorities the necessity of following the example so beneficially set by Ruster, and employ the interval of repose in providing against the possible recurrence of alarm by carefully removing, as far as their power will admit, those causes of mortality which, if they do not create, most assuredly foster and aggravate cholera: *sanissimi occurrere morbo* is a sage maxim of which we should never lose sight.

The temperature of October has been below that of the preceding year, the mean reading for the month having been only 57.5, against 60.58 last year; and the mean reading of every week but the second was equally below that of its corresponding predecessor. The mean temperature of the second week corresponded with that of the preceding year, having been 58 in both.

The atmospheric pressure, on the contrary, was in excess, both on the entire month and in the individual weeks, with the exception of the fourth, when the mean reading was 0.06 below that of the corresponding week.

The mortality during the month has again exceeded the natural increase by births, chiefly from the prevalence of scarlatina, which has been particularly fatal among the young, and yet continues to rage with unmitigated severity. The comparative movement of the population in the two years will be seen in the following table:—

	St. Andrew.			Charles.			Borough.			Movement in the population of borough.		
	Births.	Deaths.	Diff.	Births.	Deaths.	Diff.	Births.	Deaths.	Diff.		1849.	1850.
1849	102	127	25	35	57	22	137	184	47	1st Oct.	38,935	39,419
1850	72	74	2	31	40	9	103	114	11	Decrease	47	11
Diff.	30	53		4	17		34	70		31st Oct.	38,888	39,406

Snow fell on Dartmoor on the 25th, in the neighbourhood of Ashburton.

. It is highly creditable to Dr. Hamilton that he thus keeps a careful record of the medical statistics of the town in which he is residing. His example should be followed in other localities. The increase of deaths from certain diseases, when thus noted, leads to the best results, and tends to prevent that excessive mortality which was formerly witnessed from the unchecked ravages of synotic diseases.

ROYAL COLLEGE OF SURGEONS.

THE following gentlemen, having undergone the necessary examinations for the diploma, were admitted members of the College at the meeting of the Court of Examiners on the 15th instant:—Messrs. Charles Neate, Maidenhead, Berkshire; William Sutcliffe, Hedben Bridge, Yorkshire; Thomas Clarke Brady, Lifford, County Donegal; Andrew Bolton, Eb-

chester, Durham; James Cummin Toshach, South Shields, Durham; and Robert Turner Bywater, Leeds. At the same meeting of the Court, Dr. John Barclay, of Haaler Hospital, passed his examination for naval surgeon. This gentleman had previously been admitted a member of the Edinburgh College.

FEVER AT ALEXANDRIA.

INTELLIGENCE from Alexandria of Nov. 8th, announces that there has been much fever prevailing there, attended with unusual mortality. Among the deaths last week were those of Mr. W. B. Le Gros, Private Secretary to the Consul-General, Mr. Murray, Dr. P. O'Connor, and Mr. Finnie, surgeon-dentist.

OBITUARY.

ON the 14th inst., at Dorchester, Francis Jackson, Esq., M.D., aged 43.

ON the 15th inst., at Wokingham, Berks, Mr. James Wheeler, formerly surgeon of that place, aged 84.

Selections from Journals.

ON THE STRUCTURE OF THE FIBRE OF VOLUNTARY MUSCLES, AND OF THE HEART, IN DIFFERENT CLASSES OF ANIMALS.

M. LAMEN, of Paris, and M. Prevost, of Geneva, have separately been engaged in microscopical researches on the structure of muscular fibre, and have arrived independently at the same results, which we subjoin:—

In endeavouring to include in a general consideration all the elements which are combined in the production of voluntary muscles, it will be seen that these muscles pass through four phases, or occasional degrees of development, before their texture is completely adapted to the execution of its functions.

The first degree is that of *metastite*, without muscular fibre. The entire animal envelope is capable of contraction and expansion, and of performing rapid movements of progression and swimming, without the detection, by the most powerful microscopes, of fibres, granules, striæ, or cylinders—elements which are early met with in muscular fibre. These movements are analogous to those which are observed under other circumstances in animal and vegetable bodies—movements which are independent of animal life. Such are the vibratile motions of the cilium on the surface of many embryos; those of the spermatic bodies, and of the sporules of algae, of polyps, &c.

In the second degree of development the essential element of muscle, the muscular cylinder, is not met with; but, of its principal parts, the fibre exists. This is found embedded in a transparent intermediate substance, but not arranged in independent bundles. In the animal forms in which this degree is met with the fibres are arranged in superimposed planes, crossing at right angles, composing circular or radiating laminae around the orifices of the body by which the alternate opening and closing of the cavity is effected—the first trace of sphincters. These fibrous laminae are met with in polyps, scalephæ, some molluscs, and annelids. The movements effected by these differ from the preceding in the fact that their direction is more determinate and in accordance with that of the muscular laminae.

The third degree of development is that in which the fibres become aggregated into cylinders or fasciculi, which constitute true independent muscles. This degree is found in the same animals at the same time with the two preceding degrees. This

first sketch of the muscular cylinder offers several varieties in aspect. In some instances the grouping of fibres is but faintly traced, in others it is very distinct. The fasciculi are generally imbedded in a transparent medium of union, which forms the representative of the nuclear tissue of higher animals. It is important also to observe that the muscular fibre offers several varieties in the mode of distribution of the molecular granules in the interior of the cylinders, being sometimes very scarce, at others so abundant as to mask the fibrous structure; sometimes regularly distributed, at others in the direction of the bundles and in their interstices, and again distributed at regular distances in the primitive cylinder, so as to present regularly disposed alternate transparent interstices in the course of the fibres.

Arriving at the fourth, or most perfect degree of development, the muscles of voluntary motion are met with from molluscs upwards to the highest classes. The line of demarcation, however, is not so clearly traced but that these most perfect forms are found in some polyps, molluscs, helminthoids, annelides, and radiates.

M. Lebert states that errors exist in the observations of other anatomists in reference to the structure of muscular fibre. By varying and greatly multiplying his observations and researches, M. Lebert believes that he has avoided these errors.

The muscular cylinder is, in its relation to muscular movements, the analogue of the primitive nervous tube in relation to the phenomena of innervation. It is neither the primitive fibre, nor the transverse marking, but the union in the muscular cylinder or fascicle, of those essential elements which constitute the integrity of the motor force in the most perfect form of muscle.

By "primitive cylinders" M. Lebert denominates every portion of muscular tissue that is clearly defined at its circumference, or which under the microscope presents two longitudinal aspects much more definite and isolated than the longitudinal fibres of their interior, and which, for the most part, exhibit transverse foldings. These cylinders are long, parallel, flattened from before backwards, grouped together, and united by cellular tissue to form muscular fasciculi.

There is one mode of grouping of these cylinders to which sufficient attention has not been paid: it is that of their union to the number of four or five, to form one secondary cylinder much more distinctly defined externally than the constituent primary fibres. These secondary cylinders are furnished with common sheaths, and cradled to those of

these contained primitive cylinders. Under these circumstances, two errors are to be avoided: one is, not to confound the *secondary* with the *primary* cylinder; the other, still more important, not to mistake the *primitive cylinders* for the *primitive fibres*. Either of these errors is a source of confusion in terms and measurements.

The muscular cylinder, then, is furnished on its surface with transverse folds, and involves the primitive fibres with their uniting medium, and their fibrillar and inter-fibrillar molecules.

The transverse folds are wanting in the fibres of the heart of the higher animals; and in some voluntary muscles of the very young of vertebrate animals. The substance of the heart appears to form an intermediate degree between the muscles of voluntary and of involuntary action—between those of animal and of organic life.

Histological evidence is not yet conclusive to that point, but there is every probability that it will establish the division of the muscles into those of animal and those of organic life.

To return to the surface of the cylinders. The transverse folds are not the effects of their contraction: they are permanent, although they approximate and recede according to the condition of contraction and relaxation of the cylinder. These transverse folds exist only on the surface; otherwise, by traversing the cylinder, they would divide it into a series of discs.

The primitive fibres in the cylinders are firmly united together: they are smooth, alternately opaque and transparent, owing to the arrangement of their granules; and, when arranged side by side, their juxtaposition gives the appearance of transverse folds.

The size of the primitive fibre fluctuates between 0 mm, 001, and 0 mm, 0015 (about 1-25,600th of an inch), and corresponds nearly with the diameters of the opaque granules.

The size of the non-striated cylinder varies from 0 mm, 004 (1-6400th inch), and 0 mm, 02.

That of the striated primitive cylinder from 0 mm, 005, to 0 mm, 1.

The distance of the transverse folds varies from 0 mm, 001, to 0 mm, 01.

The nerves are distributed by their primitive tubes between the muscular cylinders with which they lie in contact without penetrating.

With regard to the manner of the contraction of muscular fibre, M. Lebert repeats the statement of Weber, which his researches confirm.—*Gazette Médicale*. x

EXPERIMENTS ON ABSORPTION—INTRODUCTION OF INSOLUBLE SUBSTANCES INTO THE CIRCULATION. BY M. BERNARD.

SUBSTANCES that are capable of being absorbed may be divided into three classes:

1. Liquids or solutions.
2. Gaseous bodies.
3. Solids capable of minute division.

The last mode of absorption has been disputed; but M. Bernard states that he has shown this to be no less real than that of undissolved fatty matters.

M. Bernard has studied the subject of absorption in reference to the following points:—

What part do the nerves perform in this process? Is it entirely physical, and independent of nervous influences? Does the quantity of blood influence absorption? Does absorption take place in a bleeding animal with a rapidity proportioned to the loss of blood?

The stomach, when empty, absorbs freely; but when full, and in the act of secretion, it absorbs no longer. Absorption takes place generally where there are vessels, as in the lungs, the cellular tissue, the skin deprived of its epidermis; but nevertheless, there are substances which are not equally absorbed in all parts of the body, which will cause death when introduced into the blood or tissues, but are inert when introduced into the alimentary canal. A living or a dead membrane is only capable of endosmosis up to a certain point; after saturation the process will cease. Does carbonic acid produce asphyxia by its own poisonous nature, or by the impediment offered to respiration? An elective or specific absorption has been attributed to the lacteals. M. Bernard points out that the phenomena are due to peculiarity of structure of the lymphatics and veins of each intestinal villus. MM. Magendie, Segalas, Sandras, Bouchardat, and others, considered that certain substances pass from the intestines by the veins, and not by the lacteals. M. Bernard indicates an error, in their observations having been made on the contents of the portal vein and thoracic duct. If prussiate of potash be injected into the stomach, and the lacteals be examined as they pass from the intestines, the salt will be detected, and thence to the glands.

In order to illustrate the various modes of absorption, M. Bernard first treats of the absorption of poisons.

The poison of the viper is clearly not absorbed by the mucous membrane, or the animal would poison itself by the secretion of its venomous saliva. The same may be observed of the poison of rabies. A parallel fact is recorded by Humboldt.—The South

American Indians, in the neighbourhood of the Orinoco, have a poison (the *curare*, or *woorara* poison), collected from plants of the *Strychnos* tribe, and with which they poison the weapons used in killing animals; and yet these animals feed with impunity upon the plants from which the poison is derived.

M. Bernard at first supposed that this fact was to be explained by attributing its separation from the blood in the liver; but having injected a portion of this poison into one of the veins of the portal system, the effects of poisoning speedily followed, showing that the immunity resulted from some influence exerted upon the poison in the alimentary canal.

M. Bernard introduced a portion of *woorara* through a fistulous opening into the stomach of a dog, and administered also a portion by the mouth, without producing ill consequences. The gastric juice, under both circumstances, exerted a poisonous influence when inserted into the cellular tissue, rendering it evident that the *woorara* had not been destroyed by the chemical action of the gastric juice.

When injected into the rectum, or inclosed in a portion of intestine between two ligatures, the poison was absorbed without ill effects.

The analysis of the *woorara*, and of the poison of the viper, exhibits in each an insoluble substance, which M. Bernard regards as a ferment analogous to animal and vegetable diastase, or to pepsin.

M. Bernard gives the result of experiments made upon the action of *amygdaline* and *emulsine*—two substances which, separately, are inactive; but which, mixed together with water, form prussic acid poison of great potency. If administered to animals so as to combine in the course of the alimentary canal, no injurious effect ensued; but if made to meet in the cellular tissue, or in the course of the circulation, the phenomena of poisoning are immediately manifested, the odour of prussic acid being perceptible in the dead body.

The same occurs if the *amygdaline* be introduced into the digestive tube, and the *emulsine* into the blood, because the former is dissolved and absorbed; but the *emulsine* containing the insoluble ferment is not absorbed, and the poisonous effect does not follow if the latter be in contact with the mucous membrane.

Investigations into the phenomena of *endosmosis* reveal curious analogies with reference to the non-absorption of certain substances by mucous membranes. If an endosmometer, formed of animal membrane, be plunged into a solution of *woorara*, the water passes, and not the poison. If the experiment be tried on a solution of *prussic*

acid, the water will pass, while the *emulsine* remains. The diastase of a germinating seed is in the same predicament, because vegetable membranes possess similar properties. If a bean be placed in gastric juice or a solution of pepsine, it will become swollen by imbibition, and be capable of germination so long as its surface is unbroken. In this manner seeds taken into the digestive organs may retain their germinating powers, and be conveyed to great distances. But if the slightest fissure exist on its surface, the pepsine or diastase will enter its structure, and it will be digested. In a similar manner *amygdaline* and *emulsine* exist distinctly side by side in the bitter almond so long as its structure is entire: they are separated only by a thin membrane.

M. Bernard alludes to the absorption of vermilion and of Prussian blue from tattooed marks in the skin, and the detection of these substances in the neighbouring glands by M. Föllin. M. Starlin, he adds, has also detected abundant particles of charcoal in the lungs of animals to which that substance had been largely administered. These facts, M. Bernard considers, establish the absorption of insoluble substances by the lymphatics. The explanation of their manner of absorption is still wanting. Microscopical anatomy does not detect orifices in the villi of mucous membranes.

The explanation of these apparent cases of elective power of absorption in mucous membranes is simply a want of absorption: it is only the soluble portion which cannot be absorbed, while the insoluble portion is absorbed; thus reversing the doctrine—*corpora non agunt nisi sint soluta*. All substances not capable of absorption belong to the class of ferments or poisons.

M. Bernard mentions some facts from inorganic nature analogous to the preceding—*e. g.*, if a solution of acetate of copper be poured upon a filter of animal charcoal, the acetic acid will pass through, and the copper remain in the charcoal. In retention of urine the water is absorbed, while the urea becomes concentrated. The same is observed with reference to bile, which becomes inspissated on retention.

M. Bernard gives a new explanation of the phenomenon of the rapid appearance of certain fluids in the urine after their ingestion into the stomach. He has discovered that liquids, after having traversed the vena porta and the liver, and having arrived in the vena cava inferior, descend again by that vein to the renal veins; that there exists a valve in the interior of the vena cava, at the point of the orifices of the renal veins, which hinders the passage of the blood in the *ca* and the fluid is

obliged to pass by the renal veins, which thus for the time perform the office of arteries. This phenomenon occurs only when a large quantity of liquid has been drunk. The mechanism exists in all animals which take a large quantity of food of a slightly nutritious quality. If only a small quantity of fluid be taken, the circulation proceeds uninterruptedly towards the heart.

The lumbar and azygos veins form a collateral circulation by which the obstruction of the ilio veins is obviated. The communication of the portal circulation in the liver with that of the vena cava favours the rapidity of the passage of fluid, and forms a diversion for the relief of the heart against so sudden an increase in the quantity of the blood.

M. Bernard further points out the muscularity of the vena cava from the hepatic to the renal veins. This portion he regards as a true heart, which acts temporarily under the circumstances above indicated, while the valves before named prevent the flow of blood beyond the renal veins. The quantity of blood thus diverted passes by urinary secretion. M. Bernard remarks also that this arrangement of the circulation may explain the rapid disappearance of certain poisons in some animals. It may be regarded as the representation of the circulation in reptiles, where the blood of the lower parts of the body passes through the kidneys.—*L'Union Médicale.* 7

BOOKS & PERIODICALS RECEIVED

DURING THE WEEK

Operative Surgery. By Frederick C. Skey,
F.R.S.

Elements of Chemistry; including the Applications of the Science to the Arts.
By Thomas Graham, F.R.S. L. & E.
2d Edition. Part 4.

Physico-Physiological Researches on the Dynamics of Magnetism, Electricity, Heat, Light, &c. in their relations to Vital Force. By Baron Charles von Reichenbach. Translated, with a Preface and Notes, by John Ashburner, M.D.

Guide to the Urino-Chemical Chest. By Robert Venables, A.M. M.B.

The Surgeon's Vade-Mecum. By Robert
Druitt, F.R.C.S. &c.

**Of the Crystalline Lens and Cataract. By
Bernard Edward Broadhurst.**

**Surgical Report on Bi-lateral Lithotomy;
with general Remarks on Operations for
Stone. By J. Nottingham, M.D.**

The Commercial Handbook of Chemical Analysis. By A. Normandy.

BIRTHS & DEATHS IN THE METROPOLIS

During the Week ending Saturday, Nov. 18.

BIRTHS.		DEATHS.	
Males....	787	Males....	436
Females..	794	Females..	472
<hr/>		<hr/>	
1581		908	

CAUSES OF DEATH.

ALL CAUSES	920
SPECIFIED CAUSES	920
1. <i>Symptotic</i> (on Epidemic, Endemic, Contagious) Diseases.....	920
<i>Spontaneous Diseases</i> , viz.—	
1. Dropsy, Cancer, &c.....	36
2. Brain, Spinal Marrow, Nerves, and Senses.....	100
3. Heart and Bloodvessels.....	26
4. Lungs, and organs of Respiration.....	26
5. Stomach, Liver, &c.....	53
6. Diseases of the Kidneys, &c.....	12
7. Childbirth, Diseases of Uterus, &c.....	9
8. Rheumatism, Diseases of Bones, Joints, &c.....	5
10. Skin.....	2
11. Premature Birth.....	26
12. Old Age.....	26
13. Sudden Deaths.....	16
14. Violence, Privation, Cold, &c.....	26

The following is a selection of the numbers of Deaths from the most important special causes:

Small-pox	18	Convulsions	26
Measles	24	Branchitis	70
Scarlatina	39	Pneumonia	56
Whooping-cough	27	Phthisis	125
Diarrhoea	20	Lungs	8
Cholera	0	Testicles	7
Typhus	56	Stomach	4
Dropsy	8	Liver	12
Hydrocephalus	25		
Apoplexy	34	Childbirth	5
Furcalysis	20	Uterus	3

REMARKS.—The total number of deaths was 82 below the average mortality of the 40th week of the previous years.

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer 30:
 " " " Thermometer* 46-2
 Self-registering do.^b Max. 61° Min. 14°

^a From 12 observations daily. ^b Sun.

RAIN, in inches, '09.—Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was 4.3 above the mean of the month.

NOTICES TO CORRESPONDENTS.

The letter of a Constant Reader and Subscriber is under consideration.

We are obliged to Dr. H. Fearnside for his communication. His request shall be attended to. We shall be glad to receive other papers from our correspondent.

Communications have been received from Dr. Pollock and Mr. Spence Bate. A private note will be sent to Dr. Pollock.

Mr. James Fisher's letter has come to hand. The subject referred to by Dr. W. Cholmeley is elsewhere noticed.

Lectures.

CLINICAL LECTURE
ON A CASE OF
ACUTE IDIOPATHIC TRISMUS.
(Delivered at King's College Hospital).

By R. B. Todd, M.D. F.R.S.
Physician to the Hospital.

[Reported by H. H. Salter, M.B., Dem. Anat.
K.C.L.]

LECTURE XIV.

GENTLEMEN,—Within the last day or two, as you are aware, a patient has died within the walls of this hospital of idiopathic tetanus, or, more properly speaking, of that form of it which, from the muscles of the jaws being chiefly affected, is called trismus (*tripsē, strido*); and, as this is comparatively a rare disease, and, in the majority of cases, a fatal one, I am anxious not to let an instance of it pass without making it a source of instruction to you, by giving you some account of its clinical history and pathology.

The patient, Henry Franklin, æt. 40, was admitted into the Sutherland Ward on Tuesday, November 6th. His history was this:—On Sunday, October 28th, he awoke with a swollen face, and some feeling of stiffness about the jaws: he attributed this to his having caught cold by sleeping with his window open. The feeling of stiffness increased, but he was able to swallow till Sunday, the 4th of November, seven days after the first accession of the symptoms; and since that time he has been unable to perform the act of deglutition. On his admission (Tuesday, 6th) we found him with his face very much swollen in the region of the parotids, and with considerable fetor of his breath; and we also found, on inquiry, that he had been taking some pills with mercury, though in small quantity, in them: it is therefore very possible that he may have been one of those persons who are affected by a small quantity of mercury, and that he was in a state of salivation.

We found also great rigidity of his masseter muscles, so as to prevent his opening his mouth, and, consequently, his either speak-

ing or swallowing. His teeth could at first be separated just so far as to slip in a card, but no farther. Moreover, we found that the effort was accompanied with great pain, which is always the case if tension is applied to a muscle in a state of contraction, as is seen in ordinary cramp, and in the treatment of club-foot: moreover, the attempt to overcome this contraction not only gave great pain, but also increased the spasm. It appeared that the muscles of deglutition were also affected, as he stated that the attempt to swallow his saliva nearly choked him.

Upon further inquiry we learned that his previous life had been temperate and sober; that he had had syphilis and secondary eruption, but no sore-throat, or nodes on his tibia or elsewhere; that he had never been subject to fits of any sort, but that he had had brain fever. This was the first day we saw him, and my apprehensions were excited as to the nature of the case: I feared it would turn out what it afterwards proved. But this supposition was very much negated by the absence of any manifest cause for tetanus. Excepting the exposure to cold, the slight salivation, and a small abrasion on the nose, occasioned by a fall which occurred the day before the accession of his first symptoms, there was nothing to which tetanus could possibly be assigned. The wound on the nose was very slight, and may be dismissed from the consideration at once. Cold is not an infrequent cause of tetanus in tropical climates, where the alternations of temperature are great; but it is a rare cause in this climate. I have witnessed one such case, in which the tetanic symptoms were attributed to the exposure of the patient to the open air during the whole of a very cold night. It is very possible that the exposure to the open window all night might have been the exciting cause of the disease in the present instance. Might the salivation have been the cause of the tetanus? There are on record some cases of tetanus produced by salivation. But it does not appear from the history of this case that the salivation was distinctly antecedent to the tetanic symptoms.

There was, then, a certain amount of obscurity about the case, and I was not quite prepared to act at once upon the notion that it was tetanus; I therefore adopted a treatment less decided than I should otherwise have pursued. I ordered him fomentations to the jaws and throat, an enema of starch and opium at night, and beef-tea enemata throughout the day: in fact, my treatment was very much of the expectant kind. I determined to see what the next day would produce, and

contented myself with dealing with the existing symptoms.

The next day we found the symptoms of spasm still the same. The jaw was firmly clenched, and attempts to open the mouth produced great pain. He was still unable to swallow his saliva, which frothed out of his mouth at each expiration. The secretion was certainly unduly abundant. All this time he was perfectly sensible, clear, and composed. He was not at all excited in his manner, and, although he could not speak, he understood all that was said, and wrote down on paper coherent and intelligent answers to all questions that were put to him.

Here, then, we had no symptom but the local affection of trismus, which might have been the effect of the local application of cold; but my fears were that it was but the early stage of a genuine case of lock-jaw, and I stated my fears to be such. I therefore determined to try tobacco fomentation, which I have frequently seen used, and with great advantage if care is taken to uphold the strength of the patient. Tobacco has this great disadvantage,—it is a very depressing agent, and therefore tends to throw the patient into that very condition which is the most fatal tendency of the disease. To avoid this, care should be taken to use the tobacco infusion weak, or the patient may be exhausted to a degree that may prove fatal.

At seven o'clock of the evening of this day, a symptom supervened which left no doubt as to the nature of the malady: it consisted in a violent paroxysm, throwing into active contraction, not only the muscles of the jaw, but also those of the neck, chest, and back, the last producing partial opisthotonos; the sterno-hyoid, sterno-thyroid, and sterno-mastoid muscles were thrown into a state of rigidity, and this condition implicated the muscles of respiration, and so far interfered with that function, that the patient felt as if he should be choked, and jumped out of bed to endeavour to get air. This choking sensation was not caused by any contraction of the glottis, but was due to the impeded action of the muscles of respiration: in fact, the man was kept in a constant state of imperfect respiration, or rather forced expiration, the consequence of which was that the venous system was thrown into a state of great congestion,—an interesting illustration of the way in which convulsion may give rise to congestion. The pupils were very much contracted during the paroxysm,—a circumstance of great interest, as showing a peculiar state of nervous centres connected with the excitement of the spasm; for, as soon as the spasm went off, the contraction of the pupils went off too; and, when the

spasm was at its height, the pupils were contracted to pins' points. The pulse during the paroxysm rose to 120.

From this time the case put on a more severe form: not only the permanent contraction of the muscles and all the other conditions continued, but every now and then one of the paroxysms would come on, each succeeding one being more severe and longer than that which preceded it; leaving the patient in a state of great exhaustion, and exciting in our minds the idea that he might be asphyxiated and die in one of them. If, however, there was any reason for such a fear, there was nothing to be done to obviate it; for, even had we performed tracheotomy, it would have availed little, for we should still have left the rigidity of the respiratory muscles unrelieved, which alone would doubtless have sufficed to produce asphyxia. But, in truth, death by exhaustion was the great thing to be feared: what we had most to apprehend was that which actually did take place—namely, that the increased frequency and severity of the paroxysms would at length produce a fatal exhaustion, and that the patient would sink rapidly after one of them.

The indication for treatment was therefore to support: and, since it was impossible to get the patient to swallow (the attempt being immediately followed by an exacerbation of the spasm), the only alternative was, in default of one entrance, to try another. I gave him, therefore, a drachm of sulphate of quinine, in a small quantity of water, as an injection, continued the beef-tea enemata, and being anxious, if possible, to diminish the pain and spasm, I ordered the frequent inhalation of chloroform,—the plan being to give it, not in large quantities, but in small and often-repeated doses, with a large admixture of air, so as to produce a gradual and soothing effect. By this means we were able to obtain resolution of the spasmodic condition of the muscles; but to arrest the paroxysms, or diminish their strength when on him, the chloroform seemed almost powerless.

At about a quarter to twelve, A.M., on the 8th, the quinine enema was repeated: at this time he was lying in an unconscious state from the chloroform (under the influence of which he was constantly kept), with his muscles relaxed, and breathing tranquilly. About five minutes after twelve a violent spasm came on, the jaws knocked together with a distinctly audible click, the face became much distorted and livid, the eyes rolled upwards, and there was much frothing at the mouth, and great apparent difficulty of breathing the sterno-mastoids violently contracted, and the head thrown forwards: in a minute or two

this was succeeded by a well-marked opisthotonos, the body being much curved, and resting on the occiput and sacrum. In this state he died. His death may be said to have taken place by asthenia, its immediate cause being exhaustion produced by the spasm.

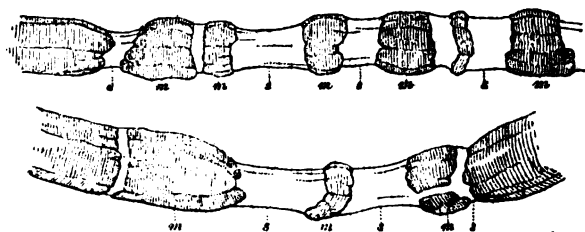
There was nothing in the previous history of this patient to throw any light upon the case; he had been a sober man, had had syphilis and secondary eruption, and an illness which was called brain fever, but there was nothing to indicate any disposition to the malady of which he died—nothing, either in the man or the circumstances in which he had been placed, that could be considered predisposing to tetanus. So it is in all the cases of this disease we meet with: there is no connection between the previous history of the patient and the disease; the immediate exciting cause, and the detail of the symptoms, constitute the entire history of the case.

Of course I was very anxious to have a post-mortem examination, and it fortunately happened that we were able to have one under very advantageous circumstances; the examination being made only four hours after death, when, if there had been any morbid appearances, they would have been free from the fallacy of post-mortem change. There was, however, as I had anticipated, no morbid appearance which seemed to offer any explanation of the disease: the nervous centres, both brain and spinal cord, were perfectly healthy,—perhaps the brain contained a little more blood than usual,—a fact not to be wondered at when we consider the violence of

the paroxysms: the grey matter of the brain was perfectly natural, and its demarcation peculiarly distinct; perhaps, indeed, this remarkable distinctness may have been morbid, and may have indicated an undue functional activity of these parts.

I was very anxious to examine the state of the masseters, to see if any change had taken place in their ultimate structure, in consequence of the prolonged spasm which they had undergone. The result has been highly confirmatory of certain doctrines first laid down by Mr. Bowman, and remarkably illustrative of the way in which the contraction of muscle takes place. When a muscle is thrown into a state of contraction, that contraction does not affect the whole mass of the muscle, or even the whole of any of its fibres, at the same moment, but certain points or nodes are contracted; while the intervals are not merely passive, but are stretched by the contraction of the contracted portions; so that you have a succession of contracted points throughout the entire length of each fibre of the muscle. Now here you see a magnified representation, made by Mr. Salter, of two fibres, taken from the masseter of the patient, showing that peculiar condition which was first described by Mr. Bowman to exist in the muscles of persons who have died of tetanus, and which, as you will see, results immediately from the physiology of muscular contraction. You must not imagine that all the fibres were so affected: these were the two most marked specimens among many hundreds that were examined, some possessing the appearance in a slight degree, some not at all. Here you see the sarcolemma continuous

Two Fibres of the Masseter Muscle ruptured by the Tetanic Spasm.



m m separated masses of sarcous parietes; s s s sarcolemma.

throughout its entire length; but the contained sarcous matter is *not* continuous, but is broken up into isolated portions of various sizes, producing a peculiar bulged condition of the sarcolemma, with collapsed constricted portions in the interval. Now how is this appearance produced? Thus: each of the ruptured portions was the centre of a contraction at the time

when the muscle was in a state of violent spasm; the force of the contraction exercised at these points was so great that the tenacity of the intervening stretched portions was not sufficient to resist the divergent force: they gave way, and the particles thus isolated when in a state of contraction, having no antagonistic force to restore them when the contracting force had subsided, remain,

as you see them, in a state of permanent contraction, leaving intervals of considerable length between them.

I think, gentlemen, that these are all the points I have to call your attention to in the history of the case: and I will now make a few observations of which this history is suggestive.

First, with regard to the name: what shall we call it? Two names are in frequent use for such affections—*tetanus* and *trismus*. I prefer *trismus*, as being more specific, localising the affection, and showing its restriction to the muscles of the jaw. *Tetanus* is a more generic name, and indicates the general existence of tonic spasm, without any restriction as to its whereabouts. Tetanus can hardly be called a very rare disease: trismus occurring in the adult is a very rare disease. In the present case the affection was for eight days confined to the muscles closing the jaw; only within a day of his death did it extend to the muscles of the neck and back; and not till the very last stage did it so affect the muscles of the back as to produce opisthotonos: whereas, in ordinary tetanus, the extension of the spasm to all the muscles generally occurs very early in the disease: opisthotonos may be a prominent symptom for three-fourths the duration of the attack; and, if you put your hand on the arms or legs of patients so affected, you find them hard, knotty, and rigid; whereas, in the present case, the affection was limited to the jaws; and I therefore prefer to call it trismus. The only essential difference between the two diseases is in the extent of the nervous centres, and, therefore, of the muscles implicated.

Sir Benjamin Brodie has instituted an excellent practical distinction between *acute* and *chronic* tetanus—the one running a short course, with strongly marked symptoms, and terminating always fatally; the other of longer duration, milder form, and frequently recovered from. Traumatic tetanus is very apt to be acute; but tetanus, however caused, frequently puts on a chronic form. Some of these cases last twenty days or more; and, when prolonged in this way, there is great hope of ultimate recovery. If such a case—or still more, if two or three such—should happen to fall into the hands of some very zealous supporter of any particular practice—if he is a great bleeder, or a free giver of opium—the treatment is immediately set down as specific, and the recoveries are appealed to in proof of its infallibility, the real secret of the successful issue being that the cases were chronic, and not acute. Now in the case of our patient it cannot possibly be said that anything was done to shorten life: possibly the means adopted for its

prolongation might have been more energetically plied; the quinine might have been earlier administered, and perhaps more support might have been given: still nothing was done that could by any possibility have accelerated the fatal termination of his disease. Yet we see him admitted into the hospital on the Tuesday with but slight symptoms of the disease, and dead on the Thursday morning. And this is an essential part of the natural history of all these acute cases—speedy and fatal termination, death taking place by exhaustion.

The peculiar points in this case that especially call for remark are, the violence of the spasm, and its restriction for so long a time to the jaws. As I said just now, genuine trismus is rare in adults. In newly-born infants, however, trismus is not rare, particularly in hot climates: in the West Indies it used to be very common: it goes by the name of *trismus nascentium*; and though now, from greater care in food, cleanliness, and ventilation, it is much less frequent than it was, it even yet carries off a great number of children.

I have said that this case illustrates the mode of death by exhaustion: this is an important point, and one I particularly wish to impress upon you. Death does not take place by asphyxia, nor by any particular effect on any special organ, but from general exhaustion, induced by the protracted and unwonted muscular exertion. This, I think, is not sufficiently attended to in the treatment both of this and of many other diseases.

The case also illustrates the little hope we have of obtaining any information, either practical or pathological, by post-mortem examination of the particular state of the nervous system: we can only draw our conclusions as to the pathology of the disease from our knowledge of the physiology of the parts concerned. Now, reasoning on this principle, it may be laid down that the phenomena result from an exalted polarity of the centres supplying the parts affected. In the case of traumatic tetanus, the exaltation of the polar state commences in the afferent nerves of the part that is the seat of the wound: if the tetanus arises from cold, the exalted polarity commences in those nerves of common sensation distributed to the exposed part: from the periphery thus irritated the condition is propagated through the nerves to the centres, and the effects on the muscular system show to what portions of the nervous centres the exaltation of the polar force is communicated. This, however, does not fully explain the production of tetanus; for peripheral nerves, and even nervous centres, are often subjected to great irritation without giving rise to

tetanus; and it is well known that it is impossible, even by severe mutilations, to produce tetanus in the lower animals: whereas a slight accidental injury (as when a horse picks up a nail) will often produce the disease in its worst form. It would seem that some peculiar state of the system—probably some peculiar condition of the blood—is a necessary precursor of the disease. Hence, no doubt, its greater frequency in warm and unhealthy climates, in over-crowded and ill-ventilated military hospitals, and, among ill-housed, ill-fed infants.

That tetanus may be produced through the blood is shown by the results of the administration of strychnine, which exactly imitate the tetanic symptoms in every respect: so that you may at will develop the phenomena of tetanus in an animal by giving him strychnine, or injecting it into his blood, but you cannot cause it by external injuries.

What was the exact portion of the centres affected in the present instance? The localization is pretty clear: it was evidently the upper part of the spinal cord, the medulla oblongata, and its upward prolongation in the cranium, especially in the neighbourhood of the implantation of the fifth nerve. Indeed, the supposition that the disease was caused by the salivation would afford a very plausible explanation of the localization of the morbid action in this latter region; for in ptyalism the seat of the peripheral irritation must necessarily be the sentient filaments of the fifth nerve, which would, of course, propagate the irritation to the centre, and excite a similar state of the motor nerves, thereby giving rise to the spasmodic condition of the muscles of mastication which are supplied by the same nerve.

And we may ask, further, what is the nature of the morbid process set up in these centres? To this it may be confidently replied, as the result of repeated examinations, that it is not inflammation, nor anything allied thereto. And it may, with quite as much certainty, be affirmed that it is a state identical with that which strychnine is capable of producing. Now this state does not reveal to the most careful observation any appreciable departure from the normal state. I have repeatedly examined with the microscope the spinal cords of animals killed by strychnine, and have never been able to detect, even with the highest powers, the slightest morbid chan-

The condition of the nervous system in tetanus is, as it seems to me, best described by the term *exalted polarity*,—which favours the undue development of the nervous force. A peculiar state is, that whilst the polar

nervous centre is at all times exalted, it is liable to frequent fits of augmentation or exacerbation, during which more extensive effects are produced. In this way we explain the absence of any affection of the spinal cord for a considerable period, and the subsequent excitation of it in consequence of severe paroxysms affecting the medulla oblongata and its prolongations.

Now, if we were to meet with another case, what treatment should we adopt? I do not know that I should pursue any different treatment from that which I have described to you as having been adopted in the case of our patient Franklin. I might, perhaps, give support earlier than I did in the present case. I should give a large quantity of quinine by the mouth or rectum: if there were much difficulty of deglutition, I should throw up beef-tea enemata freely, and I should administer chloroform carefully. The great danger of chloroform is that it depresses: now, in tetanus you ought to husband and augment the strength of your patient as much as possible; the disease itself depresses enough; and an important aim in your treatment should be to enable your patient to sustain the reiterated shock of his spasms: therefore, if you give him chloroform, take care at the same time to support him well, and be not afraid of giving him stimulants—such as wine or brandy—if you think he can digest them, and especially if the difficulty of swallowing prevents the patient from taking more than a small quantity of nutriment at a time. The cases that I have known succeed the best have all been treated with close attention to upholding the powers of the patient. If you bear in mind that the great danger is of death from exhaustion, and act accordingly, you will not go wrong in this particular. There is no use in blood-letting in cases of this kind, as frequently practised formerly, nor in active purgation: the bowels, however, should be evacuated daily, or on alternate days, but more than this is prejudicial. There is also no good to be derived from giving those excessive doses of opium that have been used in so many cases; for, besides the negative evidence of its inutility, there is this objection to the practice—that opium given in large doses tends to call forth that very exaltation of the polarity of the nervous system which we desire to combat. This is abundantly proved by experiments on cold-blooded animals. You may throw a frog into

opium; and although so it is produced by opium animals, many circumstances that it may give rise to much less in de-creases in reptiles. It is good from hydro-

cyanic acid? I think not: and my objections to it are much the same as those against opium—viz., that it tends to exalt, and not to lower the polarity of the nervous system. Those of you who have seen the death of an animal from hydrocyanic acid will at once appreciate this objection. You will remember the violent epilepsy—the tetanic epilepsy, if I may call it so,—the combination of tonic and clonic spasm, under which the animal suffers.

The use of galvanism has been proposed in cases of this description for the purpose of depolarizing the nervous centres, and has actually been tried by Matteucci. The long-continued passage of a current of galvanism tends to weaken and to paralyse a nerve or nervous centre, if the direction of the current be the same as that of the nervous force. In the case related by Matteucci the spasmodic condition of the muscles completely subsided during the passage of the electric current.

Another part of our treatment was the application of cold over the region of the spine. Cold has a powerful effect in depolarizing the spinal cord. I have tried it with great advantage in this hospital in tetanus, in laryngismus, and in the convulsions of children. The best plan is to fill a bladder with some broken ice, and apply it directly to the spine: an intense degree of cold may be thus obtained in a very short time, and its effect on the circulation will soon be manifest, cold having a very powerful influence in diminishing both the force and frequency of the heart's action: for this reason you must not apply it too long, or too much of it; you must watch your patient, and remove it and reapply, as his condition shall indicate. The cases in which I have tried it I have found manifestly benefitted by it in diminishing the intensity of the spasms.

With regard to internal remedies, I know of nothing better than small doses of opium, belladonna, and conium. The two latter drugs certainly seem to me to have a depolarizing power, as is sufficiently obvious from their influence, especially that of belladonna, in causing dilatation of the pupil. Still, a good depolarizing agent which does not tend at the same time to weaken or depress the powers of the patient, is a great desideratum, and when discovered will no doubt prove an invaluable remedy in the treatment of tetanus. What, however, in the absence of such a remedy, the physician has principally to trust to is to support, in order that he may gain time, to enable his patient to weather the storm, and to sustain the attacks of his disease till the source of irritation shall have ceased to exist, and its consequences have abated.

I must not conclude without calling your

attention to the great importance of isolating tetanic patients, and protecting them, not only from the excitement caused by many persons surrounding the bed, but also from the influence of draughts or currents of air, or external mechanical stimuli, which are peculiarly prone to excite the tetanic spasm. These patients ought always to be put into a separate ward, and visitors excluded as much as possible. The temperature of the room should be kept at a moderate degree, and care should be taken to exclude sound, irregular currents of air, and light, as far as it can be conveniently done.

MEDICAL EDUCATION IN PHILADELPHIA— THE FEMALE MEDICAL COLLEGE.

THE New York Medical Gazette informs its readers that this College has commenced its session, and, as we learn, some forty or fifty *women* were in attendance as students, a much larger number of the sex having been present at the introductory lectures. The Faculty are all males, and, we learn, are pledged to give place to females as soon as their fair pupils shall, under their instructions, become qualified as teachers in the several departments. The gentlemen occupying the several chairs at present are plain men of mature age, and appear to appreciate the novelty and difficulties of their undertaking, in which, however, they seem to be very soberly in earnest. We have heretofore expressed our misgivings in regard to the *morale* of this enterprise, but we see that "Professor Longshore," a prominent member of this Faculty, announces as the subject of his introductory lecture to *female students*, "*The immorality of obstetrical practice being conducted by men*." He seems to have overlooked the "*immorality*" of his own proposition with his male colleagues, to demonstrate to *women* the healthy and morbid anatomy of the sexes, and discourse before wives, widows, and maidens, old or young, upon topics in relation to which an inspired authority has taught that "they should not be even once named among you;" obviously meaning in a promiscuous assembly.

We cannot envy the task self-imposed by these teachers, and we cannot refrain from the expression of our pity for their infatuated pupils. We opine that both the one and the other will become heartily ashamed of their imbecility and folly, and repent hereafter for their mispent time and labour, if no worse mischiefs result from their association as teachers and pupils. We saw in their museum objects upon which no modest woman can look without a blush in the presence of the other sex, nor any virtuous maiden study under the teaching of men without mutual impurity and moral deterioration.

Original Communications.

A BRIEF SKETCH OF THE MORE IMPORTANT
FLUCTUATIONS IN OPINION WHICH
HAVE PREVAILED AMONG
PRACTITIONERS OF
MIDWIFERY

WITH REFERENCE TO THE PERFORM-
ANCE OF TURNING;

AND THE

APPLICATION OF THE FORCEPS
IN CASES REQUIRING
ARTIFICIAL DE-
LIVERY,

ON ACCOUNT OF DEFORMITY OF THE
PELVIS.

By CHARLES WEST, M.D.

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Hospital, and Lecturer on Midwifery in the
Medical College.

It is almost superfluous to remind the reader that, previous to the invention of the midwifery forceps, there were but two proceedings generally adopted in cases where either the protraction of labour, or the supervention of some symptom threatening the well-being of the mother or her child, indicated the propriety of artificial delivery. These two proceedings were *embryotomy* and *turning*; for even the *Cæsarean section* had been at that time so rarely performed that it scarcely took rank among the recognised operations of obstetric surgery.

Since the re-discovery of the operation of turning by Ambrose Paré, it had been the constant endeavour of practitioners of midwifery to bring that operation to perfection. By degrees they extended the number of cases in which they resorted to it; and at length, practice having given many a great dexterity in its performance, they employed it in numerous instances of head presentation, where either feeble uterine action, or mechanical disproportion, retarded the labour, as well as when the occurrence of hæmorrhage or convulsions indicated the necessity for speedy delivery. Of all who distinguished themselves by thus extending the operation of turning, and reducing the performance of *embryotomy* to a minimum of frequency, La Motte is unquestionably the

most remarkable; and he closes worthily the list of those who, before the forceps were invented, signalled themselves by their endeavours to bring the obstetric art to perfection.

At the time when this new instrument was introduced into midwifery practice, its exercise both in England and Germany was to a very great degree in the hands of women; and its principles and practice were, as might be expected, more barbarous than in France, where it had already, for fully half a century, been customary in the better ranks of society to employ men not in difficult cases merely, but also in cases of natural labour.

In England, the first practitioner (after the Chamberlains, who made a secret of their discovery) who employed the forceps in practice, seems to have been a Mr. Drinkwater, of Brentford, who died in the year 1728. Giffard, who died only three years later (1731) also employed it; but the first public printed description of the instrument, and its mode of application, was given by Chapman, in the year 1733. English practitioners employed the forceps as one means of carrying on the war which at that time they were waging with the midwives. Proof of this is not only given us incidentally by Sterne, in his account of all the mishaps that attended upon Tristram's birth, but it is even apparent in Smellie's writings, who speaks of the practitioner as applying the instrument out of the nurse's or midwife's sight, beneath a sheet which was thrown over the bed, while its corners were sometimes pinned to the shoulders of the operator.* It was the grand *cheval de bataille* of the doctors—a means unknown for some time at least to the midwives, and the introduction of which had a great share in driving the latter from a very lucrative kind of practice. It is not to be wondered at that, under these circumstances, the forceps came into general use in England, and that there was comparatively little inquiry as to whether any class of cases would still be better treated by the old mode of turning than by the new one of applying the forceps.

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seen at p. 50-51 of the Introduction to his Treatise on Midwifery. The great caution which Smellie observed in advising the employment of the long forceps, and his general recommendation of his short forceps only, coupled with his discountenancing the operation of turning in cases of difficult or protracted labour, had no doubt a great share in bringing about that frequent employment of embryotomy which was, and is still, in a measure, a characteristic of British practice. The quarrel between Smellie and his assistant Mackenzie, which ended in their separation, probably contributed still further to bring craniotomy into practice, since Mackenzie, influenced to some degree perhaps by his quarrel, decried the forceps, which Smellie, on the contrary, recommended. W. Hunter's teaching and practice tended in the same direction; and the just opposition which he made to bringing down the feet in breech cases led him still further to oppose turning in cases of difficult labour, and prepared the way for the general spread of those doctrines of which Dr. Osborne may be taken as the great representative and advocate; for though he recommended the forceps in preference to the lever, yet the tendency of his teachings was to increase the frequency of embryotomy.*

The influence of these teachings, however, was not so extensive but that many practitioners, long after the invention of the forceps, continued to turn in many cases of difficult or protracted labour. Thus, for instance, Dr. Baudenell Extor, who was physician accoucheur to the Middlesex Hospital, and the third edition of whose "Midwifery," published in 1753, is now before me, fancied that, when the head was low down, he could give safer and more efficient assistance with his hand than with forceps; and that, when the head was higher, it was preferable to deliver by turning. Mr. Pugh, of Chelmsford, whose book bears date 1754, though he invented a pair of curved long forceps, and gives very good directions for their application, yet recognised the existence of some cases of protracted labour in which turning was

to be resorted to: he mentions especially, at p. 78, that when the pelvis is too small or distorted, the head hydrocephalic or very much ossified, or its presentation wrong—"In all these cases, provided the head lies at the upper part of the brim, or though pressed into the pelvis, it can without violence be returned back into the uterus, the very best method is to turn the child and deliver by the feet, according to the directions already given." He then goes on to lay down the conditions which would induce him to prefer the curved forceps, and states (p. 77) that, as the result of these two modes of assisting the parturient woman—"I have never opened one head for upwards of fourteen years."

It will probably suffice to mention two more writers of this period, as showing that the introduction of the forceps into practice did not prevent many from duly weighing the advantages, or supposed advantages, of turning. Dr. Fielding Ould, in his Treatise on Midwifery, published at Dublin in 1742, while he speaks at p. 153 of "the large forceps, which is in general use all over Europe," and gives very sound directions as to its employment, at the same time suggests (p. 86-87), though he confesses that he never practised, the turning the child early in labour, and extracting it by the feet, in any case where the contraction of the pelvis had in a previous labour "refused an exit to the child (though not of an extraordinary size) by means of the common efforts of nature; and that on this account it died, or was destroyed by instruments, for the preservation of the mother's life."

He notices the objections that may be urged to this proceeding; but alleges that, though it is an interruption to the usual course of nature, still it is a less objectionable interference than would otherwise become necessary. "Again, it may be objected that the same narrowness of the passage through the pelvis which hindered the natural expulsion with the head foremost, may hinder its extraction when brought forth by the feet: this is also allowed; but yet, if we consider the matter properly, it will appear that, by drawing from a small end, which is the feet, in order to bring forth the larger, with the additional assistance of holding the legs in one hand, and having the finger

* Much interesting information as to the opinions and practice of the leading accoucheurs in London at the end of the last century will be found in Fischer, *Bemerkungen über die Englische Geburtshülfe*; Göttingen, 18mo. 1797.

of the other in the child's mouth, there is a far greater probability of bringing it forth than when the large end comes first, and that without any probability of assisting the mother's efforts but by the destruction of the child."

Burton, whose work on midwifery was published in the year 1751, advises, on the same grounds with Ould, the same proceeding, and speaks as if he had adopted it. To the directions, however, given by Ould, Burton adds at p. 168,—"and then turn the chin so as to pass the bones into the pelvis in the most commodious manner."

Enough has already been said to show that practitioners in this country were not blind to the advantages which turning might be supposed to present in some cases of protracted labour. It fell, however, more speedily into disuse in this country than in many parts of the continent, owing, in measure, probably to the same influences that have already been referred to as stamping a peculiar character upon operative midwifery in Great Britain, but also still more, in all likelihood, to the fact mentioned by Dr. Denman,* "That this practice was in general very unfortunate in the event, as I have been assured by some who have used it;" "yet," he adds, "cases may occur in which, by turning the child, the chance of saving its life is greater than can be gained by the use of any instrument." He then relates a case in which by adopting this proceeding he delivered a woman of her eighth child alive at the full period, all her other children having been still-born, and he himself having delivered her of her first two by means of instruments, which the deformity of her pelvis rendered necessary. This case is further interesting, from the circumstance of the left parietal bone having presented a depression, an inch deep, caused by the projection of the sacrum, which, however, by degrees disappeared. The woman recovered, without any untoward circumstance.

Dr. Denman concludes this section of his book, with the following estimate of turning under such circumstances:—

"But the success of such attempts to preserve the life of a child is very precarious; and the operation of turning a child under the circumstances before stated is rather to be considered among

those things of which an experienced man may sometimes avail himself in critical situations, than as submitting to the ordinary rules of practice."

Dr. Dewees,* the great obstetric authority in America, devotes a section of his work to "Turning in a deformed pelvis, as a means of saving the child's life." He decides that less than 3½ inches in the antero-posterior diameter of the pelvic brim would give no chance to the child, and even that size but a very slender one; though it may be right to adopt it when the practitioner feels hesitation as to the propriety of perforating, since "it gives a chance, though a forlorn one, to the child." He notices the different conditions essential to the successful issue of turning as far as the child is concerned, and remarking on the little likelihood of their being fulfilled when the pelvis is contracted, concludes, in words not unlike those of Dr. Denman, "that it must ever be looked upon as a doubtful alternative, rather than a probably safe resource."

Notwithstanding the evidence which La Motte's writings afforded of the successful performance of turning in many cases of protracted and difficult labour, the introduction of the forceps into France seems to have brought it even more into disuse than in England. The circumstance of Levret's forceps, being longer even than Smellie's long forceps, and that the former eminent man did not, like his English cotemporary, either employ or recommend a shorter instrument, probably had a great influence in bringing this about. With Levret's forceps practitioners were able to interfere while the head was still high in the pelvis; and if such interference failed, they had recourse to the perforator. Levret's protracted career, his wealth, and his position as accoucheur at the Court, all tended to give to his doctrines and practice a very great weight; and he disapproved of turning in cases where the pelvis was contracted, though he advocated its performance early in labour in cases of head presentation in which the head presented in an unfavourable position.† Levret survived till the year 1780, at which time Baudelecque was thirty-four years old, and had already distinguished himself by

* System of Midwifery, 8vo. Philadelphia, 1823; p. 546-548.

† *L'Art des Accouchemens*, p. 134-35, 3ième ed. 8vo., Paris, 1768; and *Suite des Observations sur les Accouchemens Laborieux*, Article 2r.

* Walter's edition, p. 268.

his opposition to the practitioners of Sacombe's school, and to the advocates of the Sigaultian operation. The influence which Levret had exerted was exercised by Baudeloque, and in great measure continues even at the present day; and he not only held doctrines concerning obstetric operations similar to those of Levret, but, by adding two inches to the length of the forceps, sought to make that instrument even more universally applicable than it had been in the hands of his predecessor. Still, Baudeloque, so far from rejecting the operation of turning, expressly sanctions it in some cases of head presentation. "The bad conformation of the pelvis sometimes prescribes it; but almost all those who have advised and practised it on this account have not taken into consideration as much as was necessary the degree of disproportion between the dimensions of the head of the child and those of the pelvis of the mother: so that for one whose life has been saved by this means a great many others have been destroyed. This method is at best suitable only for those cases in which the want of space that interferes with the patient's delivery is but slight; when it is more considerable it necessitates the employment of the forceps, of crotchets, or even the performance of the Cæsarean section, &c., as will be shown in the fourth part of this work."

"It is not for the sake of adding to the expulsive efforts of the patient by dragging at the feet of the child, and afterwards at the other parts which precede the head, as many accoucheurs do, that the operation of turning should be undertaken, when the contraction of the pelvis, though but very slight, prevents the head from entering it. The peculiar structure of the foetal head indicates plainly enough the principles in accordance with which this operation can be performed with hope of success. The structure of the head is such, that it collapses more readily in proportion to its dimensions, and enters the pelvis with greater facility when the child comes footling (provided it is directed properly), than when the head is the presenting part, though a great number of accoucheurs think the contrary, and are of opinion that it then presents its widest diameter at the pelvic strait.*

It is needless, for the further elucidation of his views, to carry these extracts further, though the next fourteen pages are occupied with directions as to the mode of performing the operation of turning in cases of head-presentation.

Madame Lachapelle, however, so far from acquiescing in this merely occasional and exceptional substitution of turning for the forceps, in cases of contraction of the pelvis, gives it decidedly the preference. After pointing out the direction in which it would be desirable that the forceps should grasp the foetal head in cases of pelvic deformity, so as to accommodate its smallest diameter to the contracted antero-posterior diameter of the pelvis, she observes* :—

"It is doubtless to the greater facility with which we can direct the head of the foetus, and to the slighter compression of this part, that must be attributed the greater frequency with which, contrary to the opinion of many authors, I have succeeded in cases of this description by performing the operation of turning than by the application of the forceps. As may be seen by a reference to the tables already quoted,† of 15 children extracted by the forceps on account of a contracted state of the pelvis, 8 were still-born, 7 were born alive; while of 35 delivered footling, 16 were born alive, and only 9 were extracted without any sign of life. The proportion of cases in which turning proved successful amounted, then, to two-thirds, while the forceps succeeded in less than half." Madame Lachapelle then specifies, as another advantage of the operation of turning, "that it affords the means of ascertaining whether the child is living or dead, and consequently supplies data that may decide us to prefer, in a case where its extraction is found impracticable, either synchondrotomy or perforation of the head. With moderate care, the separation of the head from the body will not occur, and a crack, such as that which announces the giving way of the spine, sometimes takes place as the result merely of the sudden yielding of the bones of the cranium, and of the consequent descent of the head into the pelvic cavity; when, it need hardly be observed, it is a good sign, not a bad one."

* *Pratique des Accouchemens*, Paris, 1825, 8vo. tom. iii. p. 429-30.

† The tables appended to vol. i. and ii. of this work.

* *L'Art des Accouchemens*, p. 57 2-73, 7ième ed. Paris, 1833.

Finally, she takes leave of the subject with a recommendation of the operation of turning in cases of contraction of the transverse diameter of the pelvic outlet, as being preferable under such circumstances to the application of the forceps.

I should now proceed, as briefly as possible, to relate the opinions which have at different times been entertained with reference to the performance of turning in cases of pelvic deformity in Germany, were it not that some mention of M. Velpeau's opinions and practice becomes necessary, owing to an oversight on the part of some gentlemen of very high and deserved reputation who have coupled his name with the first adoption of this operation under these circumstances. In speaking of those cases of pelvic deformity in which the contraction is confined to one oblique diameter, M. Velpeau notices* how the same woman may in one pregnancy have a very easy, in another a very difficult labour, according as the head entered on the former occasion in the uncontracted, on the latter in the contracted diameter of the pelvic brim. In such cases the operation of turning affords a very easy way of overcoming the obstacle, since it allows the operator to direct the child's head into the most favourable position; of which a case that occurred in his own practice is cited as a very good illustration. In adopting this proceeding, however, M. Velpeau does not lay claim to any originality, while his own opinions, expressed in other parts of his work, show him to be on the whole opposed to the performance of turning in cases of head presentation. He remarks,† that practitioners differ as to the course to be pursued in cases where the head is still moveable about the pelvic brim. "Some, as Levret, Smellie, Plenk, and especially Flamant, are of opinion that the forceps offers greater advantages than the operation of turning. Others conceive with Madame Lachapelle, Desormeaux, and almost all modern writers, that the opposite is the case. Both, I conceive, are to a certain extent in error." He then points out the nature of these errors; his own view of the matter being that the forceps are safer for the child, the operation of turning the less hazardous

proceeding for the mother. He sums up thus:—"The wisest course lies between these two extremes; to prefer the forceps, if the practitioner is dexterous in employing them, if no great difficulties interfere with their application, and there be no risk of injuring the woman. On the other hand, under opposite circumstances, the operation of turning is to be preferred; that is to say, when the head is too high, or too moveable to be readily grasped by the forceps, and when no obstacle exists to the performance of turning."

The case of contraction of the oblique diameter of the pelvis being the only one in which M. Velpeau counsels the operation of turning as a means of overcoming the difficulties arising from pelvic deformity, it is perhaps not unfair to regard him, after the manner in which he expresses himself concerning this operation in the passage just quoted, as generally unfavourable to its employment in cases of disproportion.

Although at the present day more frequent use is made of the midwifery forceps in Germany than elsewhere, yet that instrument did not at first obtain by any means such ready acceptance there as either in England or France. Obstetric practice in Germany was, during the first half of the eighteenth century, almost entirely in the hands of midwives, who were accustomed to perform all except instrumental operations, and to treat the diseases of pregnancy, of the puerperal state, and of infancy, appealing but seldom to medical men. Ignorance on the part of the latter was the unavoidable result of the want of opportunities for observation,* while the sound teaching of such men as Stein and Roederer exerted of necessity a much narrower influence, from their positions in the universities of small states as Hanover and Hesse, than that of Levret did in France.

The forceps at first introduced into Germany, though known as the "English forceps," were the straight forceps of Grégoire,† and their use was limited,

* *L'Art des Accouchemens*, 8vo. Paris, 1835, tom. i. p. 35-39.

† Tom. ii. p. 260.

* See the account of the condition of midwifery in Germany at this time, in Oslander's history of his own medical education, contained in his *Neue Denkwürdigkeiten*. 3te Bogensahl: 8vo. Göttingen, 1799: i. p. 1. And in v. Siebold's *Versuch einer Geschichte der Geburtshülfe*, a work which displays profound learning, acute criticism, elegant taste, and the most praiseworthy impartiality. The publication of a good translation of it would reflect honour on the Sydenham Society.

† Siebold, op. cit. Vol. ii. p. 233.

as from Puzos' own expressions* would seem to have been the case also in Paris before the time of Levret, in which the head was situated quite low down between the rami of the pubes. The publication of such writings as those of Deisch and Mittelhäuser, in which embryotomy was recommended and had confessedly been repeatedly performed by themselves on most inadequate grounds, while it proves to how great a degree the uses of the forceps were unknown, may account also, by the revulsion of feeling which this barbarous practice excited, for the disinclination to all kinds of instrumental interference which seems to have existed in Germany during many years after the forceps were in general use in France and England. No one could be better fitted than Roederer, the pupil of Levret and Smellie, for the task of laying down right principles in operative midwifery; of his merits in which respect his *Opuscula Medica* afford more abundant evidence than his short *Elementa Artis Obstetriciæ*. The tendency of his teaching was to inspire confidence in the use of the forceps, to introduce them into practice in cases of irregular positions of the head, or sometimes to substitute for them the lever; at the same time to define the conditions in which embryotomy was justifiable, and while advocating turning in cases of transverse presentation, in which Mittelhäuser had very often most unwarrantably performed embryotomy, to restrict the frequency of its employment as a means of terminating protracted or difficult labours. He did not, however, institute that formal comparison between the merits of the forceps and turning with which his pupil Stein began his career as a public teacher. In the year 1768, he published as a sort of preface to the announcement of his lectures, an essay "*De Versionis negotio pro genio Partus salubri et noxio vicissim*;" in which he treats of the contra-indications to the operation, and points out the superiority of the forceps of his own instructor Levret, in the management of some of those dangerous complications of labour for which turning was generally practised. Among the conditions which, in his opinion, should forbid the performance of turning, he especially dwells

upon the existence of disproportion between the head of the child and the pelvis of the mother.* He concedes the value of Levret's suggestion in those cases for bringing the small diameter of the head into the small diameter of the pelvis, and directing the large diameter, contrary even to what might have been the natural course of the head, into the large diameter of the pelvis; but yet there are instances in which even this is unsuccessful, and then the use of the previously despised forceps becomes unavoidable, or perhaps even the perforator and cruetet are found to be necessary to the patient's delivery. He concludes, that where the want of space depends on the greater size than usual of the head, or the narrowness of the pelvis, provided this be not extreme, the forceps will usually suffice to effect delivery, even though the head had not entered the pelvic brim; while when the forceps cannot suffice, either craniotomy, or, in extreme cases, the Cæsarean section must be resorted to. The experience of the ensuing eight years only deepened the conviction expressed in this essay, and in 1773 he published another. "*De præstantia Forcipis ad Servandam Foetus in Partu Difficili Vitam*." In the second paragraph of this dissertation he proposes the question, to which he intends to reply, in these words:—"Whether, in a difficult labour, a greater number of children are or can be saved by means of Levret's forceps, or by the hand alone,"—i. e., by turning? This inquiry, he says, is one of great moment, and the off-hand decision of which in favour of turning, indicates both ignorance and presumption, for it is by no means every person who is competent to reply to it. Neither, indeed, is it by any means a matter of indifference which of these two modes of effecting a patient's delivery is selected; but the good of mankind, and the interests of the obstetric art, alike require that a positive decision be arrived at. He himself endeavours to answer it, as he says, partly by *a priori* reasoning, partly by inference from facts observed. He sets

* This and several other dissertations by Stein were translated into German, and, with some short essays originally published by him in that language, were collected by him into one volume, under the name of *Kleine Werke zur praktischen Geburtskunde*, 8vo. Marburg, 1798; in which see p. 366-70, §§ ix. x. for Stein's opinion concerning turning in the deformed pelvis.

* *Traité des Accouchemens*. Paris, 4to., 769, p. 131.

out by showing, that while the most natural of all cases of labour are those in which the head presents, it yet is not desirable to interfere with the course which nature may in any case adopt, so as to convert a case of footling presentation (though confessedly unfavourable) into one of head presentation, albeit that is the most favourable. If, then, it be unwise to interfere for the purpose of converting the worse into the better, it must be still more injudicious to interfere with the view of converting the better into the worse. This somewhat strange theoretical reasoning he supports by the allegation, that from the very structure of the head, it must pass the pelvis more easily when it comes first than last; for while* in the former case the pelvic brim is occupied by the yielding vertex, the different parts of which are united by sutures, it is in the other case filled by the unyielding basis cranii: so that it may be fairly concluded that the child which would be born with difficulty with its head presenting would be born with far greater difficulty if the head came last. Further, the dangers to the child are much greater in footling cases than in those where the head presents. To prove this, he publishes a list of all cases in which, during the previous ten years, he had either applied the forceps or turned, excluding from his reckoning all instances in which the delivery was premature, or the child, before the performance of the operation, was certainly known to be dead. This list gives a total of 54 forceps cases, in which 41 children were born alive, and 13 born dead; while 66 cases of turning yielded 33 children living, and the same number still-born.

Of all the pupils of Stein, the elder Oslander is he who exercised the greatest influence upon the practice of midwifery in his own country. With great, probably too great, fondness for operative interference, he entertained to the full his teacher's preference for the use of the forceps in all cases where disproportion exists. That this opinion, too, was not adopted without full deliberation, is sufficiently attested by the very carefully written chapter in his work on midwifery "upon the Influence

of the Forceps, and of the Operation of Turning upon the Mother and Child." In a note to this chapter,* and more fully in another work, he mentions that between the years 1782 and 1792 he applied the forceps 39 times, and had 78 cases, either footling from the outset, or in which he performed the operation of turning. In 33 out of the 39 forceps cases the children were born alive; while of the other 78 cases only 37 yielded living children,—a fatality indeed, which, he says, greater dexterity obtained by practice in subsequent years enabled him to diminish, though it does not appear to have modified his opinions. He did not, indeed, himself complete his large work on midwifery, but his principles are laid down most positively in a condensed form in his *Neue Denkwürdigkeiten*. In the last volume of this work, published in 1799, he sanctions, p. 105, the performance of turning, provided the head be still high up, and the pelvis well proportioned; but he adds, that the indications are very different if the pelvis be contracted. "If the pelvic brim be considerably contracted, turning ought not on any account to be performed, for it will certainly be attended with great difficulty, and lead to a bad result; and at the very best, the life of the child will almost certainly be lost." Under these circumstances, then, he advocates the use of the forceps; but if the brim of the pelvis be capacious, and its outlet only contracted, he counsels the operation of turning, coupled with the subsequent application of the forceps if necessary. "The reason," he says,† "why the head can thus be more readily got through the pelvis is, that when it enters the pelvic apertures in this position, with the chin depressed upon the chest, it forms a wedge whose apex is directed towards the cavity of the pelvis; while at the same time, by holding the feet, and making traction by them, the head can be firmly fixed, and thus prevented from slipping out of the grasp of the forceps."

Oslander's contemporary, and the most formidable opponent of his somewhat too meddling practice, the late Professor Boer, of Vienna, expresses

* The meaning of the writer is here somewhat obscure, but I believe that I have stated it correctly. See § 10 of the German translation, which alone I possess.

† *Handbuch der Entbindungskunst*, 2te Aufl. Tübingen, 1830; 2ter Band, S. 363; and *Neue Denkwürdigkeiten*, 1ster Band, 2ter Bogenzahl. p. 28, Göttingen, 1799.

† *Neue Denkwürdigkeiten*, § 119, p. 282.

himself at the close of his active life concerning turning in cases of pelvic deformity, in the following decided manner:—

"Ne itaque, quin necesse sit, infantem convertas; tum vero minime, ubi non sat justa pelvis spatia habet. Vesano certe consilio capite prævio, at prægrandi, vel quod ad idem redit, pelvi angusta atque ex hac molesto nixu, versio patrat. Tale facinus infanti ac matri numquam non exitio est."*

As the medical literature of Germany is probably not so readily accessible to all members of the profession as that of our own country and of France, I will just add, that the doctrine at present taught there with reference to this point is,† that turning may be employed with advantage in cases where there exists a moderate degree of pelvic deformity, coupled with not very active pains (which have proved inadequate to drive the head through the pelvic brim), and a dilated state of the os uteri. The younger Oslander has most fully considered this practice, and the objections that have been raised to it, and recommends‡ it as more generally applicable than the majority of his countrymen are disposed to admit, since he is inclined to practice it in many cases as a substitute, not merely for the forceps, but, after they have been unsuccessfully tried, as a means of avoiding the use of the perforator. He denies the great danger or difficulty of pushing back the head, and turning the child, after the forceps have been ineffectually applied, and states that he has under such circumstances turned with facility. He denies that the difficulty in extracting the head when it comes last is as great as when it presents, and appeals to general experience in proof of this statement. "The reason why the child which could not be brought into the world while its vertex presented can be delivered after it has been turned, ap-

pears to be, that when the head presents, its broader part has to be drawn through a narrow passage without its being held fast, except at its sides; while, on the other hand, after turning it offers the under part of the face and the neck like the narrow end of a wedge, and thus its smaller part, coming first, can be firmly held on either side by the forceps, and, if need be, can have additional force applied to move it, by drawing at the lower jaw, and by pressure on the back of the neck and shoulders."*

"That by this proceeding the life of the child may be destroyed is fully admitted, but at least," says he, "it gives a chance which embryotomy at once destroys."

In addition to those cases in which turning may be resorted to after the application of the forceps, he recommends it as a proceeding to be adopted from the first under the same circumstances as those in which others of his countrymen adopt it, though he does not agree with them in admitting the fruitlessness of long continued uterine efforts as contra-indicating its employment. He moreover advises it in cases where previous tedious labours have issued, after much suffering, in the birth of dead children, and considers that under any of these circumstances the existence of any irregular position of the head renders this proceeding the more imperative.

In the foregoing sketch my object has been, avoiding all occasion of controversy, to acquit myself faithfully of the duties of a historian. My reason for undertaking this office was, that not a little of the asperity which has, unfortunately, entered into recent debates in this country concerning the propriety of turning the child in cases of pelvic deformity, seemed to me to be due to the fact of the proceeding having been regarded, both by its advocates and its opponents, as or almost altogether novel, instead of being one which has engaged the attention of some of the most competent observers and best practitioners ever since the invention of the forceps, and has employed their pens, either in setting forth its advantages, or in exposing its evils.

* De Obstetricia Naturali, p. 223, 8vo. Vienna, 1830.

† Siebold, Lehrbuch der Geburtshülfe, § 518, S. 414, 8vo. Berlin, 1841; Killan, Operationslehre, 2te Aufl. Band. i. S. 326, Bonn, 1849; Wilde, Das weibliche Gebärungsvermögen, S. 250, 258, 8vo. Berlin, 1831. The size which the pelvis ought to have to justify this operation is not estimated alike by all writers. Oslander and Siebold take three inches as the minimum; Killan three and a quarter to three and a half.

‡ Ursachen, u. Hülfsanzeigen bei schweren Geburten, 2te Aufl. §§ 119-121, S. 208-214, 8vo. Tübingen, 1833.

* Lib. cit. p. 212.

THE DETECTION OF SEMINAL FLUID IN THE URINE IN CASES OF MASTURBATION.

MICROSCOPICAL APPEARANCE OF THE ZOOSPERMS.

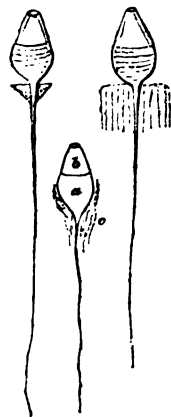
By THOMAS INMAN, M.D.

A FEW days since I received from Dr. Brett a specimen of urine for microscopical examination. It was lightish in colour, somewhat turbid, of sp. gr. 1018, did not answer to the test for sugar. It had a somewhat peculiar smell; and, after keeping a few days, was loaded by a vegetable growth like the torulæ of diabetic urine. It showed little tendency to putrefaction,—so little, that some which is now twelve days old, and has been kept in a warm room, exposed frequently to the atmosphere, smells perfectly natural, and is without any ammoniacal odour. Another specimen has been kept in an open wine-glass for about a week, and is still sweet.

On making a microscopic examination of the fluid, I found that it contained, in addition to the torulæ, a number of large fragments of epithelium adhering together, and evidently large exfoliations of very large tubes. There was also an immense number of organic granules, which I at first considered might be pus.

The general characters of the fluid not being those of a purulent urine, I made a closer investigation, and at last found a number of spermatozoa. The urine was spermatic! and the case was most probably one of extreme masturbation.

This was not, however, the most interesting discovery I made; for I was enabled most unequivocally to see the relation between the so-called animalcules and the seminal granules. Before detailing this I may mention that I have long been dissatisfied with the figures given of the zoosperms, inasmuch as they never show any caudal appendage. Now I have found, in an immense number of instances, that there is a peculiar formation at the base of the tail. This is so common that I consider it the exception when the animalcules are deficient of it. It consists of a delicate transparent membraniform substance, which may readily be detected by a good $\frac{1}{4}$ inch and a low light. It may be thus figured:—



a solid body; b sac; c caudal appendage.

This appearance could only be accounted for in my own mind by the supposition that each zoosperm was developed in a cell, the tail protruding like a cilium, until the emission took place, when all the zoosperms were torn from their attachments, the other contents of the cell going to swell the amount of the secretion discharged, and that the granules were probably immature cells in which the zoosperms were undeveloped. This idea of the cell origin of the animalcule received its strongest confirmation from the fact that the appearance of No. 1 was the most common,—when it was evident that a fragment of *something* or other was carried away; and that *something* could only be a continuous membrane or a cell: everything went to disprove the probability of the former.

I was then diligently on the look-out for granules in the transition state; but my search was unsuccessful until I examined the above-mentioned specimens. I found in one of them a *granule containing a zoosperm*, of which the following is a sketch:—



It rolled over during the investigation, so that I could convince myself that the zoosperm was not simply an adherent one. Another granule was in a transition state, neither the tail nor the head of the zoosperm being fully formed: thus—



The size of these granules was about ~~over~~ of an inch.

I have been able to verify these observations on a subsequent specimen of spermatic urine from another source, and have now seen the appearance described in at least four separate cases.

I find, on reference to Todd's *Cyclopædia of Anatomy and Physiology*, that two observers have noticed analogous phenomena in the spermatic secretions of the dog and of the rabbit, and that Dujardin and Kölliker have noticed the caudal appendage. I have not seen the drawings of the former, and therefore cannot say whether we refer precisely to the same things. I believe no observer has yet noticed them in the human semen. The authors of the article consider that the appendage may be due to matters accidentally deposited by the urine, in which fluid the phenomena are usually investigated. This can scarcely be, as they are to be found equally in that fluid when freshly passed. The fact appears to be, that the spermatozoa can never be examined with so much satisfaction as when they are expelled with the contents of the bladder. After an emission a certain quantity of semen remains in the urethra. This is exposed to little violence, as it is not so readily expelled as the first quantity evacuated. This residuum is washed out by a stream of fluid, and the animalcules are never subjected to anything likely to destroy their character up to the time of their being placed in the field of the microscope.

Liverpool, Nov. 1850.

A CASE OF
PUERPERAL UTERINE PHLEBITIS,
WITH REMARKS.

By W. B. KESTIVEN, M.R.C.S.

Much has been written on the subject of puerperal fevers; the following case is intended as a contribution to its history. It is one only of the facts which constitute that history.

Mrs. A—, over thirty years of age, a native of New York, had accompanied her husband to England shortly after her marriage, and was pregnant with her first child; her health had been good throughout the period of gestation, and although having a pale and exsanguine aspect she stated that she had never before during her life required medical advice, that she had never before spoken professionally to a medical man. She was a well-made woman, and presented an ample pelvis: everything, therefore, promised a fair labour, except the fact of her having passed more than thirty years—how many more than thirty I could not learn—in the virgin state. I felt unusual confidence in endeavouring to dispel the ordinary despondency of pregnant women approaching the time of delivery, which in the present instance had obtained an unusual hold upon the patient.

Labour commenced on the 11th March, 1850, at eleven o'clock in the forenoon, and it was found that we had to deal with rigidity of the os uteri to begin with, and fair promise of a similar condition of the external organ afterwards. However, by dint of patience and other means, the dilatation of the os uteri was got completed by eleven of the forenoon on the 14th. The head having passed into the vagina remained just within the outlet, where it was detained, as had been found by the unyielding rigidity of the external parts. At eight o'clock in the evening she was seen by Dr. Robert Lee, who fully concurred in the proposal that the child should be removed by operative interference. The pains, although strong and regular, were clearly incompetent to the expulsion of the child, except at the expense of the soft parts.

The forceps were applied experimen-

tally merely; their use confirmed the suspicion that the life of the child must be sacrificed. Craniotomy was forthwith practised, the labour finally terminating at half past ten o'clock of the same evening. The placenta was easily and entirely cast off.

The patient went on favourably until the 22nd of March, with the exception (scarcely noticed by herself) that during this time she had not slept soundly. Her nights were rather restless, and disturbed by dreams. My fears were excited by this circumstance, but the closest watching detected no other bad symptom until the 22nd, the sixth day: when, while I was standing by her bedside, she had a severe and long-continued rigor. This returned also less severely on the night of the 22nd—23rd, but no other bad symptom had yet shown itself.

On the 23rd, the patient acknowledged to no pain, but stated that she had an unpleasant feeling of heaviness of the head and confusion of ideas. The skin had become hot and dry, the bowels confined, the tongue furred; the pulse had risen to 140, and was small. Castor oil, and a mixture of Liq. Ammon. Acet., with Dover's powder, were prescribed.

24th.—Passed a restless night, at times delirious; has the same sense of confusion; the countenance has assumed a peculiarly sharp and anxious look. Bowels freely purged; the skin moist and less hot; the tongue furred, the papillæ at its apex red and enlarged; the pulse 136, small; has no pain or tenderness in the abdomen or hypogastrium.

Emp. Canth. Nachæ. Calomel and Dover's powder. Towards evening of this day the pulse fell to 100, and was soft, the skin moist and cooler.

25th.—The symptoms continued much the same, with these important exceptions, that on this day she first acknowledged pain and tenderness in the hypogastric region, and the lochial discharge had considerably decreased; at the same time that she expressed herself "much clearer in the head." Painful forcing sensations in the vagina and rectum were felt, and the hypogastrium had become exceedingly sensitive to pressure. No pain in the groins or thighs. Calomel, Opium, and Leeches.

26th.—Has slept at short intervals. mind clear. The pain had increased, and --

man, which was tympanitic. The countenance flushed and more anxious; no headache; the tongue thickly covered with a brown fur; great thirst; the skin pungently hot; pulse 160, feeble; bowels confined; urine scanty and high coloured; lochia entirely suppressed.

The patient was seen again this day by Dr. Robert Lee. Calomel and opium continued; leeches repeated.

27th.—The leech bites have bled freely; considerably difficulty was experienced in arresting their bleeding. The pulse during the night was so frequent and feeble that it could scarcely be felt at times. The heart's action was so much depressed that it was necessary to administer brandy every two hours, for six hours, when the pulse fell to 140, and became fuller. The bowels acted. The tongue had lost its brown fur, and presented a clean dry glazy aspect along the middle, with a white moist fur at the sides. The pain and tenderness of the hypogastrium remained much the same, but was rather less extensive over the abdomen. Mind quite clear, no headache. Countenance less anxious. The right forearm had become swollen and painful; the calf of the right leg also was tender to the touch. In the afternoon, redness, swelling, and tenderness over the patella of the left leg appeared. Pulse 160, feeble. Towards evening the pain and tenderness of the abdomen had nearly subsided. Diarrhœa occurred, and a painful sense of constriction about the epigastrium. Vomiting also occurred once.

Seen again by Dr. Lee. To continue brandy (3ij.) every four hours. Bark and ammonia. Fomentations constantly to the painful parts of the limbs.

28th.—It was requisite to administer starch and opium enemata frequently during the night. The pain in the leg and arm prevented her from sleeping much; started a good deal when dozing. The pulse varied during this day as to number and strength, sometimes being too rapid and feeble to be felt, at others falling to 140, with some appearance of power. The skin bathed in profuse perspiration. Diarrhœa persistent. The tongue somewhat moister, and quite clean. Quantity of stimulant decreased a little.

29th.—Had passed a quiet night. Bowels had not acted. Pulse 120, with a

clean and moist; skin cool and moist; is rather drowsy, but easily roused. Passed several stools involuntarily in the course of the day.

30th.—Passed a quiet night; some muttering and subsultus during sleep. Pulse 120, feeble; tongue clean; aphthæ appearing in the mouth and fauces; diarrhoea recurred; skin perspiring. Complained of severe pain in the left shoulder on this day. Increase stimulants, bark, &c.

April 1st.—Much the same in every respect. The abscess in the right arm was opened by a small puncture; it freely discharged good pus.

On several following days all the symptoms had improved considerably. The pulse had fallen occasionally to 104, and was fuller; the tongue had become moist and clean, the bowels acting more naturally, the appetite beginning to return, sleep more refreshing, some strength returning, so that she was able to sit up for an hour or two daily. The shoulder continued painful on touch or movement, but exhibited no external signs of inflammation.

Against these apparent signs of recovery, however, we had to set off her æmemic aspect, the fluctuations of the pulse from 104 to 120, and, after the 7th of April, an evening accession of hectic. On the 25th the right calf became more swollen and painful. This continued to increase, and on the 28th a deep narrow puncture let out about six ounces of pus, affording great relief. The shoulder also had become swollen. On the morning of the 29th the puncture made in the calf had closed; the cavity again filled, causing much pain; a director reopened the abscess, and liberated a similar quantity of pus, at first slightly streaked with blood. In the afternoon the sac of the abscess had again filled: it was very tense, hard, and painful: the introduction of a director was followed only by a flow of serum and blood in the first place; afterwards a small quantity of pus oozed forth. The orifice was enlarged, with the same result; severe rigors followed, and a feeble countess pulse. The suspicion that the sac of the abscess had become filled with coagulated blood was confirmed by the subsequent discharge of blood and conglua into the dressings. This sanguineous discharge never ceased from this period to the time of her death. Dr. Lee, who had throughout seen the pa-

tient occasionally, advised the application of ice to the leg, and a continuation of the Tinct. Ferri Muri, which she had been taking from the first occurrence of hæmorrhage. Petechial spots now appeared on various parts of the body and extremities. On the 30th April an expectoration of blood from the fauces took place. On the following day the patient became drowsy, and continued so until coma ensued on the 4th May. She had gradually sunk from the 2d, on which day blood was also passed with involuntary stools. Death ended the case on the 6th May.

Post-mortem examination, 24 hours after death.—The general blanched aspect of the body was that of death from loss of blood. Petechial spots in various parts.

Thorax.—The pleural sacs contained each about twelve ounces of clear serum. On the right side old pleuritic adhesions existed. Petechial spots were perceived under the pleura covering the lungs. The pericardium enclosed about four ounces of sanguineous serum. The surface of the heart presented numerous small petechial spots. The lungs presented extensive inter-vesicular emphysema; œdema existed posteriorly to a considerable extent. Corresponding to a puckering of the surface of the left lung was found a mass of soft, calcareous, and tubercular matter, about the size of a horse-bean. In the right lung a small pea-shaped substance, hard as stone, and of a black colour, was found enclosed in a cyst.

The heart was of a normal size, but its muscular structure was soft and flabby: its inner lining was studded with extravasations of blood: its cavities contained but very few coagula, and those soft and loose.

In the abdominal cavity the only morbid appearances were numerous ecchymoses under the serous membrane of the intestines and parietes.

The uterus had recovered its usual size in the unimpregnated state. *It exhibited no trace of disease of any kind. The ovaries were equally healthy.* These organs, as well as every other tissue and structure of the body, were pale and bloodless.

The large veins of the pelvis and thigh were examined, but showed no disease.

The cavity of the abscess in the calf of the right leg extended from below the knee-joint nearly to the ankle, and

was filled with blood, coagula, and pus: its lining membrane was of a deep red colour. No abrasion of the surface or open vessel could be discovered.

The bursa patellæ of the left knee was filled with pus. The shoulder-joint contained from four to six ounces of blood-discoloured pus: the cavity was lined with lymph, except on the external surface of the head of the humerus, where the bone was denuded.

REMARKS.—The case which is here related possesses many interesting features. It is probable, however, that other circumstances of a non-medical character may have invested this case with undue importance in the eyes of the writer.* The following reflections may nevertheless be offered on several points in the history.

In the first place, looking at the results of the post-mortem examination, it might be doubted whether this was really a case of inflammation of the uterine veins. No trace whatever of inflammation, or its termination, it has been seen, were discoverable in the uterus or its appendages. This was probably explicable on the supposition either that coagula or fibrin may have existed in some veins that were not examined; or that the poison, whether pus or morbid secretion, may, in the course of the disease, have become completely eliminated from the uterine or pelvic veins; or, that they were arrested only in those parts which, in the course of the case, showed signs of local disease—*e. g.*, the leg, arm, and shoulder; while, from the irritation thereby induced, the system became so deranged that the morbid condition of the blood which ultimately led to the immediate cause of death was produced.

It is true that the essential characteristic of pus in the blood—*viz.*, coagulation—was wanting, yet the symptoms throughout the progress of the case

were precisely those met with in cases where the post-mortem evidence of inflammations of the veins are obvious and sufficient. Hence it is probable that the pus or morbid secretions had been transmitted throughout the entire circulation. That such a result is possible is confirmed by the experiments of Mr. Henry Lee, related in his Jacksonian Prize Essay on Inflammation of the Veins, recently published, besides the facts of the case now before us, and the history of many analogous cases of the impregnation of the blood with diseased matter—*e. g.*, glanders, &c. &c.

Looking to the symptoms, it may be observed that in the progress of this case the first effects of a circulation of blood in a diseased state was felt in the cerebral functions. It is not possible in the present instance to fix precisely upon a period at which the disease may have been said to have commenced. No symptoms were distinctly perceived until the sixth day after delivery; and these, although we could not but fear their real nature, were not shown to proceed from uterine inflammation until three days later, when the obscure head symptoms greatly abated, concurrently with the manifestation of active uterine disease. These suddenly lighted up with energy on the 25th and 26th, and then, with the usual character of purulent infection of the blood, depression of the whole system speedily ensued. The entire course of the disease exhibited all the features of uterine phlebitis as described by Dr. Robert Lee and other authorities.

The long continuance of the case (from about the 23rd of March to the 22th of April), and the progressive abatement of many of the worst symptoms under incessant watching, and supporting with stimulants and the strongest diet, encouraged expectations of a favourable termination. These, however, were destined to be disappointed: the blood was too much vitiated to admit of healthy reparative processes; the functions of assimilation and sanguification were necessarily impaired; and the consequences were seen in the still further exhaustion produced by purpura hæmorrhagica, against which her enfeebled frame struggled in vain.

The opinions of various authors on the nature of such inflammation as we had here to deal with might be quoted; but, as has justly been remarked by

* The words wherein to express some of these circumstances may be borrowed from Dr. Churchill's "Historical Sketch of the Epidemics of Puerperal Fever:"—"The course of a favourable parturition suddenly interrupted, and, without any appreciable cause, exchanged for symptoms which excite the utmost alarm in the physician—the anxiety and anguish of those so lately rejoicing—the blighting of the sweetest hopes in life; and, finally, the rupture of its dearest ties, and the melancholy desolation of a home so lately the abode of happiness." Besides these considerations, the patient was a lady of great literary acquirements and brilliant talents.

Professor Paget, in his valuable lectures on Inflammation, when speaking of morbid conditions of the blood as one of the circumstances of inflammation,—“I fear that the nature of this disturbance cannot yet be chemically expressed, and that the facts which chemistry has discerned in the condition of the blood in inflammations cannot yet be safely applied in explanation of the local process.” The case now before us supplied forcible illustration of the correctness of this statement. It would, for instance, be impossible to account for the cessation of inflammatory action in the veins of the uterus, the transmission of the poison by the general circulation, and the determination of local inflammation to special parts, as the joints, and among the muscles, rather than in the lungs or any other viscus.

One or two points of treatment presented subjects of deliberation during this case, and, practically, it may be useful to allude to them.

1st. With regard to the time and extent of depletion: it is doubtful, if venesection had been practised at the outset, or when the rigors occurred, whether the subsequent symptoms might not have been arrested. The answer must be in the negative, depression being one of the worst results of the circulation of contaminated blood.

2d. Whether any other plan than that of simply supporting the system, with a hope that in time the poison in the blood might be eliminated? There are no medicines which, under such prostration, can be supposed to offer an especial prospect of such elimination within the period that fatal injury must have been inflicted, or before the natural powers of restoration could have been re-established.

3rdly. With regard to the local treatment of the inflammations and suppurations in the joints or elsewhere: how soon should abscesses be opened in such cases? In answer to this inquiry, Sir Benjamin Brodie's advice* applies to this as to other collections of pus, under certain restrictions:—

“It is not generally advisable to open an abscess while there is any considerable mass of substance between it and the skin, on account of the hæmorrhage which may take place into its

cavity, and, by putrefaction, produces typhus and fatal results.”

In the local collections of pus following on uterine phlebitis, and not enclosed in the cavities of joints, it is desirable to let out the matter, as early as distinct fluctuation is perceived, by a small deep puncture. If the pus be allowed to find its own way to the surface, further detriment accrues to the constitution from the irritation which is caused by its further burrowing among adjacent structures. When, however, there is evidence that the sac of a joint contains pus, as was the case in the shoulder and the bursa of the patella in the example before us, it is, as the same high authority remarks, not advisable to open it until the skin over it has become very thin. Neither the shoulder joint nor the bursa were opened, for the reason that in the former fluctuation was distinct only within a short time of death; and that in the latter the swelling was slight, and the pain inconsiderable; under which circumstances it was not deemed justifiable to incur the risk of additionally inflaming its sac, as was probable if it were laid open.

Lastly, this case serves to throw some light upon one very important and disputed point in the history of puerperal fevers—i. e. the question of infection. The history which is contained in Dr. Collins's invaluable practical treatise; the cases related by Dr. Robert Lee in his lectures, and in his work on Puerperal Diseases; the history of the Epidemics of Puerperal Fever, by Dr. Churchill;* the account given by Dr. Routh of the occurrences connected with puerperal fever in the Vienna Hospitals; the lectures of Dr. Murphy;† the facts which must be familiar to every one engaged in obstetric practice;—all prove the possibility of the disease being communicable.

“I fear we must conclude,” observes Dr. Churchill, after quoting the evidence of Drs. Lee, Robertson, Copland, Ramsbotham, Blundell, Davies, &c. &c., “however reluctantly, in favour, not merely of the contagiousness of puerperal fever, but of the possibility of its contagion being carried by an intermediate party.”

* *Essays on Puerperal Fever and other Diseases of Women*, Published by the Sydenham Society.

† See the last volume of the *Med. Gaz.*

The present instance may either be regarded as one of the epidemic prevalence of the disease, or of the coincidence of several sporadic cases. At the time of its occurrence no other case had been known in my neighbourhood for several years: none have occurred since, although I attended several other ladies during its progress. I did so under the conviction that it was a sporadic case, since, although Dr. Lee had been called in consultation to several cases around London at the same time, yet no case of the kind had occurred to him in his own private practice. In the next case that I attended severe rigors occurred on the third day; but no further ill effects ensued.

It may therefore be regarded as a proof that the disease may be sporadic as well as epidemic or contagious, and it also furnishes an argument why a medical man should not discontinue obstetric practice on the happening of a single case of puerperal fever, since it obviously incurs no obligation to abandon his engagements. Other cases occurring, however, in the practice of the same individual, the predicament is entirely changed, and a man's ethical principles must take the precedence of his professional requirements.

Upper Holloway, Nov. 1, 1850.

EXPERIMENTAL RESEARCHES ON THE THEORY OF A HEPATICO-RENAL CIRCULATION, AND ON ANIMAL HEAT. BY M. BERNARD.

M. BERNARD having administered prussiate of potash to a dog, and ascertained the existence of this salt in the urine within ten minutes, killed the animal, and examined the blood separately from the renal artery and vein. The latter contained a large proportion of prussiate of potash, the artery none at all. This was the reverse of what M. Bernard had expected. It happened that the salt had been administered on a full stomach. Surprised at the result, M. Bernard repeated the experiment many times, and found that, when given during the process of digestion, the prussiate was always found in the vein; if given on an empty stomach, it was uniformly discovered in the artery.

In tracing the course of the prussiate from the stomach to the kidneys, it was constantly found in the blood that left the stomach and intestines—i. e. in the portal system; while it was never met with in the general venous circulation. It is obvious,

therefore, that during digestion a circulation obtains different to that which exists while fasting.

This circumstance, M. Bernard states, explains the discrepancies met with in the results of investigations on the presence of various salts in the urine. It gives a clue, also, to many other obscure phenomena. It is known that the absorption of poisons is more active during fasting, and under loss of blood; and that their absorption is retarded by the injection of water into the veins: the fact has been explained on the greater or less degree of tension of the blood-vessels. It has been noticed, also, that certain poisons are harmless to some animals, and noxious to others. For instance, the rabbit is not poisoned by atropine, while the dog is so: this is accounted for, according to M. Bernard, by the circumstance that the rabbit, although it may not recently have partaken of food, has constantly alimentary matter in its intestines. The atropine absorbed by the portal vein traverses the liver, and, passing by the vena cava to the kidneys, is excreted in the urine; not having passed by the general circulation, except in very minute quantities.

M. Bernard has performed a series of experiments on dogs and rabbits, the results of which support his theory of a special circulation that relieves over-fullness of the vascular system. These researches, the author observes, show also that the endemic method of applying medicines will often be more certain than their administration by the digestive organs.

Is the hepatic circulation one of the sources of animal heat?

M. Bernard observes that the connection between the renal and hepatic circulations with that of the intermediate vena cava, is the physiological condition in the fœtus; M. Bernard states that in after-life the blood passing from the liver during active digestion is at least one degree higher than that of the blood as it enters the same organ; and that the injection of solids and fluids reduces the temperature of the blood in the portal vein. The temperature of animals is reduced by three or four days abstinence. The insufficiency of the observations made on the increase of temperature in the lungs, together with a consideration of the great extent to which secretion takes place in the liver, and the activity of its circulation during digestion, are, in Dr. Bernard's opinion, sufficient reasons for attributing to the liver a principal share in the production of animal heat.

MEDICAL GAZETTE.

FRIDAY, NOVEMBER 29, 1850.

In our last number we made some general remarks on the evidence adduced before the Parliamentary Committee on Prison Discipline, in reference to the supposed injurious effects produced on body and mind by the adoption of what is termed the **SEPARATE SYSTEM**. We have now to consider those facts contained in the Report which tend to show the error of the commonly received opinion, and to prove, that whether as regards health or the reformation of the individual, it is not, when fairly carried out, attended with that danger with which it has been hitherto associated.

We shall now proceed to lay before our readers the evidence which contains the facts to which we allude. And first, that of one of the most competent witnesses, Dr. G. Owen Rees, who has held the office of Superintending Physician of the Pentonville Prison, since its opening. Dr. Rees has watched the "experiment" from the first, and has arrived at the conclusion that the system is innocuous. We quote from his evidence:—

Q "Will you be good enough to state to the Committee how long a period you think a prisoner in Pentonville may be kept with safety to his mind and body, under the separate system of confinement?"

A "I think *eighteen months* is a very fair period."

Q "Do the prisoners in general improve towards the end of the year for which they are subjected to the separate system?"

A "It has appeared so to me."

Q "They are better in mind and no worse in body?"

A "I think so."

Q "You used the expression in the first part of your evidence, that you thought eighteen months was a very

fair period for the separate system; what did you mean by that expression?"

A "I think it is a period that might be adopted in general, without much injury to the prisoners."

Q "Former witnesses have almost unanimously told us that twelve months is, in their opinion, a better period for which the separate system, as a maximum period, should be applied?"

A "I speak from my experience at Pentonville, where the system of separate confinement has been applied in a modified manner, different from the way in which it has been applied in other prisons; so that I should say the system in other prisons has not been so good as the system pursued at Pentonville?"

Q "And at Pentonville you are not afraid of eighteen months separate confinement?"

A "No; as regards the effects upon the minds of prisoners, of separate confinement, which is a very important point, I should observe, *that the cases of insanity or delusion, which have occurred have generally occurred at early periods of separate confinement, and not at advanced periods.*"

Q "Do you think that the power of memory is equally great, after a period of separate confinement, as it was before?"

A "I have visited a number of men with a view to see whether the memory fails, that point having been very frequently insisted upon; and from attentive observation I can state, that in my opinion, *eighteen months separate confinement does not impair the memory.*"

Q "Have you heard of any prisoners who have been confined for a long period being subject to fits upon their discharge?"

A "Yes; I have seen men in fits. *Men have suffered from fits since the period was shortened.*"

Dr. Rees also states in the eighth Report of the Commissioners of Pentonville Prison, that in addition to four attempts at suicide during the year 1849, "there is an irritability observable, which I have never before noticed among men confined in Pentonville"

How far this irritability may be connected with the alterations lately made

in the system, and how far it is related to the increase of insanity under the short periods of imprisonment, admits of doubt. As a question of fact it still remains to be determined.

We may here notice those anomalous convulsive attacks, referred to by Dr. Rees, which have been observed in the prisoners on their embarkation for the penal settlements.

"Mr. Hampton states, that the sudden change from great seclusion to the bustle of a crowded convict-ship, produced a number of cases of convulsions, attended in some cases with nausea and vomiting; in others simulating hysteria; and in all being of a most anomalous character."

Mr. Hampton subsequently informed the Commissioners of the prison that these hysterical convulsions were propagated among the men by imitation. This we know to be common in the cases of females. The fits entirely ceased on the third day after leaving England.

These convulsive attacks are difficult of explanation. The cause to which they were assigned by Mr. Hampton is clearly insufficient, inasmuch as the measures adopted by the Commissioners for their prevention, on the assumption that they resulted from the causes pointed out by him, have not proved effectual for their prevention. It was given in evidence, that previously to recent embarkations the prisoners had been associated in a most unrestrained manner, to the number of 100 and 200, chopping wood, and making every conceivable noise that could be made, to inure them to what they would experience on board ship, and yet that not a single fit occurred while they were thus associated. They occurred, however, on embarkation, but not when the prisoners were put on board the hulks in the Thames. These fits have been noticed also in greater proportion among prisoners who have undergone the short

period of confinement than among those of the longer periods. A very singular fact is stated in reference to the convulsive attacks. The prisoners who embarked in one vessel experienced no fits, and the majority of these had been in confinement for periods ranging from eighteen to twenty-three months, and, being under the most rigid separate discipline, they *had not been associated previously to embarkation.*

Putting all these circumstances together, the only explanation that occurs to us is that the fits must be regarded as the consequence of a peculiar condition of the nervous system, resulting from the combined effect of the sudden change from separate imprisonment to complete and free association amidst the bustle of a convict ship, together with the effect on the mind produced by the certainty of banishment from all the ties of home and country.

The conclusions of Dr. G. O. Rees with regard to the effects of separate imprisonment, are confirmed by the admissions of Mr. Bradley, surgeon of Pentonville Prison.

This witness was asked:—

Q. "What is your opinion of the effect of the separate system upon the bodily health of the prisoners?"

A. "I think that towards the end of the twelvemonth the bodily health begins to be affected; the prisoners become pale, and I think the seeds of consumption are in many cases developed."

In answer to other questions, Mr. Bradley states:—

"I do not know that the separate system itself produces consumption during the first twelvemonth."

"I believe that the cases of consumption between the twelfth and eighteenth month are double the number of those that occur during either of the preceding six months."

Q. "What is your opinion of the effect of the separate system upon the mind?"

A. "During the last seven years there have been many cases of mental

disease, the greater number of which have occurred during the first six months."

"There are two periods at which the pressure seems to be greatest: the first period is from six weeks to two months after admission, and the latter period is towards the end of the first twelve-month."

In reference to this statement, we may observe that no facts are adduced in its support.

Q. "Do you think it possible that, even where actual insanity did not arise, the mental powers might be impaired and the memory affected without your detecting it in the prisoner?"

A. "Certainly." "I think some cases have occurred of the kind you speak of, where the intellect has been rendered obtuse, and the man has become stupid at the end of the first six months. When I speak of the cases of insanity, they are a small minority." (!)

Q. "What is the maximum time you would recommend for the infliction of the separate system, looking to the health of both body and mind?"

A. "Twelve months."

Q. "Not more than that?"

A. "No."

Q. "Do you find cases where the separate confinement is beneficial both to the body and mind of the prisoner?"

A. "Undoubtedly."

Q. "Inducing habits of repose and general tranquillity of mind?"

A. "Yes."

Q. "Are those cases the majority?"

A. "They are."

Q. "There is no other bodily disease (besides insanity and consumption) to which the separate confinement predisposes the prisoners?"

A. "Heart disease has been said to have occurred among a great many of the men in separate confinement. I have examined a large number of men, with a view to discover whether heart disease existed or not, and I cannot say that I found any cases of diseases of the heart that could be fairly attributed to the system; but of cases of functional derangement of the heart and palpitation, accompanied with pallor of the face, there have been a great many."

Q. "On the whole, judging from

your experience, supposing the separate system to be strictly limited in time, and carefully guarded by watching the health of the individual inmates of the gaol, I understand you to say that you think it rather tends to improve than to injure their health?"

A. "I think so."

Q. "But if not carefully guarded, or if carried too far, it is full of danger?"

A. "Yes."

Q. "Do you consider the mere fact of a prisoner being kept by himself is likely to induce consumption, where no tendency to it existed in the constitution before?"

A. "I am inclined to think that there is an amount of mental depression produced by separation which does tend to develop tubercular disease; but on the other hand, there are many, I believe, who come in with consumptive symptoms, and who are cured by a residence in the prison. The altered circumstances, the good diet, and the uniform temperature of the cells, in many cases have effected a cure."

The next evidence which we shall quote, though not strictly professional, is perhaps the more striking on that account: it is also the most decided and the most explicit with reference to the modifying influence of time in ameliorating the supposed evil consequences of the separate system upon *mental health*. We desire particularly to draw our readers' attention to the *facts* and statistics stated by the Rev. J. F. Burt, Assistant-Chaplain to the Pentonville Prison.

Q. "Are you not apprehensive of any danger to the prisoners if that system were strictly carried out for a long period of time?"

A. "None whatever; not greater than in other prisons. I may perhaps answer that by fact, rather than by opinion: the number of mental cases has never been so numerous or so disastrous in any equal period as since the relaxation of the discipline."

"From the opening of Pentonville to the present date the total number of mental cases have been,—insanity, seventeen; and of these fifteen have occurred before the twelfth month, and two after: delusions, nineteen; of these

fifteen have occurred before the twelfth month, and four after. Putting both classes of cases together, the result is a total of thirty-six cases; before the twelfth month, thirty; beyond the twelfth month, six."

Mr. Burt proceeds, in answer to other questions:—

"The Pentonville Prison has been opened seven and a half years: I will distribute that time into two periods—one in which the prison has contained the greatest number of prisoners for terms not exceeding twelve months, the other those years in which it contained the greatest number for terms exceeding twelve months, to an average of seventeen or eighteen months, and in many cases running on to twenty-two and twenty-three months. The one period embraces four years, the other three and a half years. The mental statistics for those two periods are as follows:—During three and a half years of short imprisonments, daily average population about 440; cases of insanity, 14; delusion, 11; suicides, 3; total, 28. During four years of long imprisonments, daily average population 445; cases of insanity, 3; delusions, 8; suicides, none; total, 11. Thus the ratio of insanity to the daily population has been, during the period of short terms, nearly five-fold of what it was during the period of long terms, although one period is six months shorter than the other.

What is even more startling is the fact, that during the period of short terms, the ratio of insanity is much greater than in the period of long terms, to the aggregate number of prisoners coming within the walls of the prison, wholly irrespective of the difference of the time during which the same number of prisoners will have undergone the severe punishment of the cell. The average term of detention being in one period 18 months, and in the other 12; the aggregate number passing through the prison during equal periods

has been greater during the short term than during the long by about 50 per cent. The numbers are, for the period of short terms, about 2,444; for the period of long terms, 1,627; difference, 817. Against that increase of 817 upon an aggregate of 1,627 prisoners, we have an increase of mental affections from 11 to 32. Thus the ratio of increase in the aggregate number of prisoners is as from 1 to $1\frac{1}{2}$; the ratio of all mental affections, as from 1 to $2\frac{1}{2}$; of insanity alone, as from 1 to about 5."

We must reserve our further examination of the Report until next week.

Reviews.

An Inquiry into the Nature of the Simple Bodies of Chemistry. By DAVID LOW, F.R.S.E. 2d edition, enlarged, 8vo. pp. 344. London: Longmans.

SOME time has elapsed since we received this book; but, owing to accidental circumstances, we have not had an earlier opportunity of laying a notice of it before our readers. It is strictly an essay on the higher branches of philosophical chemistry, from the pen of a man well acquainted with the science, and aware of the difficulties with which its fundamental theories are beset.

Mr. Low professes to furnish reasons for believing that the bodies now called *simple*, or *elements*, by chemists, are really compounded, and that the elementary forms of matter may be reduced to a very small number. He argues that we must not assume substances to be elementary or simple "because we have been unable to overcome the affinities of their constituent parts, or to convert them into any other form of matter." Berzelius has objected to this view that it will be time enough to examine the question when any one of the so-called elements has been proved to be compound by analysis. Mr. Low dissents from this proposition, and quotes the example of Lavoisier who predicted that the alkalies would turn out to be oxides before the discovery of potassium, sodium, and other alkaligenous metals. If Davy had sat down with the conviction that potash

and soda were elementary substances, he would not have made those brilliant discoveries which have shed such lustre upon his name. Even in our own time, Berzelius has himself stoutly contended for, and supported by ingenious arguments, the view that the now recognised element, chlorine, was a compound of oxygen and muriatic acid. Hence the same latitude must be allowed to Mr. Low to question the stability of the present views of chemists, and to prove, if he can, by fact or reasoning, that they are erroneous.

There are numerous singular analogies among elementary bodies in respect to light, heat, and their chemical relations to each other, which lead to the inference that they may be modifications of one and the same form of elementary matter. Chlorine, bromine, and iodine, present us with an example of this relationship, or, as Mr. Low would term it, of an isodynamic character. The section on chlorine affords a good instance of Mr. Low's mode of reasoning out the compound constitution of elementary substances.

It is well known that Cyanogen C^2N is a compound; and yet, in most of its chemical relations, it acts like a simple or elementary gas. It is, according to Mr. Low, isodynamic with chlorine. Thus the combinations of the two with hydrogen, mercury, silver, and potassium, are perfectly analogous: there is a want of analogy, however, in their combinations with oxygen, and an entire failure of it in the remarkable isomerism of fulminic and cyanic acids of which chlorine presents no example.

From the admitted analogies Mr. Low argues that chlorine and cyanogen cannot be dissimilar in corpuseular constitution; that if one be compound, the other cannot be simple. He proceeds to contend that carbon and nitrogen most probably enter into the composition of chlorine. Again, chlorine presents certain resemblances to sulphur "in the suffocating odour distinctive of both substances, in their unfitness for respiration, combined with the power in certain cases of supporting combustion, in the blanching properties of their compounds, and in their many dynamic relations." (110.) Considering that these resemblances of properties establish a resemblance of corpuseular constitution, it is conceived by the author to be probable that sulphur is also a constituent

of chlorine; and if one atom of carbon be removed from cyanogen, and one atom of sulphur substituted, we shall then have, instead of $Cl = 36$, the following formula: $C. S. N. = 36$. "Or if we choose to regard cyanogen as a carburet of CN, chlorine must be a sulphuret of the same base."

This reasoning is very ingenious, but wholly speculative: it falls far short of that accuracy which we look for in chemistry, and without which the science would present a mass of confusion. The so-called resemblances of sulphur and chlorine would prove just as readily that chlorine entered into the composition of sulphur as that sulphur was a constituent of chlorine. The hydracid compounds have no resemblance whatever in their chemical properties or effects on the body. As to the blanching properties, they are wholly different in kind. The bleaching by sulphurous acid is a real and soluble combination of the acid with the colouring matter; the colour is not destroyed, but it may be readily restored by a more powerful acid or base. In bleaching by the aid of chlorine the colour is destroyed, and cannot be restored.

We might quote other instances of inconclusive reasoning from this book, but we consider this unnecessary. With much that is erroneous in speculation, it contains a large amount of profitable reading. It is, however, a work which can be safely read and fairly appreciated only by the advanced chemist.

The Principles of Surgery. By JAMES MILLER, F.R.C.S.E., Surgeon in Ordinary to the Queen for Scotland, Surgeon to His Royal Highness Prince Albert for Scotland, &c. &c. Second Edition. 8vo. pp. 803. Edinburgh: Black. London: Longman. 1850.

MR. MILLER's writings are well known to be among the best contributions to surgical science. The first edition of the work before us has been so well received by the profession, and is so well known to surgeons, that we have now only to bring the second edition under the notice of our readers. We observe that the form of the volume is changed to the octavo size, and that woodcut illustrations have been copiously introduced, by which its usefulness is enhanced. At the end

of each subject, references have been made to its literature, which will also be found serviceable to the student. A section is added on the surgical use of chloroform, in which this subject is treated in all points of view.

We strongly recommend this volume, as presenting in a condensed form the best-founded principles of surgery, as laid down by its most eminent professors, and known experimentally to the author as safe guides in practice.

Lunatic Asylum Reports. Edinburgh: 1849. Belfast: 1850. Devon: 1849-49-50.

THE numbers of patients admitted into these several asylums during the past years were:—

	Males.	Females.	Total.
Edinburgh	109	166	265
Belfast	55	57	112
Devon, 1848			139
" 1849			116
" 1850			111

The report of the Devon asylum does not distinguish the sexes of the patients admitted.

The discharged were as follows:—

	Males.	Females.	Total.
Edinburgh, cured	42	77	119
" uncured	20	35	65
" died	42	37	79
Belfast, cured	24	28	52
" uncured	11	10	21
" died	25	18	43
Devon, 1848, cured	17	16	33
" " uncured	0	3	3
" " died	9	10	19
" 1849, cured	15	27	42
" " uncured	3	1	4
" " died	16	19	35
" 1850, cured	20	32	52
" " uncured	0	5	5
" " died	17	13	30

The forms of disease have not varied much from what has been exhibited in former reports of these and other asylums, which we have fully noticed on recent occasions.

Dr. Skae, in stating that more cases than usual of *Puerperal Mania* had been admitted during the year, takes the opportunity of noticing that their occurrence cannot be attributed to the use of chloroform, which had not been administered in any of these cases.

The most frequent cause of the disease which we observe in these Reports was *intemperance*.

In all the Reports we observe the same judicious plan of moral control which leads to the patient being treated as if he were sane,—the same care and attention to the bodily health, the same principles of moral and religious training, seeking to draw the sufferer out of himself, and to engage his mind with healthy and agreeable occupations.

It is much to be desired that the Commissioners of Lunacy would point out the features of greatest importance in the history of insanity on which it is desirable to obtain information, and that they would enjoin their publication in the Annual Reports of Asylums; the present desultory, and, too often, unconnected manner of stating the results of the year, leads to no public or professional benefit, and suffers many valuable facts to be entirely lost to science.

We would also suggest that the common octavo form should be adopted for the publication of Reports, so that a yearly volume might thus be formed, by binding together these scattered contributions. A hint from the Commissioners, and a plan suggested by them, would be productive of much improvement.

The Annual Report of the Verral Spinal Hospital for the Treatment of Diseases and Distortions of the Spine, Contractions of the Limbs, Hip-joint Disease, and Paralysis. 1850.

FROM the statements of the medical officers, Mr. Hugman, Mr. Charles Verral (son of the founder), and Mr. Day, this charity seems to be the medium of much good. We trust that the present appeal will not pass unheeded: the funds of the institution need support. When we state that twenty-four in patients, besides numerous out-patients, receive the benefits of this charity annually with the outlay of only four hundred and nine pounds, it will be evident that the utmost economy is exercised. One hundred and thirty pounds cover the salaries of house-surgeon, matron, nurses, and servants! The house-surgeon must be virtually an honorary appointment under such an expenditure. A series of cases are added in illustration of the advantages of the prone position in the treatment of spinal disease.

Proceedings of Societies.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Nov. 12.

DR. ADDISON, PRESIDENT.

Ovariectomy.

AFTER the reading of the papers published in our last number on the subject of *Ovariectomy*, some remarks were made by various members, of which we subjoin an abstract.

MR. CÆSAR HAWKINS dwelt upon the necessity of obtaining a full and complete history of all the cases of operation, whether only commenced or carried through, with a candid statement of the results. He stated that the operation had now been performed by ten surgeons attached to London hospitals, and not one of these gentlemen had performed the operation a second time. Recovery took place in one case only. He appealed to Dr. F. Bird and Mr. Walne, who had frequently performed this operation, to publish a full account of the results of their practice, for the information of their profession.

Dr. F. BIRD observed that he had communicated his cases to Dr. Lee, but he had declined giving the details, because he objected to the manner in which Dr. Lee's tables were arranged.

Dr. LEE said that the information furnished by Dr. F. Bird was so meagre that he could not, in justice to the completeness of the other cases in his table, add those of Dr. F. Bird to the document. He subsequently remarked that out of *thirty-seven* cases in which an unsuccessful attempt had been made to remove the tumor, *fourteen* of the patients had died. The exploration of these tumors was by no means a harmless proceeding.*

MR. B. PHILLIPS agreed with Mr. Hawkins in condemning the practice of publishing successful and withholding unsuccessful cases. He considered it to be a suppression of the truth which every honest man would condemn. As ovariectomy is an

operation not often performed in public hospitals, but in private houses, it is doubly incumbent on those who have the means to remove the veil which may conceal the results of these cases.

Dr. TILT observed that he had followed out the individual history of each case, and had classified them accordingly. This was the only way in which the cases could be made valuable in determining the risk of the operation. For instance, some of the cases of ovarian disease were of a very chronic character, whilst in others the disease was so acute as to resemble peritonitis in a mild form. Now it would be obvious that to class these two forms of cases together might lead us to a most erroneous conclusion. In the chronic form of the disease, attacking patients late in life, it was, he believed, unjustifiable to operate; but in some cases, where the disease was more acute, and it occurred between the ages of eighteen and thirty, it might be justifiable. Dr. Atlee, of Philadelphia, had operated in thirty-six of such cases: of these twelve died, and twenty-four recovered. Looking at the mortality in those cases where no operation was performed, and to the fact that life was very rarely prolonged beyond two or three years, it would at once be seen how important it was to arrive at right conclusions respecting the operation. The question, then, immediately suggested itself, as to which were the cases most favourable for operation. Upon this point we must remain in doubt, so long as fatal cases, and their histories, were withheld from publication.

MR. LAWRENCE said,—Mr. President, I did not attend the meeting of the Society to give information, for I have no experience of ovariectomy; I have not performed it, and unless my view of the matter should be essentially altered, I never shall; nor have I seen it performed. I came, sir, to listen and to learn, and my expectations of information have not been disappointed. I have heard things strange and new, at the same time instructive. From the interesting document presented to the society by Dr. Lee,—for which he deserves the best thanks of the profession, the public, and more especially of females labouring under abdominal enlargements,—it appears that in 37 out of 108 authenticated cases of operation collected by him, it was found, when the abdomen had been exposed, either that there was no diseased ovary, or one so circumstanced as not to admit of removal. We cannot help believing that this announcement, however unexpected and startling, represents ovariectomy in too favourable a light. Dr. Lee's list is imperfect, confessedly so; it contains only the cases of which he has

* In a letter since published on this subject Dr. F. Bird observes—"I have extirpated large ovarian tumors in *thirteen* cases, of which number *nine* were successful, four unsuccessful; in one case the tumor could not be extirpated, and the patient sank. In eighteen other cases I have made small incisions, as a means of diagnosis or relief, and in no case did a bad result ensue from such incisions; leaving a total of thirty-two cases in the treatment or investigation of which the abdominal cavity has been incised."

received authentic accounts. There is no reason for supposing that any successful operations have been omitted. We have generally, perhaps invariably, heard of them, as soon as the cicatrization of the wound had been completed. The same alacrity has not been observed in the communication of unsuccessful cases; and I am not aware that any one of the thirty-seven cases just alluded to has been published by the operator. We can hardly doubt, therefore, that if a correct list were drawn out of all the operations, the proportion of cases in which ovariectomy had been fatal would be increased, and that a considerable addition would be made to the thirty-seven instances, in which the perilous proceeding of exposing the cavity of the abdomen had been resorted to unnecessarily. Dr. Lee's list, however, as it now stands, is quite sufficient to make us doubt the propriety of admitting ovariectomy into the catalogue of recognised and approved surgical operations. We are still in the dark on one point, which ought to be ascertained before we can determine the true value of the proceeding; that is, its influence in prolonging life.

Our excellent colleague may probably have acquired information on this point, in prosecuting the researches of which he has now laid the result before the Society, and he could probably render them still more complete, by learning the subsequent history of the cases, in which the operation had been successful, by ascertaining the kind of health enjoyed, and the duration of life after recovery. The chances offered by the operation would be expressed by the average duration of life in those who have undergone it, if that can be ascertained, reduced in amount by a deduction, and that not inconsiderable, on account of those in whom the operation ends fatally, whether completed by removal of the ovary or not. The result thus obtained must be compared with the probable duration of life in those patients who receive such relief as can be afforded by the other resources of art. Many years ago I saw a female from the north of England who had undergone ovariectomy, as it was reported, successfully, not very long before the time of my seeing her, the operator being Mr. Lizars. It was performed when what was then called the major incision was in vogue—that is, a cut through the parietes from the ensiform cartilage to the pubes, such as we make to expose the abdomen for examination after death. There was a sound cicatrix on the person of this female, indicating that an incision of that description had been made. The pelvic region was occupied by a large solid mass of disease. The comparison I have now suggested must be made between

two sets of cases similar in their nature. Ovariectomy is not performed indiscriminately, but in selected instances. The cases chosen for operation are exactly those in which it is least necessary, the danger to life being remote, and the patients sometimes living for years with only an occasional interruption of health and comfort.

Mr. Lawrence, in common with the other speakers, commented on the statement of Dr. F. Bird, that he had adopted the exploratory incision in *forty or fifty* cases. It turns out, however, that Dr. Bird has really only adopted this plan in *eighteen cases*: hence it is not necessary to insert remarks which refer to a number now admitted to be erroneous. It is very evident from this discussion on the excellent paper brought forward by Dr. R. LEE, that the feeling of some of the ablest men in the profession is decidedly adverse to this operation. This feeling will find a strong hold in the minds of all members of the profession, until a full and complete history of every case, without any reservation of details, has been published. We have no doubt much good will accrue from this discussion. As the facts at present stand, all must admit, with Mr. Lawrence, that Dr. Lee's list of cases, as it now stands, "is quite sufficient to make us doubt the propriety of admitting ovariectomy into the catalogue of recognised and approved surgical operations."

MEDICAL SOCIETY OF LONDON.

Nov. 9, 1850.

DR. J. R. BENNETT, PRESIDENT.

On the Treatment of Ovarian Dropsy by the Production of an Artificial Oviduct.

MR. J. B. BROWN read a paper on this subject. The operation he now proposed he would designate but a modification of a previous one proposed by Mr. Bainbridge, of Liverpool, and to that gentleman he would accord the merit of having first seized upon the guiding principle—that of setting up adhesions between the sac and abdominal parietes, and leaving the sac to exhaust and destroy itself, by its discharge through an artificial opening. The operation consists in making an oblique lateral incision in the middle third of the oblique line, extending from the umbilicus to the anterior superior iliac spine, reaching at first to the peritonæum, then puncturing and emptying the cyst by a trocar and canula, and the peritonæum being reflected, to subsequently stitch the cyst to the aponeurotic tendon of the external oblique muscle; and this being done, to divide the

cyst between the line of the sutures; and, finally, in keeping up the communication between the interior of the cyst and the exterior, allowing the free and constant escape of all secretions produced within it. This operation of Mr. Brown differs from that practised by Mr. Bainbridge, in that the fistulous opening is formed in the side, and not in the middle line, whereby the discharge from the wound is facilitated, and the greatest comfort to the patient ensured. In illustration of the practicality and advantages of the operation, Mr. Brown proceeded to narrate the history of three cases in which he had carried it into practice.

In the first case, which was one of long standing, tapping had been performed several times. It afforded the patient so little relief on the last occasion, and her health was so rapidly giving way, that it was determined to perform the operation under discussion. The patient went on well for five days, but then became subject to fainting fits and much dyspnoea. Dr. Sibson considered that the patient suffered from disease of the heart. She died within a month of the operation. After death a considerable amount of visceral fluid was detected. The ovarian cyst was adherent all round the incision. It communicated at its posterior surface by an opening of some duration with a smaller cyst, and by this with several others. The contents of these cysts differed in their nature. In commenting on this case, Mr. Brown said, he regarded the fatal result to be mainly attributable to the amount of organic disease present, and that it did not militate against the value of the operation.

The second case was one in which tapping afforded so temporary a relief, that this operation was performed; her general health, however, and weakness, holding out but little hopes of success. The walls of the cyst and peritoneum were found adherent, and an apparently solid elastic tumor occupied the interior of the cyst. All went on well for the first fortnight; she then caught cold, and suffered considerably. This was overcome by remedies; but the discharge diminished, and this was found to arise from adhesion between the walls of the cyst and the tumor. The effect was the division of the cyst into two compartments, from one of which no fluid was evacuated. The evil was removed by breaking down the adhesion, and placing in the orifice a pledget of lint. The operation was performed in April. She went on well without any remarkable symptoms until July, when the large cyst in a putrescent state escaped through the orifice. At the end of August vomiting of a persistent character set in; she was seized

with paralysis of the left side, and died comatose. An autopsy being made, a cyst capable of holding an orange was found in the right ovary, communicating externally by a healthy fistulous canal. The wall of this cyst was soft and disorganised posteriorly, and there was a laceration, apparently made during the examination, through which some of the pus it contained had escaped into the recto- and vesico-vaginal pouches of the peritoneum: the latter presented no appearance of inflammation. From three to four small cysts, varying in size from that of a currant to that of a grape, existed just within the line of the incision. The kidneys were soft, large, and pale; the liver remarkably yellow, and the brain soft and unusually pale. In this case, as in the last, Mr. Brown attributed the fatal result to the impaired health of the patient, though she would no doubt have died ultimately of the suppurative discharge.

The third case was one of multilocular tumor, very large and adherent. The operation was performed. The contents of the cyst were highly albuminous. Soon afterwards inflammation in the cyst was set up. This was subdued; but the patient sank from exhaustion, the discharge having become offensive. Extensive adhesions anteriorly were found after death. The external wound was healthy. Mr. Brown remarked that, though this could not be called a successful case, yet the operation itself was satisfactory, and could not be assigned as the cause of death—an event which, in all probability, would not have been long postponed had no such attempt to relieve the patient been made.

Mr. Brown drew the following conclusions—1. That some cases are curable by pressure—namely, those in which the cyst is unilocular, without adhesions, its contents clear and not albuminous, and where the condition of the patient affords time for the requisite perseverance, no counter-indications existing. 2. That some multilocular cases, and such as present adhesions of the cyst, are to be materially relieved and retarded by pressure. 3. That in cases where pressure has failed, or is contra-indicated—whether the disease be unilocular or multilocular—but without adhesions, extirpation affords a means of cure; but in all such cases, before this severe operation is resorted to, time and the condition of the patient not opposing, pressure should be first tried. 4. That where the extent and character of the adhesions forbid extirpation, and where pressure is contra-indicated, or has been practised and failed—in such cases the lateral operation above described presents a means of cure having the greatest promise of suc-

cess. 5. That this lateral operation may be performed with facility, and is attended with little danger; no case of peritonitis having occurred to the author in the many instances in which he has had occasion to cut down upon and divide the peritoneum in the operation in question, and also in others.

In the discussion which followed the reading of this paper, the general feeling of the members was against the proposition. Mr. HIRD expressed his belief that patients suffering from ovarian dropsy had, generally speaking, a better chance of prolonged life by constitutional treatment, quiet, and the avoidance of local interference, than by resorting to any operative procedure.

November 16.

Case of Aneurism of the Aorta—Diagnosis.

Mr. CANTON exhibited a specimen of this disease, and read an account of the case as communicated to him by Mr. Leonard.

The patient was a female 37 years of age, who, about three weeks prior to her death, consulted Dr. Roe for difficulty of breathing, increased on going upstairs, pain in the arm between the shoulder and elbow, pain below the middle of the sternum, and through to a point under the middle of the left scapula, together with great depression of spirits. Late at night, on the 14th of April, 1850, Mr. Leonard was suddenly summoned to her, and found her with evident signs of collapse, accompanied with excruciating pain in the abdomen. She expired soon after his arrival. The body was examined 60 hours after death. The abdominal viscera were generally healthy, the lungs were gorged with venous blood, the pericardium contained a much larger quantity of fluid than is generally found, and the heart was fat and pale, with its muscular parietes thinner than usual. The aorta was extensively diseased as far as the first intercostal arteries; behind the left sigmoid valve, and above the left coronary artery, was a large orifice, the size of a shilling, leading into an aneurismal sac, which pressed upon the pulmonary artery, thus causing a diminution of its calibre. One of its sigmoid valves was so compressed, that half its pouch was obliterated. There were two other irregular openings above the orifice of the sac, but not communicating with it. One of the bronchial glands above the aneurism was ossified. The head was not examined.

Mercury in Syphilitic Eruptions.

Mr. HUNT read a paper on the mercurial treatment of syphilitic eruptions, in which

he contended—1. That mercury was capable either of curing or aggravating all these diseases, according as it was administered. 2. That none of them would get permanently and radically well without mercury. In reference to the first proposition, the author expressed his belief that mercury acted most favourably when its effect was sudden and intermittent, and that a continued course was liable to aggravate the disease. He believed the medicine would effect all its therapeutic benefits without irritating either the gums or the bowels, and he was accustomed to administer it in two or more successive and increasingly energetic courses, until the disposition to disease was destroyed. In reference to the second proposition,—viz., that none of these diseases, generally speaking, would get permanently and radically well without mercury, the author alleged that the apparent spontaneous recoveries were for the most part only temporary, and that the alternative or non-mercurial cures were equally unsound, and to be accounted for on the theory that, under favourable circumstances, the system was capable of tolerating the presence of an animal poison for a time, and sometimes even for life; but even in that case, the children of the patient were liable to become affected, and that, in fact, a very large proportion of cutaneous diseases now existing in England proved to be cases of hereditary syphilis, originally treated, in the parent, without mercury.

In the discussion which followed, Mr. B. Travers mentioned a case of secondary symptoms, in which inunction effected a cure, when the internal use of mercury to salivation only aggravated the local symptoms. In reply to various inquiries Mr. Hunt observed, that Mr. Travers' case was one of great interest, and he had frequently met with such. As regarded the signs of latency of the disease, the fact that it was latent rendered it impossible to detail them. A sufficient number of courses of mercury could alone, he believed, destroy the latency. He applied the term "syphilis" to that disease in which all the characters are present, and in their ordinary order. Every copper-coloured eruption is certainly not venereal, neither is every syphilitic eruption of a copper colour. Colour is no test of the disease. He urged the necessity of watching the effects of mercury more closely than had been practised, and coincided with Mr. Roberts and Dr. Golding Bird in the opinion, that in anæmic cases it was well to combine mercury with iron; and, in plethoric subjects, with evacuants and a reduced diet.

ACADEMY OF MEDICINE, PARIS.

Nov. 12, 1850.

Foreign Bodies in the Air Passages.

DR. RENDU, of Compeigne, transmitted the history of a case of this kind. On the 8th May, 1848, a child of five years of age, having a haricot bean in his mouth, suddenly uttered a cry, and was attacked with a fit of suffocation. This attack recurred frequently until the fifth day, when M. Rendu determined upon performing tracheotomy. The operation having been performed the suffocation ceased, but the foreign body was not expelled, although the incision was kept open for some time. It was then left to itself, and healed by the fifteenth day: after this time the attack of suffocation reappeared, but less severely. Three months afterwards, the bean was expelled in a violent fit of coughing. The attacks of suffocation ceased, but a cough with purulent expectoration followed. This gradually disappeared; and in September of the present year, two and a half years after the accident, the child is in a perfectly good state of health.

SURGICAL SOCIETY OF PARIS.

Nov. 13, 1850.

Pathological Anatomy of a Tumor on the Inferior Maxillary Bone.

M. CHASSAIGNAC stated that he had removed the growth from the maxilla of the patient he had presented at the last meeting of the Society. The operation was simple, and the tumor easily detached. The bone presented a deep depression, in which the tumor was lodged; the walls of the cavity were smooth and unchanged, the bone was simply gone, the nerve was exposed at the bottom of the hollow, the bone at the lower border of the cavity was quite thin. Hemorrhage took place from the surface of the bone, to arrest which the actual cautery was employed. The patient was recovering.

The tumor presented to the naked eye a firm fibrous envelope, and a yellow homogeneous dry texture internally. Under the microscope it exhibited, according to M. Lebert's report, the structure of fibro-plastic tissue, and, at some points, cells of a doubtful character, which might be suspected to be cancerous.

Umbilical Puncture in Ascites—Hernia of the Omentum.

M. DEBOUT referred to a case published

in the *Bulletin Therapeutique*, in which tapping having been performed at the umbilicus, and the opening having been enlarged with a lancet, the omentum became protruded, and could not be returned. Inflammation of the part took place, and extended to the peritoneum within the abdomen, terminating fatally.

A Tumor, difficult of Diagnosis.

M. FORGET exhibited a tumor removed from the axilla of a female. It was round, soft, elastic, moveable, and had been situated in the subcutaneous cellular tissue. At the centre of its surface was a nipple-like projection, which was made prominent by pressing the inferior portion of the tumor. The diagnosis of M. Forget was that it was a fatty tumor enclosing a cyst containing fluid. The majority of the members of the Society were of the same opinion. A section of the tumor exhibited a lipoma of a soft tissue, demonstrating the difficulty of diagnosis of tumors, and the circumspection required of the surgeon in pronouncing upon their nature.

BIOLOGICAL SOCIETY OF PARIS.

Monthly Summary, August 1850.

Cerebral Hemorrhage in consequence of Rupture of a Branch of the Middle Meningeal Artery.

DR. DUPLAY communicated the case of an old man, seventy years of age, of a middle stature, strong and well made, who had for some time manifested signs of mental derangement. On the 11th of July he left his home, and did not return as usual in the evening. On the 15th he was found by the police: having a bruise on his face he was taken to the hospital. On the 16th he was seen by Dr. Duplay. He was then evidently insane. There was no impairment of sensation or motion. The pulse was natural. The patient acknowledged to no headache. No external or internal injury, beyond the contusions on the face and legs, could be discovered. The patient was bled, and sinapisms applied to the extremities; quinine was given internally. On the following days severe rigors and fever occurred. His nights were restless, and he was constantly getting out of bed. On the 21st a swelling appeared, as large as a hen's egg, in the region of the parotid gland; the skin over it was of a reddish colour, and hard to the touch. A little below the ear a blister appeared, of a violet colour. On the next day the tumor had increased. The patient became comatose, and died on the 22nd.

On post-mortem examination it was found

that the parotid tumor when cut into exuded pus from the duct of every lobule. The integuments of the head presented no trace of the bruises. The bones, very carefully examined, discovered not the slightest trace of injury. On opening the cranium, an extravasation of blood was discovered between the left parietal bone and the dura mater. The hemorrhage had occurred from a branch of the middle meningeal artery. The inner plate and diploë of the parietal bone had disappeared, the outer table only remaining. Corresponding with this was a thin friable coagulum, which filled a solution of continuity in the dura mater. The arachnoid sac, over the left hemisphere of the brain, was lined with a thin layer of coagulated and liquid blood. The posterior surface of the brain through the cortical substance was died of a pinkish colour where it was in contact with the effused blood. The substance of the brain was healthy throughout.

M. Duplay considers that in this case fungoid disease of the dura mater had existed; that the fall occasioned rupture of the artery, and the fungoid disease being at the same detached was diffused in the extravasated blood.

ACADEMY OF SCIENCES, PARIS.

Nov. 11, 1850.

Contagion and Treatment of Scabies.

M. BOURGUIGNON read an analysis of a paper which he submitted as a supplement to his Essay on Scabies, to which the Montyon prize had been awarded in 1846. From his later researches M. Bourguignon had arrived at the following results:—

A. With regard to scabies transmitted from the horse to man—

1. That an undoubted and indisputable instance of this form of communication of scabies has not yet been produced.

2. That the patients admitted into the Hospital St. Louis as cases of this manner of communication have always presented the acarus and furrow peculiar to man.

3. That the acarus from the horse, placed upon the author's arm, and on that of another healthy individual, did not pierce the integument, nor give rise to any appearance of the disease: in fact, that its transmission from the horse is impossible. But it does not follow that other skin affections are not so transmitted.

B. On the contagion of the scabies from the dog to man.

Scabies has not yet been shown to be thus transmissible, although the skin diseases of this and other animals may be communicated to man.

C. On the transmission of scabies from man to animals.

The acari of scabies from man having been placed in considerable numbers on the skin of the cat, rabbit, sparrow, and rat, were observed to pierce the integument, but did not give rise to any trace of the disease. Whence the author concludes that the contagion of scabies to animals from man is impossible; that certain cutaneous affections are produced on the skin of man by contact with the same class of diseases of animals; and that their treatment does not involve the death of insects (insecticide), but should be conducted on general principles.

In the second part of his paper the author enumerated a series of experiments on the various remedies for scabies. The three principal remedies, *pommade de pou-dre et au soufre, huile de cade, et pommade sulfuro-alcalin d'Helmerick*, are superior to all others, and will cure the disease in forty-eight hours, if applied once thoroughly over the whole body every twelve hours for a quarter of an hour, followed, twenty-four hours after the last application, by a soap-bath. The patient should cover his hands well with the application before rubbing it on the rest of the body.

A New Property of Chloroform.

M. AUGEND, of Constantinople, transmitted a memoir, in which he pointed out a property that places a very distinct line of demarcation between chloroform and ether; this is, its power of disinfecting organic matters. M. Augend related the following experiment:—

Take three wide-mouthed flasks, the first containing a few drops of ether, the second a few drops of chloroform, the third left empty. If in each of these a piece of beef be placed, and the flasks be closed and left undisturbed in the summer season, the following circumstances will be observed:—The meat, which was of a reddish brown colour in its natural state, changed instantly to a vermilion-red in the mixture of chloroform and air, while in the ether vapour no change occurred. At the end of a week the difference was greater still; the meat in the flask containing atmospheric air was but little changed in its aspect; that in chloroform had acquired the appearance of boiled meat. On opening the flasks it was found that the meat, both in the atmospheric air and in the ether vapour, was putrefied, and emitted a most offensive odour; while that in the mixture of chloroform and air had the sweetish taste and odour of chloroform.

M. Augend has ascertained that 1-200th of chloroform completely prevents the pu-

trification of fresh meat. The most apparent action of the chloroform is the rapidity with which it traverses the thickest tissues, and causes an immediate contraction of their parenchyma, with consequent exudation of the fluids of the structure experimented upon. The author further dwelt upon the value, in a medico-legal point of view, that chloroform thus possesses in arresting putrefaction.

A new theory of the Production of Images on the Retina.

Dr. DEZAULIERS addressed a note, in which he stated the theory that the retina, in transmitting impressions to the brain, corrects the images which arrive on its surface—that it acts, indeed, as a concave mirror, which reverses the images which fall upon it.

Medical Trials and Inquests.

COURT OF ASSIZES, DARMSTADT.

March 11th—April 11th, 1850.

Case of the Countess of Goerlitz—Opinions of Baron Liebig and Dr. Bischoff on the alleged Spontaneous Combustion of the Body—Medical speculations on the Cause of Death—Conviction and Confession of the Criminal.

[Concluded from page 903.]

AT the trial of JOHN STAUFF, at the *first Diet of Assize*, which was held on the 11th of March, 1850, after the verification of the circumstances already detailed regarding the Countess's last movements, the discovery and extinction of the fire, the finding of the body, and the state of her apartments, on the requisition of the President of the assizes, Professors Dr. von Liebig and Dr. Bischoff, of Giessen, were conjoined with the members of the Hessian Medical College, and the following questions referred to this body of experts—viz. :—

I. In the foregoing circumstances is it possible, probable, or certain, that the death of the Countess of Goerlitz, and the state in which her body was found on the 18th of June, 1847, had been the consequences of the so-called spontaneous combustion?

II. In the foregoing circumstances is it possible, probable, or certain, that the deceased had perished from the influence of a fire external to her, and that she was exposed to such an influence, either—

1. Through an unfortunate accident; or
2. Intentionally from her own or another's deed?

III. In the foregoing circumstances is it possible, probable, or certain, that the deceased had not been exposed to the action of fire till after her death, and in this case is it to be admitted that she had perished—

1. By suicide; or
2. By the hand of another,—perhaps by the fracturing of her skull, or suffocation (throttling); or
3. From an attack of disease, or the occurrence of some unlucky accident?

IV. In the foregoing circumstances is it possible, probable, or certain, that the obvious source of the fire (the burning writing-desk) was the sole cause of the burning of the body of the deceased, or was any further cause necessary for the production of this burning?

V. Is verdigris to be considered as a poison, and to what extent would the use of the sauce drugged with verdigris, or a portion of it, tend to endanger the life or the health of the party partaking of it?

Report of the Commission of Experts, 27th March, 1850.—Question I. To this question the Commission returned an unanimous reply in the negative, considering that the case in hand was not an instance of spontaneous combustion. With the exception of Dr. Graff, they all go further, and contend against the reality and possibility of death being producible in this way at all, it being, as they conceive, an occurrence, 1st, not sufficiently established on historical grounds; 2dly, opposed to the known and acknowledged truths and laws of flame (inflammation) and combustion; and 3dly, incompatible with the known laws regulating the possibility and continuance of human life in health and disease. From these reasons Dr. G. dissents, contending for the abstract possibility of spontaneous human combustion, though admitting that the case in hand was not an instance of it;* while Drs. Büchner, Leid-

* In the appendix to the trial we have a formal defence by Dr. Graff (Zeitschr. Ergänz., pp. 126-149) of his dissent from his colleagues on this and other points. His reasons for believing in the abstract possibility of spontaneous combustion are based alone on the authorities adduced by him in his 2d Report (*ante*), and in that of Dr. von Siebold. These, however, are met by Professor Bischoff (Ibid. 3 V. H. pp. 181, *et seq.*), who has shown the little reliance which can be placed on these, as well as the unscientific assumptions involved by the admission of their credibility. The correspondence with MM. Regnault and Pelouse, and the Préfet of Police at Paris, adduced by Prof. von Liebig (Ibid. Ergänz., p. 159), has shown that the only modern case of spontaneous combustion—viz., that published in the "Journal des Débats," of February 24, 1850—was destitute of the very slightest foundation in fact. Vide also Ann. d'Hyg., Nos. 87 and 88, *passim*.

hecker, and von Siebold, yielded their assent to them, though opposed to their previous convictions, when brought forward by von Liebig and Bischoff during the discussions carried on in the Committee.

Question II. To this question the Commission also agreed unanimously in returning a reply in the *negative*, believing it to have been impossible, in the circumstances of the case, that the deceased's death could have been occasioned by the action of a fire from without. Further, none of the circumstances suggested, in order to account for her death in this way, appeared to them to be satisfactory. 1st. None of the facilities in favour of *accidental* death from this cause were present here. The upper garment was a woollen one. The kindling of her head-dress, her hair, or the upper parts of her clothes, by the fire, would not have been so fierce and sudden a catastrophe as to have prevented her from at least calling for assistance. 2dly. They hold it to be an impossibility that any one fully aware of the extreme tediousness and torture of such a mode of death could *voluntarily* resign himself to it. 3dly. They also hold that in general it would be difficult, admitting its bare possibility, for one individual to destroy another *against his will* by the slow action of fire—a supposition, besides, which receives no support from the known facts of the present case. In conclusion, they point to the absence of the usual signs of vital reaction on the surface of the body as favouring their general inference.

Question III. This question is unanimously answered in the *affirmative*: the body of the deceased was *certainly* exposed to the action of fire *after* death.

As regards the kind and manner of her death prior to the exposure of the body to the fire, no definite conclusion can, they conceive, be come to, with the imperfect and defective evidence now alone attainable, which affords grounds for only the following balance of probabilities:—

1. *Suicide* in the circumstances of the deceased they think improbable. Suicide by fire they hold to have been impossible. One of the known facts, indeed, is compatible with the assumption of suicide by some one of the more rapidly fatal narcotic poisons—such as prussic acid, morphia, or strychnia—and fire-raising for its concealment. The Countess, before taking the poison, might have set fire to the ottoman, the sofa below the mirror, and the writing-desk, and then lain down in front of the latter to perish by the deadly drug. The only support of this hypothesis is the absence of the keys; while its likelihood is

destroyed by the torn bell-pull, the smoke from the Russian stove, and the varied indications of the Countess's death prior to the outbreak of the fire. The moral bearings of the case also lend strength to the *objective* evidence—such as absence of motive to suicide, or indication of such an intention on the part of the deceased. Its proof or disproof, as far as the search for the above-mentioned poisons is concerned, would, after nearly three years' interment, be impossible as regards prussic acid, and as regards morphia and strychnia very problematical.

2. The commission consider it unlikely that the Countess had perished either by disease or accident. The only supposable diseases in this case would have been either apoplexy, syncope, or stupor, of some sort. Several general reasons may, they think, be urged against the probability of all or any one of these having occurred. The following apply to all the three:—

1. On different grounds they think it highly probable that the Countess's death had been prior to the return of the Count at half-past seven. He perceived no traces of fire at this time; and it is probable that it had broken out later than this. If this was correct, the deceased could not have caused the fire.

2. If the Countess in any of the conditions above enumerated (apoplexy, &c.) could have accidentally kindled the fire, a candle and candlestick, or some other burning body, must have been found in her vicinity, which does not appear to have been the case.

3. Supposing even that there had been a candle or other burning body near her when overtaken by death, syncope, or sleep, the upper part of the writing-desk or of its contents could thus alone have been kindled; while it is certain that the combustion of the desk had spread itself from below upwards, and not in the reverse direction. In opposition to this, should it be further assumed that the lower drawer had been standing open, it is inconceivable, as the folding-leaf of the desk was down, that the burning body could have reached the drawer beneath.

4. Setting aside the incomprehensibility of the assumption that the lower drawer had been in some way kindled, we must then admit in addition, that the dead, syncope, or stupefied Countess, had, while occupying her chair in front of the writing-desk, sunk down on the floor prior to the spread of the fire, since not the lower, but the upper parts of her clothes, and of her person, were laid hold of by it.

5. Neither of the above presumptions harmonizes with several other points in the evidence; such as the absence of the keys.

the torn bell-pull, the burning of the ottoman, and the smoke from the stove.

Besides the above general objections against death from disease or accident, special reasons, on a closer inquiry, suggest themselves as bearing against the individual diseases presumed.

(1) The uniformly vigorous health of the Countess up to the time of her death, her habit of body, and her little advanced age, rendered the occurrence of any form of apoplexy in her case unlikely.

(2) Syncope would not have been immediately and directly fatal, and the spread of the fire would have tended to have roused her from such a state.

(3) Opposed to the supposition that the Countess, in falling asleep at the writing-desk, had accidentally set fire to it, had thus been first stupified, then suffocated, and afterwards dropped from her chair and been burned, we have the following considerations:—Such a slow combustion as is here supposed would have diffused around such a volume of gaseous products of a noxious and, in the end, destructive kind, calculated to irritate in the highest degree the organs of sense and respiration, as would of itself have roused a sleeping person, and compelled him to have taken measures for his relief.* Charcoal alone burns without giving out smoke or smell, and generates a very stupifying, and at length destructive alteration of the atmosphere.

3. That, in fine, the deceased had perished by the hand of another they consider to be possible and probable, not merely from the failure and exclusion of the other probabilities, but on direct grounds. In this category they reckon—

(1) The combustion of the body, for the effecting of which a second party was, they are convinced, absolutely necessary, as will appear by their reply to Question IV.

* We are not a little surprised at the ignorance shown in this argument of facts well known in this country. Dr. Alfred Taylor has recorded several cases of persons who had been suffocated in this way (vide *Man. of Med. Jur.* p. 739, and *Treatise on Poisons*, p. 795.) We have ourselves met with two instances of accidental death from suffocation from the smoke of burning buildings. In the first of these, five persons thus perished; and in the other, one adult woman. In the former of these accidents the parties were of various ages, from eleven to sixty years, and all, with, perhaps, one exception, had gone to bed sober, when within an hour the house was in flames, and some time elapsed before their dead bodies were got out of the building. What makes this illustration of the fallacy of the above argument the greater is, that the house in question was one of those old-fashioned ones, the walls of which were panelled throughout with wood. The combustion here was not, therefore, from coal fires, as in most of our published cases. In four out of these six cases the vault of the cranium was extensively deficient, as in the Countess's body, and the heads of some of the late . . . the limbs protruded.

(2) The position of the tongue. Though, from being uncertain what effect the action of the fire on the hard and soft parts of the face would have had in leading to the protrusion of the tongue, they do not consider the inference that it was necessarily caused by suffocation to be based on sufficient data, yet they are not prepared to combat the possibility of suffocation, and its connection with the position of the tongue.

(3) The fissure in the right temporal bone. This they consider fully as possible to have been the consequence of a blow on the head as of the effect of fire.

(4) The fragment of stuff* coloured with what was conjectured to be blood-stains. On this they lay little stress, from the nature of the stains not having been determined by chemical or microscopic examination.

On the whole, therefore, they are inclined to attribute the Countess's death with greater probability to the effects of a blow than to suffocation. They are not altogether satisfied of the possibility of a very powerful and strong man being able to seize and directly strangle such a woman as the Countess in the waking state. That she could have been overpowered during sleep they consider as quite unlikely, as she was in the custom of locking herself in on such occasions. From her habits of body and mind, she was a person likely to have made a powerful struggle for life. The conjecture which has the most probability is, that she had been first stunned by a blow, and then suffocated.

Question IV. The Committee were divided on the solution of this question. The minority, consisting of Drs. Graff, Leidhecker, and Rieger, did not conceive it likely that the combustion of the writing-desk alone would have sufficed to produce the burns on the Countess's body, and deemed it probable that some other and further cause was necessary to their production; while the remaining members of the Commission, constituting the majority, considered it both possible and probable that the apparent cause—the burning of the desk—was the real and sole one.

The opinion of the majority rested upon data both of a positive and negative kind.

1. The positive data assigned by them are as follows:—

(1) The temperature and amount of heat generated by the combustion of the writing-desk was sufficient to have caused the burns at the upper part of the Countess's body at the distance of about two feet from it. Two separate and independent calculations gave the bulk of the wood,

* A piece of linen found in the privacy of the house.

partly fir and mahogany, but chiefly oak, thus consumed to have been 7 Hessian cubic feet (3·863 cubic feet English), which, reckoning the cubic foot at the minimum of 18 Hessian lbs. (19·838 Eng.) would amount to 126 Hessian lbs. (138·83 lbs. Eng.). Of the wood, however, a residue equal to 2 cubic feet (1·103 Eng.) of charcoal was left unburned, which, at 7 lbs. Hessian per cubic foot, would correspond with 60 H. lbs. (66·12 lbs. Eng.) of wood. Sixty-six Hessian lbs. (72·73 lbs. Eng.) would have thus been burned, giving out a temperature of 184,800° C. (332,672° F.) Now the medium weight of a woman's body from 40 to 50 years of age amounts to 118 Hessian lbs. (124·526 lbs. Eng.) Assuming, further, that one-fourth of the weight of the Countess's body, thus calculated, was desiccated and reduced to the state of cinder, we have only from 7 to 8 per cent. of the heat required for this purpose, leaving 9·10ths, or nearly 171,780 C. degrees of it (*i. e.*, 309,236° Fahr.), which would serve to elevate the temperature of the 7617 Hessian cubic feet of air (4257·68 Eng.) in the parlour and closet. This calculation, moreover, can only be considered as the minimum temperature, as it leaves out the effects of the combustion of the other articles in the writing-desk, sufficient data for the calculation of these being wanting. Besides, the heat generated had sufficed to melt gold, silver, and iron, some of which metals require a temperature to effect their liquefaction of upwards of 1000° C. (1832° F.), while a heat of from 800° to 400° C. (572° to 752° F.) will char and ultimately burn animal matter. Again, stearine candles were melted at the distance of from 9 to 27 feet, which presupposes an atmospheric temperature of at least 58° C. (136·4° F.) Further, several chairs were burned which were at a greater distance from the fire than the dead body. Now it appears from the experiments of H. Merck, that a temperature closely bordering on the melting-point of lead is requisite to char paper and organic matter; and hence, if the heat given off by radiation from the burning desk acted on the surface of the chair at the distance from it at which it stood, it must have had a temperature at least as high as the melting-point of lead. Finally, one of the witnesses stated that the frame of the looking-glass, at about 16 feet from the fire, was so hot at the time that he could hardly bear to touch it.

(2) The time during which the combustion of the writing-desk had lasted was sufficiently long to have affected the body in the way described. It appeared to them that the burning of the writing-desk had commenced after the Count had gone out,

about half-past seven, which, as it had gone on till 11 P.M., or from 3 to 3½ hours, allowed a sufficient time for the effects of it on the body to have been produced. It is also to be noticed that at one period—*i. e.*, while there was still enough of fresh air in the room to maintain it—the combustion of the desk must have been very active, judging from the entire destruction of the folding-leaf, if we except a fragment of it left at the lock. The heat from the folding-leaf must have been the main cause of the desiccation of the body, the charring of it afterwards requiring less of the remaining caloric.

(3) The posture of the body was in the highest degree favourable to its combustion. It had lain immediately in front of, and at about two feet distant from, the writing-desk. The radiant heat would have been entirely concentrated on the body from the sides, the upper and lower parts, and especially the folding-leaf of the desk.

(4) The form of the burns corresponded with the assumption that these must have occurred at the spot where, and in the position in which, the body was found. The position of the body was more clearly evinced by the extent, situation, and limits of the burns, than by the depositions of the witnesses, and they even think that the head must have been closer to the fire than they had supposed.

(5) An experiment was undertaken at Giessen,* with the express object of imitating, as closely as possible, the burning of the Countess's body, when it was demonstrated, by means of the head of a human subject, that the burning in the present case could have been produced by the writing-desk, and in the position in which the deceased was found, particularly if the papers, articles of dress, and other combustible matters contained in this repository, were taken into account.

2. *Negative data.*—Besides the positive proofs that the burning of the Countess's body was caused by the combustion of the writing-desk alone, the majority of the Commission point to the following negative grounds on which they believe it partly impossible, partly in the highest degree improbable, that any other and further combustion, and through a second party, was necessary for this end.

(1) If the deceased, after being murdered, had been burned in the servants' room—from the chimney of which, as well as that of the parlour, smoke had been observed to issue on the night of the fire—the transport of the body, they think, particularly after a death of violence, could

* The details of this experiment are given in the Appendix (Zeitschr. Ergänz. p. 140). Vide also Ann. d'Hyg., 66 no., p. 400.

not have been managed without leaving some traces behind it, none of which were anywhere found.

(2.) The following circumstances negative the combustion of the body by alcohol, oil, charcoal, or wood, within the Countess's room.

(a.) The manipulation requisite for this, the adjusting of the fire, the necessary alteration of the position of the body, the employment of props for the head and chest in succession, and the special preparations for the burning of the upper extremities, as they were found burnt, could not have been accomplished by an unassisted individual. Any one of these manipulations would have demanded preparatives which would have left traces on the floor, the tables, chairs, the utensils, or other objects, which the closest scrutiny failed in detecting.

(b.) No supposable manipulation and change in the positions of the body will be found to be consistent with the form of the burns on the head, arms, chest, or even on the clothes.

(c.) It seems impossible that any person could have remained shut up in the comparatively small apartment of the Countess during the combustion of her body and clothing. He could neither have opened the doors nor windows for fear of detection, nor have remained in the room in the midst of the products of combustion, nor returned to it had he left it for an instant, on account of the smoke, vapour, and heat. He must besides have had time to change his dress, clean, and wash himself, to get rid of the smell which would have adhered to him, which even as it was had nearly betrayed him.

(d.) The combustion of the Countess's body by alcohol is opposed to these considerations:—

(aa.) The quantity of it that would have been required. The burning of the head alone would have demanded 3 maas of spirit (6·34 wine pints). To burn the chest and arms a still greater quantity of it would have been needed; and to place these parts in the spirit-flame, a much greater portion in reserve.

(bb.) Admitting even that 3 maas of alcohol was sufficient, the following calculation proves that a person could not have remained shut up in the room during its combustion. The parlour and closet contained 1,512 H. cubic feet (833·11 cubic feet, Eng.), to 68·64 lbs. H. (75 504 lbs. Eng.) The 3 maas of alcohol at 80° weigh 104 lbs. H. (11·775 lbs. Eng.), and contain 8·6 lbs. H. (9·46 lbs. Eng.) of absolute alcohol, for the combustion of which 20·64 lbs. H. (22·704 lbs. Eng.) of oxygen would be required. The oxygen of the air in the

room would be thus brought down from 21 to 14 per cent., which being much less than is contained in expired air, would no longer support life.*

3. The period of time at his disposal could not have admitted the interposition of other combustibles. From the time of the Count's leaving the house and the return of one of the servants, less than an hour elapsed (i. e., from half past 7 and twenty minutes after 8), too short a time to prepare and execute the combustion of the body, and then to obliterate all traces of the act. They cannot forget that the burning of a head alone with spirit of wine required an hour and a quarter.† The criminal must have been in constant dread of interruption, and could scarcely have meditated such an operation.

4. They cannot forbear expressing their doubt on psychological grounds that any one could be found capable not only of committing a murder, but likewise, under the pressure of its guilt, of also accomplishing such a complicated and fearful operation as the combustion of the body, by means of alcohol, &c., while it was so much easier for him after it was over to obliterate its traces by an ordinary fire-raising.

In concluding their reply to this question, the majority of the Commission express their opinion regarding the burning of the ottoman.

This combustion of the ottoman may have occurred, they conceive, accidentally, with the view of an ordinary fire-raising, or to get rid of blood-stains.

The *minority*, while they express their opinion to be that the combustion of the writing-desk was more than sufficient to produce the burns on the Countess's body, yet contend for the necessity of some further agency, on grounds which appear to be of a very slight kind. Their strongest argument is the one they draw from the circumstance of finding it impossible to fire the horse hair cover and stuffing of the ottoman.

Question V. In answer to this question the Commission unanimously conclude, that the verdigris in the sauce would have occasioned no serious or lasting effects on the life or health of the person swallowing it.

At the diet of the assize on the 11th of April, 1850, John Stauff was found guilty of the murder of the Countess of Goerlitz, and of the subsequent fire-raising, and

* This implies that the room was so closely secured that no one could enter from without.

† The reference here is to an experiment by Dr. Graff and others, to ascertain the possibility of the alleged combustion of the Countess with spirits of wine (*Zeitschr. Erganz.* p. 153. *Ann. d' Hyg.* No 83, p. 407).

attempt to poison, and sentenced to imprisonment for life.*

Correspondence.

HOMEOPATHIC HYPOTHESES, AND CHRONIC POISONING BY LEAD.

SIR,—As you have published in your journal of Oct. 25th, 1850, p. 717, and at the same time politely noticed my letter to you, I venture to suggest that your remarks do not meet my argument. The argument was not that, because a half-millionth part of a grain of lead would affect the system in health, that *therefore* a decillionth part of a grain of medicine would act on the human healthy body. The homœopathsists, so far as I can judge from their works (and I am indebted to a review in your journal of the work published by the English Homœopathic Association, "Homœopathy and its Principles Explained," for greater acquaintance with homœopathy), do not maintain, if I understand their views aright, that "a decillionth taken at one dose will be as potent in its effects on the system as a half-millionth proportion of the carbonate of lead diffused through water which is continued daily for weeks and months together, the lead acting not by its small proportion, as contained in the water, but by its special accumulation in the system." They maintain that a decillionth part of a grain of medicine, the powers contained in which have been developed by the process of preparation to which the medicine is subjected previous to use, when taken by a person with a disease to which the remedy is in homœopathic relationship, is potent against the disease. This you, as a logician, must acknowledge, is a proposition widely different from that which you put forth, your proposition being one which, I presume, no homœopathist would dispute.

I regret to find you still maintain that twelve jurymen are to be judges of a medical question. "A homœopathist treating a case according to the globular system resembles an orthodox practitioner who

professes to treat it with pills of *mica panis*. In the event of death in either case, there should be a coroner's inquest."

How is the question to be considered? If death, following any treatment, is to be proof that the death was caused by the treatment, then all deaths should be inquired into. But suppose a case of pneumonia died under homœopathic treatment, which is likely to occur, as cases die under all treatment (I find in your journal of Oct. 25 that 53 died in one week of pneumonia), and the question was to be considered by a Coroner, how is he to decide whether the treatment was correct or not? Is he to argue the question *a priori*, and to say, "It is contrary to common sense to attempt to cure diseases by infinitesimal doses of medicine, and therefore the treatment was wrong and criminal?" What would be the answer of the homœopathist? What but this?—I can prove, by witnesses who have been cured of pneumonia by infinitesimal doses of medicine, that infinitesimal doses of the appropriately-chosen medicines can cure pneumonia.

Could any Coroner, being a lawyer, presume to dispute such evidence? I have often thought it would be highly beneficial to have a verdict of "Manslaughter" passed against a homœopathic practitioner for treating an acute disease with infinitesimal quantities, and not succeeding with the given case (but do not put the homœopathist in Newgate, as was Mr. Pearce by Mr. Membury Wakley), and then let the question, if it is to be discussed and decided by twelve jurymen, but not Coroner's jurymen, be discussed fully and freely, and with all the sifting of evidence which legal acumen and medical skill can bring to bear upon the subject. Let me conclude this note by a quotation from the life of Dr. Andrew Combe, by his brother George Combe: it is part of a letter to Dr. Forbes, p. 483:—

"I cannot get over the extreme improbability of such visionary doses having palpable effects. But then we are too ignorant to decide what *may* or *may not* be in nature; and you admit that you have men of great talent, skill, learning, experience, and honesty, affirming their actual experience of active results, and can oppose nothing to that affirmation except your opinion. I have often said that, were I in practice, I should feel myself bound to test homœopathic practice. It has established its claim to a fair hearing, and only useful knowledge can come out of an impartial and adequate trial of its powers."—With this quotation,

I beg to remain, sir,

A CONSTANT READER AND
SUBSCRIBER.

* We copy the following notice from an English newspaper of date, 7th September, 1850:—
"John Stauff, condemned to perpetual imprisonment for having assassinated the Countess of Goerlitz at Darmstadt, has just confessed his crime. He declared that he had entered the room of the Countess to announce to her that he was going out, when, finding no one in the room, he was tempted by the articles of value he saw there to commit a robbery. While doing so the Countess came in: a struggle took place, and he seized her by the throat and strangled her. He afterwards placed the body on a chair, and putting round it a quantity of combustible articles, set fire to them."

his views freely; but when he admits that he is a believer in the assumption that the decillionth, or any other *illionth*, part of a grain of any substance whatever can have medicinal powers developed in it by "the process of preparation," and that it will cure a disease according to an alleged homoeopathic "relationship," we have no common ground of argument. The same amount of faith would justify a full belief in the potency of amulets and charms. No homoeopathic practitioner has any means whatever of knowing that he is prescribing any medicine, as he cannot prove its existence in the globe, and he is obliged to place implicit reliance upon the statement of some other person that the medicine is there. Shake up a box of homoeopathic globules, and you have no means of distinguishing sulphuric acid from charcoal, or belladonna from gold! If we are to rely upon supposed effects merely, we may, as well believe in the cure of diseases by amulets and galvanic rings. But it was lately shown, by a case reported in this journal, that when these alleged potent globules were administered to a patient without her knowledge they had no effect whatever; it was only when she took them *knowingly* that certain effects followed. We refer our correspondent to the case reported at p. 82 of the present volume.

THE STANDARD LIFE ASSURANCE OFFICE
AND MEDICAL REFEREES.

SIR,—Allow me to request the favour of your inserting in your next publication, if convenient, the following remarks with reference to a letter from Mr. J. H. Houghton, surgeon, of this town, which appeared in your paper of the 25th October last, but which has only come under my notice to-day:—

As the object of Mr. Houghton's communication appears to be to create an impression that the Standard Life Assurance Office has, in the transaction he refers to, admitted through me, its agent here, the equity of the claim set up by certain members of the medical profession to the payment of a fee by the Office, for answers given relative to the health and habits of their patients who may wish to assure their lives in such offices, I consider it my duty to explain the case which occurred between me and Mr. Houghton more fully than he has done, so that erroneous inferences may not be deduced.

When I called upon Mr. Houghton, at the time he states, to induce him, if possible, to alter his views respecting the question in dispute, I distinctly told him that the Company would not sanction my paying, on *their* account, the fee (10s. 6d.)

which he demanded, and that, if I did pay him the amount, I must do so out of *my own pocket*.

After some discussion, however, it was at length proposed by Mr. Houghton (to convince me that he was not actuated by selfish motives) that, if I paid him the half-guinea, he would give it to the Dudley Dispensary, or I might, if I thought fit, pay it myself to the Secretary of that Institution.

This offer I agreed to, and subsequently I paid Mr. Houghton the above-named sum to be so applied, and, when I left him, I stated that I should be happy to pay him in such cases similar sums to be so appropriated, never for a moment intending to intimate a readiness on the part of the *Standard Company* to pay any fees beyond those of their *own* medical officer, because that would have been acting in direct contravention of their repeated instructions, and a course too unbusiness-like for me to have pursued.

I have therefore to assure Mr. Houghton that the money which I paid him for the benefit of the Dudley Dispensary, and the fee which I paid to his "brother practitioner," were paid out of my own purse, and that I have not been, and never expect to be, reimbursed by the Office for such payments.

Under these circumstances, therefore, Mr. Editor, I think you will admit that the medical profession have no cause for congratulating themselves on having achieved any victory over the principles of the non-paying Assurance Offices.

I am, sir,

Your obedient servant,
J. B. FISHER.

Dudley, 20th Nov. 1850.

P.S.—I should, perhaps, mention that the occurrence above alluded to took place in the year 1849, so that Mr. Houghton has been somewhat tardy in bringing forward his remarks upon it. J. B. F.

DEATH FROM CHLOROFORM.

DR. ASCHENDORF states that a child, one year old, had been operated upon for a *nævus* under the influence of chloroform; with apparent success, when, on the child being removed from the table, its head fell back, and it instantly died in a state of convulsion. The quantity used had been six drops in the first instance, and a second application of three drops in some tow placed in a cup.—*Casper's Wochenchrift*, Sept. 6th, 1850. x

Medical Intelligence.**UNIVERSITY OF LONDON.**

M.B. SECOND EXAMINATION—1850.

Examination for Honours.

Tuesday, November 19.—Morning, 10 to 1.

*Physiology and Comparative Anatomy.**Examiner, Prof. CARPENTER.*

1. Describe the Anatomical elements of an ordinary *Mucous Membrane*; and state the principal modifications which these undergo in different organs.

2. Show the dependence of all *Vital Action* upon *Heat*, by examples drawn from different classes of Animals; and give an account of the chief provisions which are met with for its constant or occasional development in the living body.

3. Describe the *Nervous System* of an *Insect*, in its three states of Larva, Pupa, and Imago; point out its anatomical relations to that of Vertebrated Animals, and give an account of the probable nature of its actions, as elucidated by observation of the natural habits of the class, and by experimental observation.

Afternoon, 3 to 6.

*Physiology and Comparative Anatomy.**Examiner, Prof. CARPENTER.*

1. Give a general account of the process of *Respiration*, as performed in the Human subject, specifying the minute structure of the air-cells of the Lungs, and of their capillary network; the mechanism by which the air contained in them is continually renewed; the parts of the nervous system by whose action the movements are sustained; the average quantity of air that passes through the lungs per minute; and the nature and amount of the changes of composition effected in it by the respiratory process.

2. Describe the structure of the *Electric Organs* in the Fishes which possess them, and enumerate the principal phenomena of their action, pointing out the dependence of these upon Nervous agency.

3. Give an account of the Microscopical characters of the different forms of the *Nervous Tissue*, and of its chemical composition; state what may be probably regarded as their respective functions, and how far these are dependent upon oxygenated blood; and point out the relations between Nerve-force and Electricity.

Wednesday, November 20.—Morning, 10 to 1.

*Surgery.**Examiners, Sir STEPHEN HAMMICK and Mr. HODGSON.*

1. Give the different diseases of the Testicle, whether they arise from natural or accidental causes; you will describe the appearances, symptoms, and treatment, of each variety through its several stages; and when the testicle requires to be removed, you will give the method of performing the operation of Castration, with the subsequent management of the case, both locally and generally, under favourable and untoward symptoms.

2. Describe the various Polypi of the Nose and its communicating cavities, whether of a mild or malignant character: you will give the situation, attachment, appearance, symptoms, and mode of treatment, of each variety of Polypus, with the probable termination: you may further illustrate your observations by the detail of any cases which may have fallen under your notice.

Afternoon, 3 to 6.

*Surgery.**Examiners, Sir STEPHEN HAMMICK and Mr. HODGSON.*

1. Give the local and general treatment of a compound fracture of the Thigh-bone, about its middle third, whether arising from falls, blows, or gun-shot: and when it is decided to amputate the Limb, either immediately after the receipt of the fracture, or any subsequent period, you will give your reasons for such decision: you will describe the method of performing the operation of Amputation, with the subsequent management of the patient, both locally and generally, when under favourable or the dangerous symptoms arising from exhaustion, inflammation, suppuration, sloughing, hæmorrhage, or any other cause.

2. Enumerate the various diseases to which the lower part of the Rectum and Anus are liable: give the method of performing the operation required for a fistula in Ano, when in a simple or complicated form, with the subsequent treatment of the case according to the symptoms which are likely to arise.

Thursday, November 21.—Morning, 10 to 1.

*Medicine.**Examiners, Dr. BILLING and Dr. TWEEDIE.*

1. Sketch the more recent views as to the modes by which pus may be introduced into the blood. Describe the pathological causes, diagnostic symptoms, and treatment of Pyæmia.

2. Describe the morbid appearances in the brain in fatal apoplexy.

Afternoon, 3 to 6.

Medicine.

Examiners, Dr. BILLING and Dr. TWEEDIE.

1. Describe the principal varieties of morbid structure usually classified under the term *dropsy of the ovary*. What are its diagnostic signs?

2. Sketch the causes and the indications of treatment in the lithic, phosphatic, and oxalic diathesis respectively.

Friday, November 22.—Morning, 10 to 1.

Midwifery.

Examiner, Rr. RIGBY.

1. Describe the prophylactic treatment of abortion.

2. What is the meaning of a Mole Pregnancy?

3. What are the sources of danger to the Child's life in face and nates presentations, and how would you act under such circumstances?

4. Give the diagnosis of true from false pains.

RAVAGES OF THE CHOLERA IN THE WEST INDIES.

By intelligence just received from Jamaica it appears that the cholera was raging fearfully in Kingston, Port Royal, and St. Catherine's; and, up to the time of the packet's departure, the deaths in Kingston averaged at least 30 a-day. The latest official accounts published up to the morning of the 27th of October showed a total of 266 deaths in Kingston alone. In Port Royal and St. Catherine's the actual numbers were less; but, compared to the population of Kingston, the mortality has been far greater, particularly in Port Royal, where about an eighth of the population has been cut off. The deaths in all parts, with two or three exceptions, occurred among the lower orders of the black people, some of whom resided in miserable hovels and damp localities. Up to the last account received in Kingston, the north side of the island was free of the disease. An order has been issued by the Governor in Council forbidding communication coastwise between Port Royal and Kingston, and the uninfected ports, under certain restrictions. The authorities were particularly vigilant in the uninfected districts as well as the others, and the most stringent sanitary regulations were being generally adopted. In all districts the want of medical men was much felt. Kingston, with a population of some 40,000 inhabitants, contains only ten doctors, who, in addition to their private business, have in various instances to

attend the public prisons and institutions. Spanish Town contains but three doctors, and Port Royal none at all. At the latter place the services of the surgeons attached to the Royal Army were secured through the influence of the Kingston authorities. Our correspondent assures us that, should the disease make its appearance in the country districts, the mortality is likely to be alarming, as there is not to be found, in some of them, a doctor for twenty miles round. One of the Kingston newspapers (the *Daily Advertiser*) writes on the positive necessity of the lower orders being "compelled" to contribute to the support of a resident medical man in the respective parishes.

The state of alarm in which the public are at Kingston cannot be described. The poorer classes are in an awful state of destitution, and in some parts of Kingston and Port Royal whole families have fallen victims to the scourge within a few hours. Independently of the presence of cholera, the town was otherwise unhealthy, fevers and other diseases having appeared.

Further advices from Jamaica to the 31st ult. announce that the cholera was still raging with great virulence. The Kingston returns for the 28th report 53 new cases, 18 recoveries, and 30 deaths—remaining 85: at Port Royal 21 new cases, 3 recoveries, and 11 deaths—remaining 34. Sanitary measures were being actively carried into effect.

THE SANITARY STATE OF THE CITY.

ACCORDING to the Report presented to the City Commissioners by Mr. Simon, on Tuesday last, it appears that the deaths during the 52 weeks ending the 28th of September, 1850, in the city of London, were 2,752, and the rate of mortality was estimated at somewhat less than 22 deaths out of every thousand living persons; while under the ravages of the pestilence of last year the general death-rate had risen to the height of 30 in 1000.

POISONED GAME.

WE have already directed attention to the public sale of poisoned game, and the danger which might arise from using as food such as have not been shot. The following letter shows that the practice still continues in the London markets:—

"A friend of mine purchased at a shop and presented us with a brace of partridges; to-day we had them dressed, and they were partaken of by myself, my wife, and little boy, the remains being finished by our servant. About an hour after dinner I was seized with pains in the stomach, giddiness and pain across the forehead, accompanied by a metallic taste in the mouth,

evidently betokening the presence of some mineral poison: these symptoms continued, more or less, for three hours, after which their virulence surrendered to medicine, but still leaving me very unwell. My wife partook very sparingly, and was not affected; my little boy, who ate more, complained of similar pains in the stomach, but was not ill; our servant, on the other hand, soon after I was attacked became very unwell, and suffered from very much the same symptoms as myself. Whilst at dinner, I remarked, 'These birds have not been shot; they have been caught in some way.' I have now no doubt whatever that the partridges in question were taken with poisoned wheat or other grain; and for the future, in selecting partridges, pheasants, &c., I would strongly urge the public to choose those that are evidently shot, which can easily be ascertained on examination, such as by broken legs or wings, or shot-marks in the head, neck, and other parts of the body, and never pick out those birds that look clean, and whose plumage is unbroken.

DISCOVERY OF THE MAGNETIC PROPERTIES OF OXYGEN GAS.

At the last monthly meeting of the Royal Institution, Mr. Faraday announced to the members present his discovery (the subject of a paper sent in to the Royal Society) that oxygen is magnetic, that this property of the gas is effected by heat, and he believes the diurnal variation of the magnetic needle to be due to the action of solar heat on this newly-discovered characteristic of oxygen—the important constituent of the atmosphere. We do not mean to give the above as the terms of Faraday's announcement, or as the exact facts of the conclusions drawn from his last experimental researches, but only as a foreshadowing of the new results and views of one of our most eminent British philosophers. We must add, however, that Becquerel also has recently directed attention to a somewhat similar conclusion; he communicated to the Academy of Sciences at Paris, that oxygen is magnetic in relation to the other gases, as iron is to the rest of the metals, and inferred that it is probable or possible (we have not the paper by us to refer to) that the diurnal variation may be connected with this property of oxygen.

POOR-LAW MEDICAL RELIEF IN IRELAND.

ACCORDING to a report to Parliament, printed on Saturday, there were 468,028 hospital patients admitted in the year ending the 25th of March last. The average number under treatment per week during the year was 34,072, and the expenditure in the year was £177,039. 11s. 9½d.

for medical establishments under the Poor-Law in Ireland.

STATISTICS OF LUNACY—ANNUAL REPORT.

ON Saturday last was issued the fifth Annual Report of the Commissioners of Lunacy. The Report, which is addressed to the Lord Chancellor, was signed by Lord Ashley on the 30th of June last. It appears that on the 1st of January there were 15,079 lunatics in asylums, hospitals, metropolitan licensed houses, and provincial licensed houses, of which number 11,305 were paupers and 264 criminals. Out of 454 lunatic patients attacked by cholera 311 died.

MICROSCOPICAL ANATOMY.

MR. GOADBY, so well-known by his minute dissections at the Royal College of Surgeons, is at present engaged in giving a series of demonstrations on microscopical anatomy in Philadelphia, U.S.

OVIARTOTOMY.

THE New York Medical Gazette of October 19th, announces that Professor E. R. Peaslee has successfully removed an ovarian tumor, weighing twenty-four pounds.

AMPUTATION OF THE THIGH NEAR THE HIP-JOINT.

THIS operation was performed on Saturday week, at the Westminster Ophthalmic Hospital, by Mr. Guthrie, junior, in the presence of many London surgeons. It was not found necessary to remove the head of the bone. The operation was required for the removal of a malignant tumor of the thigh.

SUCCESSOR TO PROFESSOR WEBSTER.

DR. E. N. HOESFORD has been appointed to the Chair of Chemistry, in the Massachusetts Medical College, in lieu of the ill-fated Professor Webster.

ELECTION OF SURGEON TO THE GENERAL HOSPITAL NEAR NOTTINGHAM.

At a special general meeting of the Governors of the General Hospital near Nottingham, held on Thursday, Nov. 21st, Booth Eddison, Esq. F.R.C.S., was elected surgeon to the above Institution, vice G. M. White, Esq., who has resigned the appointment from impaired health.

TESTIMONIAL TO DR. CANHAM, OF RAMSGATE.

A PUBLIC DINNER was recently given to Dr. Canham, of Ramsgate, on the occasion of his retirement from practice in that town; and on the 25th inst. a handsome testimonial in plate was presented to him at the Town Hall. It consisted of four elegant inscriptions for this purpose.

amounted to £175, received from 140 subscribers. Dr. Canham has been in practice at Ramsgate for a period of twenty years, and has earned the good opinions of all.

CONSUMPTION CURABLE.

THE following advertisement, taken from a daily paper, is a curiosity in its way. It is a new feature in quackery to find one of the "cured" occupying the chair in a public institution:—

"CONSUMPTION.—Dr. COFFIN will deliver the TENTH LECTURE of his COURSE, being the second on CONSUMPTION, at the British Institution, Cowper-street, Finsbury, on Tuesday evening, 26th inst., on which occasion, Mr. Winchester, of Vauxhall-road, recently cured of consumption by Dr. Coffin, will take the chair. Several persons will also be upon the platform who have been cured of the same disease."

APOTHECARIES' HALL.

NAMES of gentlemen who passed their examination in the science and practice of medicine, and received certificates to practise, on Thursday, 21st November, 1850:—James Stoate, Bristol—Edward Adolphus Brown, Chelmsford—Joseph Wilson Beaumont, Sheffield, Yorkshire—Thomas Young Thompson, Sherburn.

OBITUARY.

MR. DORRINGTON.

WE regret to announce the death of Mr. Dorrington, which took place at Manchester on the 5th inst. The deceased was in his 33rd year, and had already acquired a high reputation in his profession. The immediate cause of death was water in the chest. The deceased was greatly respected by his brother practitioners in Manchester, a large number of whom followed him to his grave.

DR. JOHN TAYLOR.

ON the 22d inst., at 72, Upper Berkeley-street, Portman-square, Dr. John Taylor, late surgeon in the army, in the 80th year of his age.

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer 29.34
" " Thermometer 46.9
Self-registering do. Max. 57° Min. 37°
" From 12 observations daily. " Sun.

RAIN, in inches, 1.02.—Sum of the daily observations taken at 9 o'clock.

MEMOROLOGICAL.—The mean temperature of the week was 4° above the mean of the month.

BIRTHS & DEATHS IN THE METROPOLIS During the Week ending Saturday, Nov. 23.

BIRTHS.	DEATHS.
Males.... 708	Males.... 492
Females.. 673	Females.. 524
1381	1016

CAUSES OF DEATH.

ALL CAUSES	1016
SPECIFIED CAUSES	1016
1. Zymotic (or Epidemic, Endemic, Contagious) Diseases....	203
<i>Spurious Diseases, viz.—</i>	
1. Dropsy, Cancer, &c.	69
2. Brain, Spinal Marrow, Nerves, and Senses	124
4. Heart and Bloodvessels	20
5. Lungs and organs of Respiration	201
6. Stomach, Liver, &c.	26
7. Diseases of the Kidneys, &c.	18
8. Childbirth, Diseases of Uterus, &c.	9
9. Rheumatism, Diseases of Bones, Joints, &c.	8
10. Skin	4
11. Premature Birth	19
12. Old Age	68
13. Sudden Deaths	13
14. Violence, Privation, Cold, &c.	28

The following is a selection of the numbers of Deaths from the most important special causes:

Small-pox	11	Convulsions	41
Measles	26	Bronchitis	77
Scarlatina	41	Pneumonia	54
Hoping-cough	36	Phthisis	111
Diarrhoea	18	Lungs	13
Cholera	1	Teething	14
Typhus	47	Stomach	5
Dropsy	19	Liver	6
Hydrocephalus	26	Childbirth	9
Apoplexy	28	Uterus	1
Paralysis	23		

REMARKS.—The total number of deaths was 39 above the average mortality of the 47th week of ten previous years.

NOTICES TO CORRESPONDENTS.

D. R. should read with more attention when he indulges in hyper-criticism. Our comparison of the period of education required by the English College of Surgeons with the period required by the Colleges of the United States referred obviously to the education of surgeons generally, i.e. of those who constitute the great body of the profession in the two countries. For these the period is four years in England, and only eight months in the United States. D. R. ought to have known that our comparison did not extend to F.R.C.S.'s of the United States (if there be such a class), and therefore to have placed in comparison the F.R.C.S. period of the English College with the Membership period of the United States, would not only have been a deceptive but an untrue representation of the facts. We must express our surprise that an examined Fellow of the College should commit such a blunder.

Mr. J. W. D. Brown's communication has been received, and will be inserted in an early number.

We regret that we are compelled to postpone the Reports of the Liverpool and Newcastle Pathological Societies until next week.

Reports have been received from Guy's Hospital and King's College Hospital.

Mr. R. Sarell.—The letter from the Paris Medical Society has reached us.

Dr. T. H. Barker.—Application should be made to the publishers, with whom these matters entirely rest.

The contribution of Mr. Samuel Berry shall be inserted, if possible, in our next number.

RECEIVED.—Mr. J. O. Burgess.—Mr. Deane.—The "Yorkshire Gazette."

CORRECTION.—At p. 954 of last number, col. 1, line 17 from top, for "8 per 1000," read "11 per 1000."

Lectures.

COURSE OF LECTURES

ON

DISEASES OF THE HEART.

Delivered at St. Vincent's Hospital during the Session 1849-50.

By O'BRYEN BELLINGHAM, M.D.

Fellow of, and Member of the Court of Examiners of, the Royal College of Surgeons in Ireland, and one of the Medical Officers of the Hospital.

LECTURE XIV. (CONTINUED).

GENERAL SIGNS OF HEART DISEASE.

Indirect or secondary symptoms of heart disease (continued) — Dyspnœa—Starting in alarm from sleep—Cough—Hæmoptysis—Pulmonary apoplexy.

Dyspnœa.

THE respiratory movements, like the motions of the heart, are involuntary, though the former are much more under the influence of the will than the latter: they belong to the class of movements termed "excito-motory" by physiologists. The object of the respiratory process, it is scarcely necessary to say, is to bring the venous blood in contact with atmospheric air, which is accomplished through the medium of the innumerable capillaries that ramify upon the air-cells of the lungs; and the changes which ensue in the blood and in the air take place through the delicate membranous walls of these cells. From the manner in which these parts are arranged, every particle of the blood, in its passage through the capillaries of the lungs, is necessarily exposed to the action of the oxygen of the air; and upon the regularity with which the process is carried on the regularity of the heart's action depends, anything which impedes or interrupts it tending to derange or to disturb the general circulation.

When the respiration and circulation are perfectly tranquil, the respiratory movements are performed almost entirely by the diaphragm, which, contracting in inspiration, enlarges the diameter of the thorax from above downwards, and, relaxing in expiration, returns to its former state. When the respiratory movements are carried on more actively, the thorax is enlarged in all its diameters,—the antero-posterior, lateral, and vertical. When, finally, the respiration becomes difficult and laborious, all the muscles in addition, which receive filaments from the system

of respiratory nerves, are brought into action.

In a state of health the exact amount of venous blood is transmitted by the right ventricle to the pulmonary artery, which can be converted into arterial blood in the lungs; and the exact amount of arterial blood is returned by the pulmonary veins to the left auricle, which will pass freely through the chambers of the left side of the organ. At the same time an amount of atmospheric air is taken into the lungs, at each inspiration, sufficient to convert the blood contained in its capillaries into arterial blood. We have likewise seen that, in a state of health, there is an exact standard or proportion between the number of the pulsations of the heart and the number of respirations in a given period: hence, when the circulation is hurried by exercise, the respiration becomes more frequent in proportion; and, as the circulation becomes again tranquil, the respirations diminish in frequency. There is, therefore, in health, a perfect equilibrium between the circulation and the respiration and if from any cause this equilibrium disturbed, dyspnœa will ensue.

It sometimes happens that the blood is transmitted with too little force by the right ventricle to pass freely through the pulmonary capillaries: more frequently, however, the blood is impeded in these vessels, owing to some obstruction to its passage through the left chambers of the heart. Again, it sometimes happens that the blood is transmitted with increased force, or in greater quantity than natural, by the right ventricle: more frequently, however, the air cannot enter the air-cells of the lungs in sufficient quantity to decarbonize the blood transmitted to these organs. In either case there will be a want of due proportion between the extent of the oxygenating surface and the mass of the blood; the respiration will become hurried, or difficult, or laborious, according to circumstances; the dyspnœa will pass into orthopnœa, which, in extreme cases, may terminate in asphyxia.

Dyspnœa will therefore ensue whenever the pulmonary capillaries contain more blood than can be oxygenated by the air admitted into the air-cells, or whenever too little air can enter the air-cells to oxygenate the blood contained in the pulmonary capillaries. In either case the respirations may be more frequent than natural, or the number of the respirations and of the pulsations of the heart may not bear their normal proportion to one another. Under such circumstances the respiration is no longer an involuntary act, but requires the aid of a number of the voluntary muscles, particularly in inspiration, in order that a

sufficiency of air may be taken in to convert the venous into arterial blood. In extreme cases all the muscles which receive branches from the respiratory nerves,—viz. the facial, the external respiratory nerve, the spinal accessory, the glossopharyngeal, the par-vagus and its laryngeal branches, and the spinal nerves distributed to the muscles of the trunk,—are called into almost convulsive action.

Dyspnœa in its mildest form, when the individual is merely short-winded, or readily put out of breath, or finds a difficulty in ascending stairs, although a frequent attendant upon disease of the heart, is observed in many cases where this organ is not in the least engaged. Indeed, dyspnœa is seldom marked in cardiac disease until the pulmonary tissue and the bronchial mucous membrane become secondarily engaged, and it is then often one of the most distressing symptoms.

Dyspnœa to a considerable extent may, however, exist, and yet the patient will deny its existence, either unconscious of it, or regarding it as too trifling to be mentioned. This is not unfrequently witnessed in the class of patients whom we see in hospital, in whom the dyspnœa has made its approaches so gradually that they have become habituated to it, and regard it as of trifling consequence. This, no doubt, is in some measure due to the parts having had time to adapt themselves to the altered circumstances, and in some measure, also, "to the sensibility being gradually blunted by the circulation of ill-oxygenated blood." Thus, as Dr. Williams* observes, "persons affected with extensive emphysema of the lungs are habituated to an imperfect state of respiration, which is shown by a constant lividity of the lips and cheeks:—such an appearance would be a sign of approaching death in other persons."

The severity of the dyspnœa, both in disease of the heart and lungs, is, in the majority of cases, in proportion to the suddenness with which this symptom has set in: thus in capillary bronchitis, in pneumonia engaging a considerable portion of both lungs, in pleuritis with copious effusion, it is the most prominent symptom. It is likewise so in the acute forms of cardiac disease, as where a large amount of fluid is suddenly effused into the pericardial sac,—the result of pericarditis; or where the circulation through the heart becomes suddenly impeded or obstructed, as occurs in some of those cases of acute endocarditis which prove rapidly fatal.

It sometimes happens that a very advanced stage of valvular disease is accompanied by little or no dyspnœa: such cases

are usually met with among the better classes. In the majority of the cases of this kind which come under treatment in hospital, where the patients are obliged to labour for their bread, often exposed to all the vicissitudes of the weather, and often, at the same time, the victims of intemperance,—more or less dyspnœa is constantly present; and in very aggravated cases, besides the habitual dyspnœa, it occurs in paroxysms of increased intensity, accompanied sometimes by a sense of constriction or suffocation, threatening dissolution, which is truly distressing to witness.

When more or less dyspnœa is constantly present, and yet no symptoms of pulmonary disease can be detected, or there is no other manifest cause for its presence, "we may," Dr. Furnivall* observes, "suspect disease of the heart almost from this symptom alone." In some cases which he met with he says—"This was the only symptom for a long time observable, and it appeared long before the characteristic murmur of diseased mitral valve could be detected."

The diseased states of the heart, of which dyspnœa is a prominent symptom, are—pericarditis, with copious liquid effusion; endocarditis, with obstruction in the orifices or chambers of the heart; dilatation of the ventricles, with thinning of the parietes; hypertrophy of the ventricles in an advanced stage, or a combination of hypertrophy with dilatation; valvular disease, particularly considerable contraction of the orifices of the left side of the heart, or free regurgitation through the mitral orifice; or aneurism of the aorta, compressing the trachea, or large bronchi. Dyspnœa, however, is not limited to cases of organic disease of the heart; it is a frequent symptom of the anæmic or of the plethoric state. In such cases, however, the dyspnœa is not constant, but is experienced on walking quickly, or ascending stairs, or on making any unusual exertion. In the former of these cases the dyspnœa, according to Andral, arises from there being too much air relatively to the quantity of blood to be oxygenated; in the latter, from there being too much blood relatively to the air entering the bronchial tubes.

Patients labouring under valvular disease in an aggravated form, complicated with dilatation of the ventricles, and accompanied by much congestion of the pulmonary tissue, are predisposed to attacks of pneumonia, which aggravate remarkably the habitual dyspnœa, and usually hurry on the disease rapidly to a fatal termination.

Starting in alarm from sleep.—In con-

* Principles of Medicine.

* Lancet, 1846.

nection with dyspnoea, a symptom sometimes observed in cases of heart-disease of long standing is a sudden starting in alarm from sleep, accompanied by a distressing feeling of oppression and violent action of the heart, often following a frightful dream. This symptom, Dr. Willis* observes, is probably connected with impeded circulation through the lungs. "As we fall asleep," he observes, "the respiration becomes considerably slower than it was immediately before: in very deep sleep each respiration is an effort, and is apparently only performed from the increasing urgency of the uneasy sensation that is at once allayed by taking in a new draught of air. The pulse at the same time falls in frequency, and the balance is maintained between the activity of the circulation and that of the respiration. But, with a heart acting faultily, pushing rather more blood, perchance, into the pulmonary artery than it can transmit, or, on the contrary, refusing readily to receive so much as returns to it by the pulmonary veins, the balance between the activity of the circulation and that of the respiratory system is destroyed, accumulation takes place in the lungs, the patient awakens in alarm, and, plying the muscles of respiration more vigorously, shakes off the sense of suffocation that was beginning to be imminent."

Cough.

Cough is not a necessary symptom of cardiac disease; few cases, however, go through all their stages without this symptom being present in a more or less marked degree, particularly if congestion of the lungs ensues. The act of coughing, which consists in a sudden voluntary or involuntary expiratory effort, is almost always the result of some irritation of the mucous membrane lining the larynx, trachea, or its ramifications. The nature of this irritation varies in different cases; thus it may depend upon inflammation of some portion of the mucous membrane which lines the air-passages, in consequence of which the sensibility of the membrane is increased, and its secretions, which are not irritative in a healthy state of the membrane, become so, and occasion cough; or it may arise from the accumulation of the secretion in the air-tubes, by which the free admission of air is interfered with, and cough is excited in order to get rid of it. Again, it sometimes happens that the calibre of the air-tube is narrowed by submucous infiltration, or by a morbid growth pressing upon a bronchial tube, and diminishing its calibre, when the irritation excited gives rise to cough.

When cough arises in diseased states of the heart it may depend upon any of the causes just mentioned; most frequently, however, it arises from the irritation occasioned by the increased quantity of fluid poured out into the air-passages, the amount of which is sometimes considerable, and which, by its simple presence, excites irritation, or, by its quantity or situation, impedes the passage of the air during respiration, and cough is excited in order to get rid of it.

In the early stages of cardiac disease, if cough is present, it is in general due to an accompanying bronchitis. In the advanced stage of certain forms of disease of the organ cough is scarcely ever absent, and it is always associated with more or less dyspnoea. In such cases oedema of the pulmonary tissue is generally present, the congested condition of the lungs being in part relieved by the transudation of the more watery parts of the blood into the air-cells and minute tubes: but this very effort of nature to relieve the congested state of the capillaries of the lungs becomes often the source of considerable distress to the patient; the copious secretion poured out, as it can only be removed by coughing, excites and keeps this up; while, if the secretion from the bronchial mucous membrane is at the same time very profuse, and the patient much debilitated, the expectoration becomes difficult, and not unfrequently the patient dies asphyxiated from this very cause.

The diseased states of the heart, in which cough is most frequently a symptom, are valvular disease of the left side, particularly contraction of the mitral orifice, dilatation of the ventricles and hypertrophy of the same parts; in fact, the same forms of disease in which dyspnoea is observed: indeed, the latter is seldom prominently marked, without cough being likewise troublesome.

The cough at first is usually dry, or accompanied by scanty expectoration,—eventually, when congestion or oedema of the lungs is superadded, it becomes free, the expectoration is copious, and consists frequently of a colourless, watery, or mucous fluid. The cough, too, comes on in paroxysms, and in the advanced stage the fits are sometimes both frequent and prolonged, by which congestion of the lungs is still further increased. Patients in whom the pulmonary circulation is much obstructed, and in whom congestion of the lungs has lasted long, appear likewise to be more subject than others to bronchitis, by which the habitual dyspnoea and cough are considerably aggravated.

It is scarcely necessary to observe, that whenever the trachea, or a large bronchial

tube, is compressed by the growth of an aneurismal or other tumor in the cavity of the thorax, cough is a prominent symptom. In such cases, the sensation first experienced by the patient is usually as if something existed in the air-tubes which he would be relieved by expectorating, and he makes many ineffectual efforts to do so. Afterwards, the act of inspiration or of expiration, or both, are accompanied by peculiar sounds, which in general are sufficiently characteristic, and when once heard can scarcely be mistaken, but upon which it is not necessary to delay here.

Hæmoptysis.

The blood which escapes upon the mucous surface of the air passages, and is expectorated, may have its source either from the bronchial or from the pulmonary vessels; and it may come, either from a ruptured vessel, or it may transude without the rupture of any vessel.

We have already seen how intimately related the functions of the heart are to those of the lungs, and how liable the pulmonary circulation is to be impeded when the valves or orifices of the heart are diseased, and prevent the free passage of the blood through its chambers. We have likewise seen the manner in which the over-distended capillaries of the lungs are relieved by the transudation of the serous portion of the blood, giving rise to œdema of the pulmonary tissue. If the impediment to the passage of the blood becomes still more considerable, the vessels may relieve themselves, by allowing the blood itself to transude, when it may be expectorated; or rupture of a vessel may take place, and the blood be effused into the tissue of the lungs, constituting the state termed pulmonary apoplexy.

Hæmoptysis is a symptom only of the advanced stage of cardiac disease, and is limited to certain forms of it; it is most common in young subjects, and in individuals who at some former period had laboured under acute inflammation of the lining membrane of the organ. It was at one time a common opinion, and is probably still with some, that when hæmoptysis takes place in the progress of cardiac disease, or when pulmonary apoplexy occurs in such cases, they depend upon hypertrophy of the right ventricle, transmitting the blood to the lungs with so much increased force as to cause rupture of the minute vessels, and effusion of blood, either upon the mucous surface, or into the pulmonary tissue. This, however, is not quite a correct view of the subject, because, even though hypertrophy of the right ventricle did exist, the blood would find no difficulty in returning by the pulmonary veins to the

left side of the heart. But hypertrophy of the right ventricle is itself almost always a secondary lesion, and the result of valvular disease at the left side, particularly contraction of the mitral orifice. We have seen that this form of valvular disease is a very frequent cause of pulmonary congestion; and, if hypertrophy of the right side supervenes, as is frequently the case when the contraction of the mitral orifice is considerable, the pulmonary circulation being placed between a strong right ventricle propelling the blood with increased force into the pulmonary artery, and a contracted state of the left auriculo-ventricular orifice, by which the pulmonary veins are prevented from freely pouring their blood into the left auricle, it is not surprising that the blood-vessels should relieve themselves by allowing their contents to transude, or that rupture of a vessel should sometimes take place; particularly if anything occurs to hurry the action of the heart.

Hæmoptysis would be a much more frequent symptom than it is in those cases, if, as the right ventricle became hypertrophied and dilated, the tricuspid orifice preserved its normal dimensions, or became narrowed; but as a general rule, almost, when the cavity of the right ventricle becomes dilated, and its walls hypertrophied, the tricuspid orifice increases in size, by which free regurgitation is permitted into the right auricle: part of the strength of the ventricular systole is thus wasted, and the injurious consequences which might otherwise ensue are prevented.

Hæmoptysis is a more common symptom of pulmonary than of cardiac disease; the source of the hæmorrhage is not the same, however, in the two cases: the appearance of the blood is likewise often somewhat dissimilar, while the circumstances under which the hæmorrhage occurs, and the conditions of the circulation at the time, are different. For instance, the hæmoptysis which occurs in phthisis is of the active form, and arises from determination of blood; the general circulation is quickened, and the hæmorrhage comes (as was first pointed out by Dr. Graves*) from the bronchial vessels. Pure blood is frequently expectorated, which is frothy, and of a bright florid hue, though it is often also in clots, and of a dark colour, if it has lain for any time in the air-tubes: the quantity expectorated is sometimes considerable, and in the intervals the sputa are frequently streaked with blood: finally, this form of hæmoptysis is seldom associated with pulmonary apoplexy.

On the other hand, the hæmoptysis which occurs in the advanced stage of car-

diseased disease is of the passive form, and is the result of venous congestion, not of local determination, while the circulation is not quickened generally. The blood expectorated comes from the pulmonary vessels, and the amount is sometimes considerable. This will, however, in some measure, depend upon whether the hæmorrhage is owing to rupture of a vessel, or depends simply upon the transudation of the blood: in the former case the blood may have a florid hue, or it may present a dark or grumous appearance; in the latter, the blood is frequently intimately mixed with the sputa. Finally, hæmoptysis from this cause is occasionally associated with pulmonary apoplexy.

The diseased conditions of the heart in which hæmoptysis most frequently occurs are:—

1st. Considerable contraction of the mitral orifice.

2d. Dilatation with attenuation of the left ventricle.

3d. Extreme contraction of the aortic orifice.

4th. Hypertrophy with dilatation of the right ventricle. This, however, is almost always a consecutive lesion to valvular disease at the left side of the heart.

Few cases of considerable contraction of the left auriculo-ventricular orifice go through all their stages without the occurrence of some hæmoptysis. It is rare in the regurgitant lesions of the orifices of the left side of the heart, particularly so in aortic regurgitation.

The explanation of the cause of the hæmoptysis in all these cases is pretty nearly the same. Thus, when the mitral orifice is contracted, and the passage of the blood through it is impeded, congestion of the lungs in general sooner or later follows, particularly if the parietes of the right ventricle become hypertrophied: under such circumstances any sudden or continued exertion on the part of the patient, or anything which hurries the heart's action, may give rise to hæmoptysis, the over-distended vessels allowing of the transudation of the blood, or rupture of a vessel ensuing. When, again, the cavity of the left ventricle is dilated, and its parietes are attenuated, the weakened ventricle is unable to propel its contents with sufficient force to pass through the capillaries; the blood accumulates in the left cavities of the heart, the pulmonary veins are unable to empty themselves, congestion of the lungs ensues, followed, as in the preceding case, by hæmoptysis.

Pulmonary apoplexy.—When the blood, in addition to being expectorated, is extravasated in the tissue of the lung, it constitutes, as has been already said, the state

with which we are familiar as pulmonary apoplexy. There is then always rupture of a vessel, and the effused blood may fill the air-cells, or the areolar tissue of the lungs may be infiltrated with this fluid, by which the air-cells of the part are compressed or obliterated, and no longer permit of the entrance of air.

Pulmonary apoplexy, although very generally accompanied by hæmoptysis, may occur without it; and, according to its extent, to the quantity of blood extravasated, and to the amount of lung engaged, it may prove immediately fatal, or only so after a longer or shorter interval, or the patient may recover. The attack may be sudden or otherwise, and the symptoms will vary in intensity according to the circumstances just mentioned. It is usually accompanied by increase of the palpitation and of the dyspnoea, by oppression of the chest, by pain referred to some part of the front of the chest, or to the sides, or shoulders, or by a sensation of heat in the chest, by anxiety, by cough, with more or less hæmoptysis, by paleness of the face if the amount of blood expectorated is considerable, or by injection of the face if it is less, as well as by certain physical signs, upon which it is not necessary to dwell here.

The diseased states of the heart in which pulmonary apoplexy is most likely to supervene are those in which the impediment to the return of the blood to the left side of the heart is greatest: consequently it is most frequently observed in diseased states of the left auriculo-ventricular orifice, particularly in a very contracted state of this orifice. When this morbid condition has followed rapidly upon an attack of acute endocarditis, and when the subject is still young, pulmonary apoplexy, as M. Gendrin* observes, is more liable to occur than when the disease has come on gradually, and when the congested state of the lungs has been in some measure relieved by the escape of the serous portion of the blood, constituting œdema of the lung, or when anasarca of the cellular tissue has supervened.

In addition, M. Gendrin remarks, as free regurgitation through the tricuspid orifice usually occurs in these cases, this tends materially to diminish the injurious effects of congestion upon the lungs, and renders pulmonary hæmorrhage less liable to occur, because, at each systole of the right ventricle, a considerable portion of the blood is propelled backwards into the right auricle, which prevents the pulmonary artery from being over-distended, and diminishes the risk of rupture of a vessel. From M. Gendrin's remarks under this

head, it might be supposed that regurgitation through the tricuspid orifice had been overlooked by preceding writers:—"It has always surprised me (he observes) to find this insufficiency of the valve unnoticed by writers, even by those who have made the diseases of the organ their particular study." When describing the mechanism of the action of the valves, however, we saw that the tricuspid valve permits regurgitation even in a healthy state of the heart; and that a condition of the right auriculo-ventricular valve and orifice which permits a partial reflux of blood into the right auricle, when the ventricle is much distended, or when the passage of the blood through the lungs is temporarily impeded, is a natural provision, which had been noticed so long ago as the time of Hunter,* and has been particularly described by Mr. Adams,† and Mr. King.‡

Original Communications.

OF THE

RISE, PROGRESS, AND VARIOUS TERMINATIONS OF CHRONIC OVARIAN TUMORS.

By EDWARD JOHN TILT, M.D.

Senior Physician to the Paddington Free Dispensary for the Diseases of Women and Children, &c. &c.

AFTER describing the structural appearances of chronic ovarian growths,§ we attempted to discover in what elementary tissues they originate, and the causes which determine their development, so as to enable us to form a correct notion respecting their nature: we must now detail their progress, and become acquainted with the accidents by which this may be marked, in order to be furnished with all the elements of a correct diagnosis, when hereafter we seek to distinguish ovarian tumors one from another, and from tumors of other organs. Unlike the acute ovarian tumors which we have already described, the more chronic growths of the same organs are remarkable for their insidious beginnings and for the length of time which they generally require to arrive at their full growth; and their

development is often unattended by fever, whilst we have seen acute ovaritis excite a degree of fever proportionate to the amount of suppuration occurring in its tissue or in its vicinity.

In following the author in a description of the progress of these tumors, the reader must recal to his recollection the extent and nature of the abdominal and pelvic cavities, in part osseous and inextensible, and in part most elastic and yielding. He must also bear in mind the relative position of the abdominal and pelvic viscera, and particularly that of the organs by which the ovaries are surrounded. The enlarged ovarian tumor may encroach upon all these organs, and it is only by following out these encroachments on every individual organ located in the abdomen and pelvis that we shall be able fully to appreciate these symptoms, and to distinguish chronic ovarian tumors from the diseases of neighbouring organs, with all of which they are daily confounded.

In thus carefully studying the influence of chronic ovarian tumors on the different viscera, we shall attempt to continue what Dr. Bright has so admirably begun in the *Guy's Hospital Reports*; that is to say, a more perfect study of abdominal pathology, a subject of immense importance, since full two-thirds of all the diseases to which we are liable either ultimately attack or soon implicate the abdominal viscera.

When treating of the morbid anatomy of chronic ovarian cysts we were able to class them into—1. Piliiferous; 2. Hydatid; 3. Fibro-serous; 4. Heteramorphous tumors; and 5. Cancerous encysted tumors. But it is useless to make this distinction in the description of the effects of these tumors upon neighbouring organs, because these effects are for the most part mechanical, or the result of inflammation, and independent of their nature. We shall see at a future period how difficult it is to distinguish these varieties, as well as to distinguish from ovarian tumors those which are more rarely developed in the Fallopian tubes, but which have the same symptoms and require the same treatment.

The effect of ovarian tumors we shall generally find is—I. To displace more or less the other viscera. II. To give rise to more or less disturbance of their natural functions. III. To wear and

* Treatise on the Blood.

† Dublin Hospital Reports, vol. iv.

‡ Guy's Hospital Reports, vol. ii.

§ Lancet, vol. ii. 1849.

perforate them when these organs are hollow, so as to evacuate by their channel what cannot be reabsorbed. Besides this common mode of acting on all the abdominal and pelvic viscera, these tumors disturb the functions of other organs to which they are allied by some consentaneous bond of union such as the breasts, on which they cannot possibly exert any other than a nervous influence.

We shall hereafter see that the distinction of the symptoms of ovarian tumors into pelvic and abdominal is not without practical importance; but at present, when investigating the march of these tumors, we shall group together the whole of their effects on the same organs, so as not to mar the unity of the plan by too minute a division of the subject. After having fully inquired into the progress of these tumors in relation to other organs, we shall also see what is the *vital activity* of these growths, what is their *duration*, and what their *termination*.

One of the first, certainly the most important organ with which ovarian growths interfere, is the womb, and we shall have to examine the action of these tumors—

I. On the unimpregnated womb.

II. On the impregnated womb.

1. Action of ovarian tumors on the impregnated womb.

The ovarian tumor at first retains the usual position occupied by the ovary, but when it has attained the size of a hen's egg, if free from inflammatory adhesions, it has a tendency to glide down into the recto-vaginal pouch. As the tumor increases in that recess it pushes before it the posterior wall of the vagina, often obstructing this passage so as to render difficult the introduction of the finger, and still more so the introduction of the male organ. When thus situated the tumor presses on the neck of the bladder by its anterior surface, and on the rectum by its inferior, while it often draws down the fundus uteri with it, so as to produce more or less complete retroversion, which has been noticed by Siebold, Boivin, Seymour, and Burns. It may even raise it above the pubes, and by its continued pressure cause it to become atrophied. Sometimes, however, the tumor insinuates itself into the vesico-vaginal pouch, and in time gives rise to ante version of the womb. It will be easily

understood what peculiarities of its mode of attachment may cause the lateral flexions of the womb which have been sometimes noticed.

But the tumor may be so placed as to press upon the fundus uteri, forcing it down as it grows, and thus determining prolapsus or even proclivencia. This is of rare occurrence, but such cases have been noticed by Meissner, Dugast; and in Dr. Mogerikouski's successful case of ovariectomy it is said that the uterus was forced out of the vagina by the cyst. The patient had two children after the operation.

It is impossible for these displacements to occur without considerably modifying the position and dimensions of the vagina; and we find that it is lengthened, and often drawn to one side, when there is but one tumor, as it has been observed by Montaulieu *fil.*, Laporte (Tom ii. *Mémoire de l'Acad. de Chirurg.*), Voisin (*loc. cit.*); and myself.

In a case cited by Duges, the tumor so pressed upon the vagina as to determine the inflammatory adhesion of its internal surface, and consequent obliteration (?). Of course, this latter accident, and even the uterine displacements we have enumerated, interfere sufficiently with the functions of generation so as in many cases to produce sterility.

When, however, the tumor has so increased as to be ill at ease in the restricted proportions of the pelvis, it imitates the womb, which after the first few months of pregnancy rises above the brim of the pelvis, draws up the womb and therefore the vagina, to which it gives a slanting direction, which is recognisable by the oblique and elevated position of the os tincæ. In such cases the vagina often takes upon itself an infundibuliform appearance. Burns thinks that the vagina is more particularly drawn up and lengthened when both ovaries are affected. Thus it has occurred that the uterus is drawn completely out of the pelvis by an ovarian tumor, the neck of the womb forming a narrow canal of from four to six inches long. In the case observed by Cleghorn the womb was drawn up to the navel.

Dupuytren has several times met with the womb drawn up, so as to run the risk of being pierced by the trocar during the operation for paracentesis performed to remove the contents of an

ovarian cyst; an accident, Voisin relates, as having really occurred to a surgeon of his acquaintance.

One of the most singular cases of lengthening of the womb is quoted by Morgagni (Epis. 45, 12) from Vater, who relates the fact of the neck of the womb being stretched in contrary directions by an enormous swelling of the ovary, and by *prolapsus vaginæ* (?) and this leads us to remark, that the ovarian tumor often rises above the brim of the pelvis without leaving the pelvic cavity, to the walls of which it has become strongly adherent. The solid tumors are particularly liable to become permanently established there, displacing the pelvic viscera, firmly fixing the womb, and even incorporating it in their own substance.

If both ovaries are implicated, inasmuch as they are generally affected successively, and one is less enlarged than the other, the smaller one remains in the pelvis, and its retention is proportionate to the obstacles offered to its ascent by its fellow. It is wedged in between the uterus and rectum, even if there are no adhesions. If we find the above-mentioned irregularity in the uterus and the vagina, and at the same time discover an immovable tumor in the pelvis, which weighs upon the posterior walls of the vagina, and pushes it together with the uterus forwards, it may be assumed, if there are no countervailing symptoms, that both ovaries are diseased.

As the tumors increase, and their contents often have an outward tendency, inflammation may arise between the cyst and some part of the parturient canal. The tumor most frequently voids its contents through the vagina; and we subjoin such instances as the reader will not find in the literature of our own country.

Bluff (Jour. de l'Expérience) relates the case of a woman who by degrees ceased to menstruate, while in the meantime the left ovary, by gradually increasing, had at last attained the size of a man's head, and pressed strongly on the vagina, the womb and right ovary remaining unimpaired. Hectic fever came on, in consequence of which an opening was formed on the left side of the vagina, which had become very painful. The contents of the cyst passed through this opening, and the tumor disappeared. The opening being closed,

menstruation became regular, and the patient's health was restored.

Helman (Siebold's Jour. vol. ii.) relates the occurrence of this termination after an effort to lift a heavy weight: probably inflammation had previously softened the tissues, which burst asunder from intense muscular exertion. Wertenkampff also saw the left ovary attain the size of a man's head, and empty itself through the vagina; the patient was cured, and menstruation returned. Munro observed a similar substance; but the dropsical effusion returned, and terminated by carrying off the patient. Mme. Boivin saw two similar cases; the one was cured, and in the other the disease returned. Hey (Prussian Med. Journ.) was on the point of tapping a woman, when water gushed from the vagina, and flowed for four days: the patient recovered. Burdach treated a woman who, on lifting a heavy weight, burst an ovarian tumor, the fluid of which was evacuated by the vagina. A year and a half after, the dropsy had not returned (Hufeland's Journ. of Prac. Medicine, June 1833). A similar case is published by Forcke (Hanover Annals, 1838). In some cases the liquid seems to have sought for itself a more complicated, although a natural passage, by passing through the Fallopian tubes, the uterus, and the vagina. Blasius (Commentatio de Hydrope ovariorum profuente, Halle, 1834) has collected several cases wherein the contents of the cyst were emptied by the Fallopian tubes into the womb.

Schmucker also says, that in one case, after the sixteenth tapping, the ovary became suddenly extremely painful in one night, and was followed by the discharge of a large quantity of very offensive ichorous matter through the womb, which discharge continued for some days, and then ceased.

Frank (De Cur. Ret.) gives a case where serous matter was discharged until death, by the uterus and vagina, to the extent of one pint per day. The patient died of consumption, and thirty-one pints of fluid were found in the left Fallopian tube. Sir Astley Cooper mentions, also, a case of this description.

And, lastly, Dr. David D. Davis used to narrate a case in his lectures where the discharge of the fluid was brought on through this channel in a country-woman by the jolting of her horse as she rode to market.

CONSIDERATIONS ON THE CLIMATE OF ITALY,

WITH REFERENCE TO THE TREATMENT OF CHRONIC DISEASE.

BY JAMES EDWARD POLLOCK, M.D.,
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THE effects which a change of climate is capable of producing on chronic diseases must ever be a matter of great interest to a community like ours, where the delicacy of frame engendered by the pursuit of wealth, or the luxuries which it supplies when attained, are so often and so conveniently sought to be remedied by the complete alteration in climate, habits, and scenery, which is popularly called "change of air." Thus the expediency of obtaining a greater degree of accuracy in the data from which the practical physician is now so frequently called on to form a judgment on the propriety of removal to a southern climate in certain cases of disease, threatened or existing, must have often occurred to those who, from their position, are regarded by the public as the best authorities on such matters. Of the numerous class of English travellers who are met with in almost all countries at the present day, a very large number have obtained the authority of some physician of note at home as a warrant for the efficacy of their experiment in search of health. In our present artificial state of society, where wealth, and the struggles for its attainment, are the too common causes of derangement of health, and the frequent origin of organic disease, the same power fortunately brings with it its antidote, and supplies the means of obtaining a remedy which removes the patient from the influence of a host of exciting causes, and often relieves the physician from the painful task of prescribing remedies whose power is daily counteracted by the pernicious habits of the sufferer, for which business and the requirements of station and society form so ready an excuse. On the other hand, an examination of any average number of these valetudinarians, when arrived at their destined residence abroad, reveals, as might be expected, a vast variety of ailments, and the usual modifications of disease by the accidents of

temperament and constitution, as well as many individuals whose frames will pass a critical examination without offering any symptoms but those of an overworked brain, or the signs of the life-battle in the senate or at the bar, in the pulpit or in medicine; for the characters of which the philosophic physician will seek first not in the tongue, pulse, and organic structures, but in the careworn face of his patient, and in the recognition of which he will be more assisted by Lavater than by any treatise on medicine or doctrines of the schools. But, deducting from the crowd of cases, real or imaginary, all those to whom the mere abandonment of home and its cares is a remedy of power, there will always remain a number of real affections, which the foreign physician will with surprise find grouped in the same locality. A large proportion will be found to be in some stage of phthisis, or in that precarious condition of health which so often precedes the deposit of tubercle in the lung, where a depraved state of nutrition and a lowered condition of the vital powers seems to prepare the ground, as it were, for the seeds of an intractable disease. These cases perhaps more than any other are modified by the temperament and idiosyncrasy of the threatened victim. And we might in many cases forget the kind of course which the disease, when developed, will pursue by noting with care the state of circulation, the facility and degree of perfection of sanguification, and the strength and vitality of the tissues. When declared disease exists we need scarcely say that its varieties and modifications by age, hereditary predisposition, sex, and temperament, are almost endless, and require on the very face of them a various form of remedy, whether that remedy be the mild and sedative in diet, drugs, or air, or the stimulant and bracing furnished from the same sources.

The object of the present remark is merely to call attention to the important fact of great differences existing in the aspects of the disease in question, which should in all cases be taken advantage of by the scientific physician,—differences which so alter the main features of affections bearing the same name, and resulting in similar pathological changes, that, were it not for consistency in classification (a rock on which we so often split), we should be compelled to assign a different nomenclature, as we are forced

to meet them with a widely varying mode of treatment.

To prove that the remedy in question—a removal to a southern climate—is nearly as varied as the affections for which it is recommended, we have only to consult the accumulated observations of scientific visitors and others to the numerous places of resort in France and Italy, which have obtained some character for the curative powers of their climate. That these differences are real, and not imaginary sensations of too nicely observant invalids; any one who has travelled from Paris to Naples by land can testify; especially if he has been unfortunate enough to encounter a "Mistral" at Montpellier, or Hyères in Provence; the "Bis" at Nice, a blast up the Valley of the Arno in Florence, or a "Scirocco" at Rome or Naples. Nay, more: let the everyday air of Nice and Rome be contrasted by any one in health, and the different effects of what are called the "stimulant" and "sedative" will be very perceptible.

Now that our facilities of travelling have been so increased, and information on foreign localities become so general, the English physician declines sending his consumptive patients to encounter the keen breezes of Montpellier in France, or of Geneva from the Alps, and is also well aware of the differences between Rome and Nice; yet many English inscriptions in these places bring painful memories of a time when the medical philanthropist will often bitterly regret when the long journey and the abandonment of home and friends—those alleviations of the last stages of chronic weakness and the foreign grave, how much dreaded by the many!—might have been spared the wretched sufferer. But on these local differences in characters of climate it is not necessary here to dwell much. Let us establish distinctly the two facts, that among those cases for which the physician at home would often recommend removal to a southern climate, there exists an infinite variety, the individual peculiarities of which he would gladly meet by some corresponding characters of climate; and that such climates as the South of Europe offer at least some important differences of which we may take advantage. If we can satisfy ourselves that a remedial action is in any case manifested, we desire to know with the greatest possible accuracy how to

select both the nature and stage of the affection, so that our curative means shall correspond with the indications furnished by a careful observation of our patient, which will, in fact, be only to follow, in the application of climate as a remedy, the same rules which daily guide us in therapeutics; and if, by a like observation of the localities resorted to, we can satisfy ourselves that in any case a positive *nischief* may result from such a removal, we should shun its employment as we would that of depletion, or other lowering treatment, in an already exhausted system.

In elucidation of these points it is a question of no little moment, and one pressed upon us by the light which modern research has thrown on the subject of vital actions, as well as by ethnological investigations, to what extent a continued residence in a climate new to the constitution can alter or modify the *healthy* system. If in our study of the question we regard attentively the physical and moral features exhibited by natives of the soil, we are struck at once by the fact that mere differences of "race" cannot wholly account for the varied physical condition of the inhabitants of countries geographically separated. The effect of governmental institutions bearing on the education, supply of home comforts, and the entire social condition of a people, can do much, even in one or two generations, towards conferring a peculiar recognizable stamp on all classes of a population. Yet there are plainly constitutional varieties in whole nations or districts for which no government or misgovernment can be entirely responsible, and which are attributable to other than even these important elements of physical elevation or depression. From whence arises the hardy enterprise and intrepidity of the Swiss,—the languor approaching to solemnity of the Roman, his impassive, almost sullen aspect, to a northern eye,—or the unceasing yet trifling life-energy of the unresting Neapolitan? The two latter are equally badly governed, equally ignorant and oppressed; but the air which blows across the Bay of Naples is not the sullen vapour which is wafted from surrounding stagnation into the city of the plain. How marked is the progressive increase of energy as the traveller proceeds from Italy to France and Germany, till he reaches that great mixture of the energetic and phlegmatic

temperament found united so happily in his own islands. The authority of travellers of intelligence has no doubt attributed these varieties to "race;" yet climate forms so essential an ingredient in race, that very much must be attributed to its influence, and the writer has been led from observation to believe that all the effects on disease traceable to a southern climate are shown on the healthy northern system in degrees varying with the sensibility of the individual to such external influences.

A great deal has been already written on the subject, and few places of note on the Continent are unfurnished with a treatise by a local English physician, whose dictum is generally a law to his freshly arrived countrymen. It must be confessed that such treatises are generally very worthless to the practitioner at home, being more commonly addressed to the public than to the profession, containing often that species of "popular medicine" for which our countrymen have at this moment so great a mania, and, by indiscriminate praise of the climate or the springs, as the case may be, have not tended to raise us above the level of the vast mass of pretenders to curative powers who are at war with the less attractive, but more calm, observations of science on the cure of disease. To these there exist brilliant exceptions in the works of such men as Sir J. Clark and others, whose remarks are founded on personal observation, and contain impartial comments on climate. Of late, also, we have been furnished with correct tables of temperature and its ranges, of the hygrometrical condition of the atmosphere, of the prevalence of winds, of the influence of geographical aspects of countries, and of various other physical observations which have most important bearings on the frame, both in health and disease; and, without doubt, much more remains to be done in perfecting these records. On these points we do not intend to dwell, but rather to present a view of more practical facts; for, after all, we do not (we should not) select a particular climate for an invalid from mere considerations of temperature, &c. The latter may guide us, and should always have their due weight; but we have long been persuaded that information gathered *only* from these sources is liable to mislead, and has misled, the profession at home. It is

by viewing the various peculiarities of a locality *as a whole* that we shall form a correct judgment beforehand of its eligibility, as it is by watching the after history of its actual effects that we can alone truly test its results on the individuals placed under its influence. In such a view the action of the imponderable agents—heat, light, and electricity,—the variations of atmospheric currents—the presence and degree of moisture, &c.—are to be regarded in connection with other considerations, such as the possible admixture of marsh poisons with the air, and the general influence, moral and physical, of all these agencies together, as evidenced in the health and natural temperaments, as well as in the diseases of the native inhabitants; and to this must be conjoined a study of the climate left, to which the traveller has been, as it were, indigenous, and from which he derives himself many striking peculiarities. We shall thus regard him much as a scientific gardener views the plant of other climes, whose former physical relations he studies with care in regard to its present treatment, which he even makes comparative to them.

Supposing, then, that we are familiar with the general physical facts regarding temperature, winds, electrical and hygrometrical phenomena, which may be obtained from many published tables, the writer proposes to record his observations on the effects of a southern climate on the healthy and diseased system, and thus to contribute something to the after history of cases of which the home physician often loses sight, as well as to elucidate, if possible, the physiological aspect of the questions which arise in connection with the removal of invalids to a foreign country.

It is right to state that the observations are the result of seven years' continuous residence in Italy, each winter being spent in Rome, and that the writer has enjoyed fair opportunities, from private practice, of judging of the effects of the climate of that country.

It will be convenient, before entering on any details of the effects of climate, to consider briefly—first, the class of cases which are most generally so treated, and, secondly, ~~what~~ we propose to ourselves by the ^{present} patient. If we are question general characters of the ^{present} patient.

abroad, and the average success which attends the experiment, we shall be led to the conclusion, that two kinds of sufferers are most exported, and most frequently benefit by the removal. First, those *threatened* with organic disease, principally of the chest; and, secondly, those who, having already undergone some morbid change of structure, have exhibited a degree of power at least of rallying under the injury, nature making an effort to support the general strength, which the physician would gladly and wisely assist by every means in his power. The first is the antecedent to, the latter the confirmed stage of, what we understand commonly as chronic disease. In our present consideration we include the curative attempt of nature as an essential part of the definition of chronic disease, a pathological point of vast importance, and our best guide to treatment of an effective kind, leading its student to an investigation of when and how the reparative attempt is being made, and pointing him to the inquiry, how far he can assist, by extraneous treatment, in the curative effort of the system.

If we analyse the variety of remedies advised in chronic disease by those most called on to prescribe for, and most familiar with such cases, we shall find that an improvement of nutrition, a strengthening of the reparative power by an increased supply of an improved quality of blood, is what is aimed at, and that to this tends the multifarious means so often resorted to; and that, whether attempted by the highly-lauded horse exercise of Sydenham, or by a course of tonics, by any of the innumerable class of so-called "alterative" medicines, or by removal to better air, and improvement in diet, the same object is in view—a bettering of the material by which nature is attempting to elaborate or repair, or wall round with a barrier of low vitality, the injury which, in a weaker hour, she has undergone. In selecting from such a numerous class of remedial agents, the student of the present day will, by observation, be gradually led more and more to abandon a vain search after specifics, and, if he have gathered any hint from the spirit which emanates from those best and highest in the profession, will endeavour to spare his patient many a dose, but rather, by strengthening organs which are plainly awaking to

energy, than by stimulating actions which it is doubtful if he ever can command by drugs, lend a hand to the restoration which nature is attempting. Along with this it is important to remember that slowness, whether to destroy or to repair, is a constant character of chronic disease, and that to depart from this in our efforts is to impede rather than to assist, and has too often converted a natural tardy process into one rapidly destructive to the powers of life itself. If we inquire how the scientific physician has been led to adopt the above idea of treatment, we shall find that it has been founded on the observation of the fact, that in chronic disease a deprivation of the process of nutrition has invariably preceded the lesion of the affected organ, and we can thus perceive with increased force the value of the efforts which would strike at the very root of the affection, in preference to an attempt to obviate some of its more prominent results. It also enables us to see that the two classes into which we have divided the cases usually selected for a change of air have in reality one important feature in common, and mainly important as regards their treatment—that both the cases of threatened disease, and those of resisted established disease, are accompanied by some change in nutrition in the largest meaning of the word, which forms at once the point of departure from a state of health, and furnishes the curative indication, being the obvious field for the exercise of all, or nearly all, the treatment which can be efficacious. With these views it will be a question of first importance what influence climate and particular climates can exert in effecting any curative change on the nutrition? and an inquiry clearly secondary to this, but still of interest, will be made into its palliative effects on symptoms.

To elucidate these points, we propose to examine briefly what are the characters of the climate of the South of Europe, and what its effects on the healthy system as manifested in the temperament of its inhabitants, as well in health as in disease, and, further, to trace the parallel operation of such influences on the healthy and diseased English system; and, shortly, to record a summary of our observations on the varied cases of disease which have fallen under our notice.

ILLUSTRATIONS OF PANCREATIC DISEASE.

By H. FEARNSIDE, M.B. LOND.
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THE cases detailed below are chiefly important in a pathological point of view. The first is remarkable for the rarity of the disease of which the patient was the subject; Storck being the only observer hitherto, so far as I am aware, to whom an example of pancreatic hæmorrhage has occurred. The second case is interesting, not only from its bearing upon disease of the pancreas, but also as an instance of a form of heart disease which has lately attracted considerable attention.

Disease of the pancreas has been considered to be less frequent than that of any other large gland in the body; but the correctness of this opinion is somewhat questionable.

The importance of the pancreas in the animal economy is seen in the all but universality of its presence in the vertebrated classes, and its existence even in some invertebrata. Its situation in the body, and the point at which its duct enters the intestine, indicate that its function is to assist in the digestive process; but respecting the precise nature of the office which its secretion serves, much obscurity has prevailed. Even the statements made by different observers, as to the character and composition of the pancreatic fluid, have been contradictory. Some of these discrepancies are accounted for by the fact that, of all the animal fluids, it appears to undergo change most readily; and, in experiments upon animals, its quantity is often increased, and its composition altogether altered, by the inflammation consequent upon the operation.

The results of a more complete and extended series of investigations upon this subject have recently been published by Dr. Charles Bernard,* from which it seems that the secretion is a colourless, limpid, saline, and somewhat viscous fluid, invariably alkaline in reaction, and coagulable by heat and acids; but the material to which it owes this property differs in certain respects from albumen. M. Bernard's researches

tend to prove that the special office of the pancreatic fluid is to effect the digestion of fatty bodies. Until their publication the doctrine generally taught by physiologists was that held (with certain modifications) by Leuret and Lassaigue, Tiedemann and Gmelin—that the bile dissolves the fatty part of the food, or at any rate favours its absorption, by contributing to its minute division. M. Bernard states that, on the admixture of fatty matter with pancreatic fluid, it is resolved into glycerine, margaric, stearic, and oleic acids; whereas saliva, gastric juice, and bile, produce no such effects. This last assertion, however, is denied by Frerichs, who merely admits that pancreatic fluid will form an emulsion with oil.*

As an acquaintance with the functions of organs in their normal state must ever precede the useful study of their deviations from the standard of health, it is to be hoped that these observations may be susceptible of beneficial application to this obscure department of pathology.

Gradual failure of the health for twelve months, with symptoms of deranged digestion; increased intensity of these indications during the last three months of life; paroxysms of acute suffering in the region of the stomach; sudden accession of excruciating pain in the same situation; syncope, vomiting, hiccough, great prostration, and death.
—Great enlargement of the pancreas; signs of inflammation in the body and left extremity of the organ; hæmorrhage into the substance of the right extremity; great dilatation of the stomach.

R. H.—, aged 49 years, a man of middle stature and average conformation, was moribund when seen by me for the first and only time in August 1849. His employment for many years had been that of a farm labourer, and his habits not remarkable for temperance; but he had generally enjoyed good health until within the preceding year. During the period mentioned he had frequently suffered from deep-seated pain in the region of the stomach—occasional sensations of coldness—others, of heat in the same situation—gastrorrhœa, pyrosis, constipation: and

* *Archiv. Génér. de Médecine*, Jan. 1849; vide also *British and Foreign Medico-Chirurgical Review*, April 1849.

* *Wagner's Handwörterbuch der Physiologie*, quoted in *Monthly Journal of Medical Science*, February 1850.

he had never had vomiting; he had lost flesh and colour in some, but not a very striking degree. For the preceding three months the paroxysms of pain had been more severe in character, and more frequent in recurrence, until at length they became almost constant; but they were generally most distressing a few hours after taking food. For some weeks his appetite had failed, and he had complained of chilliness, and pain in the limbs, shoulders, and back, which he ascribed to his having taken cold, as he still remained at his work.

Three days previously to my visit he had been suddenly seized with excruciating pain in the epigastrium: this was followed by faintness and vomiting, and the fluid ejected was described as being of a dark brown colour.

On the following day he became jaundiced; the vomiting continued; the pain abated somewhat; but he began to be harassed by frequent hiccough.

When I saw him his pulse was very small and rapid; his hands and feet were cold, his intelligence perfect; he complained of severe pain in the upper part of the abdomen, which was obviously fuller than natural. On pressure between the ensiform cartilage and the umbilicus, a deeply-seated tumor could be felt, extending chiefly in a transverse direction; the liver did not appear to descend below the margin of the ribs in the right hypochondrium; he had frequent hiccough, which often gave rise to an attempt at vomiting. There was extreme restlessness and jactitation, and he died in a few hours afterwards.

Examination of the body 18 hours after death.—There was some, but not a remarkable degree of emaciation, there being still present a small quantity of fat in the walls of the abdomen. When the abdominal cavity was exposed, the stomach, much enlarged, was seen extending across its upper part, and reaching much farther to the right than natural. On drawing aside the stomach, the tumor which had been felt during life was seen to be formed by the pancreas, increased to at least four times its normal size; some adhesions of a rather firm character existed between it and the posterior surface of the stomach; and the peritoneum in the neighbourhood, both diaphragmatic and parietal, had in several spots lost its transparency, and was thickened by the

deposit upon its surface of exudation-matter, some of which was soft, flocculent, and easily detached, but the greater part was firm and adherent. Implicating the whole of the organ in a certain degree, the increase of size was most striking in its right extremity, which passed deeply into the right lumbar region, carrying with it the duodenum. This portion, of a blackish brown colour, was almost entirely converted into a pulaceous, grumous mass, whose slight coherence was such that it gave way in the attempt to remove it from the body. In the more consistent parts distinct sanguineous coagula were met with, and every trace of the natural tissue of the gland had disappeared. Both the body and left extremity were also profoundly changed in structure: the body was of a dark red colour, mottled with black lines and points; it was very firm and close in texture; and, on a thin slice being broken across, the fracture was short and crisp. In approaching the left extremity, the colour underwent a gradual change, becoming lighter, and the part last mentioned was of a pale yellow hue; it was also close, firm, and uniform in structure, although less so than the body of the organ. Both these portions were strongly adherent to the posterior abdominal walls.

The liver was small, firm, and rather yellow than brown in colour; the biliary ducts contained much bile; the gall-bladder also contained a large quantity of dark bile.

The kidneys were small, contracted, and the cortical tissue less distinct than usual.

The rest of the viscera were healthy.

Progressive loss of strength, flesh, and colour, for two years; symptoms of imperfect digestion and sanguification; development of extreme anaemia; signs of cardiac atrophy; gradual sinking, and death by exhaustion.—Fatty infiltration of pancreas; pulpy softening of spleen; fatty degeneration of the walls of the left ventricle of the heart.

J. H.—, aged 48 years, a rather tall man, of light complexion, and moderate conformation, came under treatment in the autumn of 1849. His occupation was that of a millwright; his habits of late years temperate, but early in life the reverse. Both his parents died at an advanced age. His health had been generally good until two years pre-

viously: about this time his circumstances became less prosperous than they had formerly been, and he was the subject of considerable anxiety of mind. Since the period mentioned he had suffered occasionally from a sense of uneasiness in the epigastrium, at times amounting to actual pain, usually increased after taking food, sometimes immediately, but more frequently after the lapse of two or three hours; occasional vomiting, especially in the morning; profuse flow of saliva; gastrorrhœa, pyrosis, flatulence, loss of appetite, flesh, and colour.

These symptoms had been gradually increasing in severity and constancy; and, during the last twelve months, he had several times been compelled in consequence to discontinue his employment for a week or fortnight, at once; and, for the three months preceding the time of my seeing him, he had been altogether unable to work. When he came under my notice he was much reduced by long suffering: his complexion was remarkably pale; his lips and gums bloodless; his feet and legs œdematous: he complained of extreme debility, and of the existence of a "lingering gnawing pain" in the region of the stomach, not constant, but often brought on by taking food, and especially felt some hours afterwards: he had no vomiting, and had only once been sick during the preceding two months: his tongue was pale and clean; his appetite variable, but not often below its average in health; his bowels were irregular, sometimes confined, but more generally relaxed, and the evacuations often dark in colour, but at other times light and frothy. For some weeks he had experienced almost constant palpitation in the epigastrium, and during the same time he had been distressed by a continual humming or buzzing in the ears: he had also suffered much from frontal headache. On any little exertion his breathing became hurried and embarrassed; he often complained of a sense of oppression about the chest, and sighed frequently, and on several occasions he had shown a tendency to syncope: his skin was cool and dry; his pulse was small, weak, sometimes irregular, and about 80 per minute. On physical examination of his chest, the lungs presented no signs of disease; the impulse of the heart was feeble; and, on percussion, the dull sound eli-

cited over the heart was below its normal extent; a faint blowing murmur accompanied the first sound, and was heard over both the apex and base of the organ, but most distinctly in the latter situation: there was also a want of clearness about the second sound: a very loud, continuous murmur was heard over the veins of the neck.

There was some tenderness on pressure over the epigastrium, and in a lower degree over the whole abdomen, but no well-defined tumor could be distinguished; the liver did not descend much below the margin of the ribs; there was considerable throbbing of the abdominal aorta; and, on auscultation over the præcordia, a distinct bellows murmur was heard with each systole of the heart. The urine was high-coloured, and deposited a copious sediment of lithate of ammonia.

His speech was hesitating, as if he had difficulty in recalling appropriate words, and he complained of dreaming much at night, especially about old days and former scenes.

Progress of the Case.—For a short time he seemed to derive benefit from the use of steel and a general tonic and supporting regimen: but these means afforded him only temporary relief: he gradually sunk, and died in about a month from the time when he was first seen.

Examination of the body, thirty hours after death:—

There was very considerable emaciation, and great and universal pallidity of the body.

The head was not examined.

Thorax.—There were about four ounces of serum in each pleural sac; the lungs were voluminous, somewhat congested inferiorly and posteriorly, but healthy in every other respect. There was a small quantity of fluid in the pericardium; the heart was small, weighing only 7 oz., it was pale in colour, soft and flabby; the lining membrane of both auricles, ventricles, and aorta, were stained of a deep red colour; there were a number of small red granulations, of the size of millet seeds, along the margin of the mitral valve, both laminae of which were of a deep purple hue; there was no thickening nor loss of transparency in the endocardium; the aortic valves were healthy; the walls of the heart in several situations, but the columna carnea of the left ventricle

in particular, presented numerous pale buff-coloured spots, portions of which, on examination under the microscope, were seen to consist almost entirely of fat, and many of the fibres in the adjoining parts presented a granular appearance, and were devoid of the cross striæ. Beneath the lining membrane of the aorta, in several situations, were patches of pale yellow atheromatous matter.

Abdomen.—The intestines generally were dark-coloured externally. The stomach was small; its mucous membrane exceeding small, but not otherwise unhealthy; that of the duodenum, however, especially in the neighbourhood of the stomach, was raised up in points of the size of pins' heads by enlargement of its glandular structure, and in the inferior transverse portion of the duodenum and commencement of the jejunum the membrane was of a dark red colour, and very soft in consistence. The liver was rather small than otherwise, firm, and of a pale brown colour.

The pancreas was rather above the usual size; it was pale in colour, firm in texture, and had undergone a great alteration in structure: on a section it presented a nearly uniform pale yellow surface, greasing the scalpel in a marked degree: scarcely any of the true lobular tissue of the organ could be discerned, the whole being infiltrated with fat.

The spleen was of the usual size, but exceedingly soft, being little more than a capsule containing semi-fluid blood.

The kidneys were small, pale, and loaded with fat internally. The mucous membrane of the colon and rectum was of a dark grey colour.

REMARKS.—As in both of the cases just narrated disease of the pancreas must have been long in progress, it may be useful to indicate what common and characteristic symptoms were afforded by them.

The earliest phenomena noticed were gradual failure of the general health, and faulty digestion; but these symptoms being present in a variety of diseases are of little diagnostic value. *Deep-seated pain in the epigastrium* occurred in both patients, and in both is said to have become aggravated a few hours after taking food. But the importance of this remark is diminished by the signs found after death in one of the cases, of the previous existence of inflammation

of the mucous membrane of the duodenum. *Gastrorrhæa* was present in both cases. Some have supposed that the fluid ejected in this affection is a vitiated secretion of the pancreas; but its existence concurrently with such extensive disease of that organ as was found after death, and must have been going on long before that event, when little or no secretion could be eliminated, is adverse to that opinion, and would rather lead us to conclude that it is simply a flux, the result of hyperæmia of the mucous membrane of the stomach, and analogous to the catarrhs of other mucous membranes.

Ptyalism was made the subject of spontaneous complaint by one of the patients. This is a symptom which has been repeatedly observed in cases of disease of the pancreas, and to which considerable importance has been attached, from some conjectured antagonism or "balancement" between the pancreas and the salivary glands.* In the same case in which the last symptom presented itself, the tongue was invariably *pale and clean*, and the freedom of this organ from morbid coating in cases similar to those under review has been made the subject of former remark. *The appetite* was variable; in the case longest under observation, the quantity of food consumed was generally not below the average of health.

The last case also presented a sign upon which some stress has been laid by Professor Siebert, of Jena,† as evidence of pancreatic disease, when co-existing with other indications,—viz., *abdominal pulsation*. In one of the cases the *action of the bowels* was insufficient, as is stated to be the general rule in these diseases; in the other it was irregular; but in neither case, so far as my observation extended, or I could learn from others, was fatty matter ever present in the evacuations, as noticed by Dr. Bright in certain cases of diseased pancreas.‡ In the case last narrated, the urine was repeatedly examined, both chemically and microscopically, but except in being generally scanty, and abounding in lithate of

* M. Mondiere, in *Archiv. Génér. de Médecine*, t. xii. p. 153; also Dr. Battersby, *Dublin Quarterly Journal of Medical Science*, vol. xxiv.

† *Monthly Journal of Medical Science*, Sept. 1848.

‡ *Pathological and Practical Researches on Diseases of the Stomach, &c.* 3d Edition, p. 387.

ammonia, it did not deviate from the natural state. *Emaciation* and *anæmia*, present in both patients—in one strikingly,—are generally seen in protracted pancreatic disease: in the one respects, as in some others, the last-detailed history shows a resemblance, almost amounting to identity, in symptoms with a case related by Dr. Abercrombie.* In both, the blood-making functions were greatly impaired, as a consequence of which, and loss of tone in the blood-vessels, there occurred throbbing in the arteries of the head and neck, humming in the ears, and abdominal palpitation. In but one of the cases could any humour, or fulness of the epigastrium, be recognised during life.

It may be remarked incidentally, that the thoracic symptoms present in the last case were such as have been frequently observed in fatty degeneration of the heart. The dyspnoea, oppression about the chest, tendency to syncope, feeble impulse of the heart, and small irregular pulse, all give evidence of a weakened condition of the central organ of the circulation. The altered character of the sounds of the heart during life was ascribed to the existing anæmia.

2. From a review of the symptoms met with in these cases, it may be concluded that although none can be stated to be absolutely pathognomonic of disease of the pancreas, yet the union of several affords a very high probability of the existence of a departure from the healthy condition of that organ. In attempting to arrive at a more minute diagnosis, the *general* symptoms and history, with any information derived from a physical examination of the patient, will prove our chief guides. In the case first related the individual had long suffered in a mode which might have led to the opinion that disease of the pancreas existed; then a considerable tumor was found in the situation of that gland: this might have been occasioned by tubercle, cancer, or simple inflammation: the age of the patient, and especially the freedom from pulmonary symptoms, discountenanced the first supposition. More difficulty, however, existed in deciding between the two last, and the absence of any cancerous hue of complexion merely warranted the expression of a probable diagnosis.

The sudden accession of acute pain, syncope, and vomiting, followed by jaundice, indicated the occurrence of some change, producing pressure upon the gall-ducts, and, from the constitutional sympathy, of a probably disruptive character, but the rarity of pancreatic hæmorrhage prevented the idea of its presence being entertained. In the second case, the group of symptoms, leading us to infer the presence of disease of the pancreas, was more complete than in the first patient. Respecting the nature of the affection it might have been allowable to hazard an opinion from the probable existence of fatty degeneration of the heart.

3. In the case first related the pancreas exhibited on post-mortem examination a very striking deviation from the natural condition of the organ. The increase in the volume and density of the body and left extremity was due to a new deposit of matter within its tissue; not in masses, as when this consists of tubercle or cancer, but uniformly diffused throughout its structure. Presenting also none of the other characters of those adventitious growths, it could be referred only to the effusion of exudation matter, the results of inflammation, to the existence of which the state of the peritoneum, and the adhesions to the adjoining parts, abundantly testified. The appearance of the central portion of the gland closely resembled that seen in a case recorded by Mr. Lawrence,* as one of inflammation; in both the pancreas was of a deep, dull-red colour, very firm, and crisp on incision. In the left extremity the disease seemed to have been of more ancient date, and afforded many of the recognised characters of old-standing inflammation. "When the pancreas has been the seat of chronic inflammation, it is said to acquire a great increase in the density of its tissue, which swells, becomes more dry than natural, and of a reddish and whitish-yellow colour."† The head of the pancreas was completely disorganised, and occupied by a mass of grunous blood. Viewed in connection with the state of the other parts of the gland, there can be no hesitation in regarding the hæmorrhage as the result

* Medico-Chir. Trans. vol. xvi.

† Dr. W. Thomson, Library of Medicine, vol. iv. p. 201.

* Med.-Chir. Trans. vol. xviii.

of softening and ulceration produced by inflammation.

In the second case, the whole substance of the gland was infiltrated with fat, a condition which must be distinguished from mere accumulation of fat between the lobules of the organ.* The results of the same vitiated nutrition were also apparent in the fibres of the heart, which presented both the naked-eye, and microscopical characters of fatty degeneration in a marked degree.

4. Respecting the etiology of these forms of disease, nothing very definite or satisfactory can be stated. Both patients were predisposed to their occurrence, by former intemperance in the use of alcoholic liquors; and from the nature of their occupations, one was much exposed to the weather, the other to sudden and frequent changes of temperature. In the subject of the last case, nutrition had suffered from the employment for some time, of an impoverished diet, and the existence of considerable anxiety of mind. But when we attempt to trace further the operation of these causes, granting them sufficient, our light fails us, from our imperfect acquaintance with the nutritive process.

5. It has been supposed that the obscure disease, diabetes, has an intimate connection with imperfect primary, especially duodenal, digestion; and Bonchardat has endeavoured to associate it with disease of the pancreas, and consequent depravation or insufficiency of pancreatic secretion. The cases related above, however, afford no countenance to that opinion. Although, for some time before death, the gland must have been functionally inactive, yet in the case longest under observation the urine showed no change from its natural condition, except an occasionally being scanty, concentrated, and depositing urate of ammonia.

THE DIVISIBILITY OF MATTER.

MANY years ago a curious calculation was made by Dr. Thomson, to show to what degree matter could be divided and be still sensible to the eye. He dissolved a grain of nitrate of lead in 500,000 grains

of water, and passed through the solution a current of sulphuretted hydrogen, when the whole liquid became sensibly discoloured. Now a grain of water may be regarded as being about equal to a drop of that liquid, and a drop may be easily spread out so as to cover a square inch of surface. But, under an ordinary microscope, the millionth of a square inch may be distinguished by the eye. The water, therefore, could be divided into 600,000,000,000 parts. But the lead in a grain of nitrate of lead weighs 0.62 grains; an atom of lead cannot weigh more than 1,310,000,000,000th of a grain; while the atom of sulphur, which in combination with the lead rendered it visible (in the mass?), could not weigh more than 1.2, 013,000,000—that is, the two billionth part of a grain.

But what is a billion, or, rather, what conception can we form of such a quantity? We may say that a billion is a million of millions, and can easily represent it thus—1,000,000,000,000. But a schoolboy's calculation will show how entirely the mind is incapable of conceiving such numbers. If a person were able to count at the rate of 200 in a minute, and to work without intermission twelve hours in the day, he would take to count a billion 6,944,944 days, or 19,025 years 319 days. But this may be nothing to the division of matter. There are living creatures so minute, that a hundred millions of them may be comprehended in the space of a cubic inch. But these creatures, until they are lost to the sense of sight, aided by the most powerful instruments, are seen to possess organs fitted for collecting their food, and even capturing their prey. They are therefore supplied with organs, and these organs consist of tissues nourished by circulating fluids, which circulating fluids must consist of parts or atoms, if we please so to term them. In reckoning the size of such atoms we must speak not of billions, but perchance of billions of billions. And what is a billion of billions? The number is a quadrillion, and can be easily represented thus—1,000,000,000,000,000,000,000,000; and the same schoolboy's calculation may be employed to show, that to count a quadrillion, at the rate of 200 in the minute, would require all the inhabitants of the globe, supposing them to be a thousand millions, to count incessantly for 19,025,875 years, or for more than 3000 times the period for which the human race has been supposed to be in existence.—*Love's Inquiry into the Simple Bodies of Chemistry.*

* Lobstein, *Anatom. Pathol.*: quoted by Dr. Walshe, *Cyclop. of Anat. and Phys.* Article Adventitious Products.

MEDICAL GAZETTE.

FRIDAY, DECEMBER 6, 1850.

IN the remarks on the SEPARATE SYSTEM of imprisonment published in our last number, it was shown by the statistical records of the Pentonville Prison, that attacks of Insanity are more prevalent among criminals committed for short periods of imprisonment, than among those who are confined for long periods, whether the comparison of numbers be made with the daily population of the prison, or with the aggregate number of prisoners admitted within the walls; and also that these attacks are more frequent during the earlier than the later periods of long terms of imprisonment.

Results so unexpected as these, and drawn from facts which prove to demonstration the correctness of the opinion given by Dr. Rees and other witnesses, naturally compel us to look for an explanation. This is given by Mr. Burt, whose views we find in accordance with what has been observed of the laws of mental disease.

This gentleman was formerly Chaplain to the Hanwell Lunatic Asylum; he has therefore had excellent opportunities of observing the phenomena of the mind under disease, in addition to his being by education and by profession acquainted with its condition in health. For these reasons his opinions are of great importance in this inquiry. We shall here quote Mr. Burt's explanation of the facts which have fallen under his own observation:—

"It is one of the few known laws of mental disease," observes Mr. Burt, "that transition from one state of moral feeling to an opposite extreme are marked as critical to reason. In accordance with this law, a change of religion is attended with risk to the mind when there is any tendency to

insanity. In the case of grief the facts are notorious. Men inured to suffering will bear it without much danger to reason. It is the sudden inroad to misfortune which overwhelms and destroys the mind, or which calls forth too violent an effort of resistance. That excessive effort will be followed by a prostration of the moral energies; and derangements will sometimes supervene, or the mind be left in the power of the slightest disturbing causes, until it is rallied under new invigorating influences. And certainly it is somewhat remarkable, that of the only two cases of insanity which have occurred at Pentonville beyond the twelve months, one was produced by too sudden a return to association. At once, without having been forewarned, the prisoner found himself among one hundred others in unrestricted intercourse.

The opinion of Mr. Burt, as to the existence of danger to the mind under the separate system, and of the means whereby to counteract that danger, appear to us satisfactory, and it receives confirmation from the fact which he states, that during the last two or three years, in which period the mental affections have increased two or threefold, there has been a diminution of the sustaining moral influences generally, in very nearly the same proportion.

It is to be regretted that more direct and explicit evidence was not elicited by the Committee from Dr. Rees and the Medical Commissioners. The latter were not even examined. If these gentlemen had been submitted to as close an examination as Mr. Burt, we do not doubt that much interesting and important information of a similar tendency would have been elicited.

As it is, we learn, from the concurrent testimony of many witnesses, that the separate system is safe up to a certain term, although they differ as to the precise limit; and we have the results of the experiment carried out at Pentonville, leading to the conclusion that long periods are less injurious to mental health than short periods. It has been

shown that the whole danger to the mind is either exhausted before the first twelvemonth of confinement, or beyond that period it is neutralised by the accumulating moral influences of the earlier period. That the mental affections are not traceable to separation alone, is shown from the fact, that when separation was most rigorously carried out, the proportionate number of cases of insanity at Pentonville was not greater than in prison populations generally, and that since the rigour of separation has been relaxed the cases of insanity

have increased. The evidence of Mr. Burt shows also that the number of cases of mania bear a direct ratio to the number of new prisoners admitted, and an inverse ratio to the number of old prisoners detained in the prison. In short, the cases of mental disease have been observed to undergo a regular diminution in proportion as the term of imprisonment was prolonged, and the moral elements more strictly applied.

As a further illustration of this subject, we subjoin a few tables extracted from the Report of the Committee:—

TABLE I.—*Total Number of Cases of Mental Disease for the Four Years during which the Prison has been open, with the Period of Imprisonment at which the Attack of Insanity took place.*

MANIA.

1843.	J. R.	.	attacked between	.	5th and 6th week.
—	J. H. S.	.	"	.	9th and 10th "
—	W. C.	.	"	.	5th and 6th month.
1845.	J. G.	.	"	.	7th and 8th "
1846.	T. S.	.	"	.	13th and 14th "
1847.	J. H.	.	"	.	5th and 6th "
1848.	G. T.	.	"	.	8th and 9th "
—	J. C.	.	"	.	4th day after admission.
—	D. S.	.	"	.	2nd day.
—	C. T.	.	"	.	11th and 12th month.
—	T. L.	.	"	.	16th and 17th "
1849.	E. D.	.	"	.	4th and 5th "
—	S. S.	.	"	.	5th and 6th "
—	W. W.	.	"	.	10th and 11th "
—	E. H.	.	"	.	7th and 8th "

TABLE II.—*Summary of Cases of Mania which occurred from the Opening of the Prison to the Close of 1849, distributed according to the Period of Attack.*

1st month, attacked 2d day.	.	1
" " 4th day	.	1
Between 1st and 2nd month	.	1
" 2nd and 3rd "	.	1
" 4th and 5th "	.	1
" 5th and 6th "	.	3
" 7th and 8th "	.	2
" 8th and 9th "	.	1
" 10th and 11th "	.	1
" 11th and 12th "	.	1
" 13th and 14th "	.	1
" 16th and 17th "	.	1
Total	.	15

Total population passed through Prison to 31st December, 1849 . . . 2,769.

TABLE III.—*Summary of Cases of Delusions to the close of 1849, from the Opening of the Prison, distributed according to the Period of Attack.*

1st and 2nd month	.	.	2
3rd and 4th "	.	.	2
4th and 5th "	.	.	2
5th and 6th "	.	.	1
6th and 7th "	.	.	1
7th and 8th "	.	.	2
8th and 9th "	.	.	1
9th and 10th "	.	.	1
13th and 14th "	.	.	1
15th "	.	.	1
18th and 19th "	.	.	1
20th and 21st "	.	.	1
Total	.	.	16

The following table will exhibit the corresponding statistics for two periods; one in which the prison contained the

greatest number of prisoners for terms exceeding 12 months, and in many cases ranging to 22 and 23 months, and

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upwards, one in which it has contained the greatest number for periods not exceeding twelve months. The first period will embrace four years—namely, 1844-5-6-7. The second period will embrace three years and a half—namely, 1843, 1848-9, and the expired five months and a half of 1850. Removals on medical grounds have not been included

among casualties, to which they do not properly belong. But their relative numbers during the two periods have an important bearing upon the question, inasmuch as they contribute to reduce, in proportion to their numbers, the number of actual casualties.

The returns for those two periods will then be—

PERIODS.	Average daily population.	Deaths.	Medical Pardons.	Suicides.	Insane.	Delusions.	Totals.	Medical Removals.
Four years of long terms, viz. 1844-5-6-7	445	11	16	—	3	10	40	9
Three years and a half of short terms, viz. 1843, 1848-9, 1850, actual numbers	447	11	15	3	14	12	55	51
Ditto, ditto, proportional numbers	447	12·7	17·14	3*	16	13·7	62·7*	58·28

The first year having been a period of rigid discipline, it may be well to exhibit the results when that year is omitted. The sum of the statistics of the four years, 1844-5-6-7, will represent the results of the original system for long terms and rigid discipline. The

corresponding totals for the last two years and a half will represent the results of the altered system, during which the term has been reduced and the discipline relaxed, for the purpose of preserving the mental and bodily health.

PERIODS.	Average daily population.	Deaths.	Medical Pardons.	Suicides.	Insane.	Delusions.	Totals.	Medical Removals.
Four years of long terms and rigid discipline	445	11	16	—	3	10	40	9
Two years and a half of short terms and relaxed discipline actual numbers	486	9	12	3	12	7	43	36
Ditto, ditto, proportional numbers†	486	14·4	19·2	3†	19·2	11·2	67	49·6

* No proportional increase is taken under the head of suicides.

† The difference in the population is not estimated.

* No proportional increase is estimated under the head of suicides. If it were, the casualties would

"If we exclude insanity and delusions, and limit the comparison to the mortality, then it will be found that the average annual mortality during the four years of long terms was to the daily population, in actual deaths, 6·18 per 1000; during the three years and a half of short terms, 7·67; during the last two years and a half of both short terms and relaxed discipline, 8·23 per 1000. If we take the whole five years during which the original system prevailed, the average in actual deaths was 5·8 per 1000; but during the last two years and a half we have seen that it has been 8·23.

"Thus there has been an excess of mortality during the years of short terms above those of long terms in the ratio of 6·18 : 7·67, and the increase in the average mortality during the two last years and a half, above the first five years of the original system, has been from 5·8 to 8·23 per 1000. The ratio of medical pardons to the daily population has not much varied. During the last five years it was as the ratio of 8·9 to 1000; during the last two years and a half as 9·05 : 1000."

"If suicides were included in these estimates the results would be even more adverse to the validity of the avowed reason for curtailing the term from 18 months to 12. The returns would then be—

Five years of the original system,
average mortality 5·8

Two years and a half of the altered system, average mortality 13·37

"But the suicides are not included. Upon moral grounds far greater weight attaches, and no doubt will attach, to these disastrous cases, than would be correctly represented by their numerical value. They stand by themselves. During the first five years in which the prison was opened not one case occurred. Since the recent changes in the system, in two years and a half, there have occurred three.

"These naked facts appear more than sufficient to annihilate the argument for reducing the term of separate imprisonment from 18 months to 12 on the ground of increased risk to the mental health during the protracted period."*

It is apparent from all that has pre-

ceded that various influences must conspire before reason will be entirely overthrown under any system of prison discipline. These are to be found in previous habits and course of life, personal character, the greater or less amount of religious and secular instruction that is afforded, the character of associates, and the extent to which association is permitted—all or most of these conditions contribute to the production of insanity among prisoners. Their separation from each other withdraws the vicious and admits the alleviating influences: it favours reformation; and, by favouring reformation, soothes the mind, removing the injurious effects of evil passions on the mind, and thereby acting beneficially on the mental health; and, as body and mind exert mutual influences on each other, it must also tend to diminish the bodily injury. Whatever tendency, then, there may be in prison discipline generally to produce insanity, is counteracted under the separate system, when its educational and reformatory elements are fully brought into play. This is shown by the actual increase of insanity as concurrent with the decrease of moral and religious instruction in Pentonville.

"What to do with our convicts" has always perplexed the British Government: it has ever been felt in England that it was undesirable to return so much vice and such elements of discord loose upon the dense populations of our towns, there to increase the amount of crime, or to foment rebellions, as in France. The recommendations of successive commissions have all encountered obstacles in reconciling with the rights of colonists the protection of society and the claims of justice. One of the most formidable difficulties recently encountered has been the opinion that we have already alluded to, of the injuriousness of the separate system to body and mind. This opinion has been

* See Mr. Burt's supplementary letter in the Appendix to the Report of the Select Committee of the House of Commons.

fostered by medical men on insufficient grounds, has been echoed in Parliament, and has thus, as it is now demonstrated by facts, impeded the progress of the best interests of society in the safe disposal of criminals. We trust, however, that the evidence contained in the Report, which we have here fully examined, will disabuse the public and professional mind on the subject, and permit a free and full trial of the system, for the removal of any doubt which may remain in the minds of those who have neither the time nor inclination to examine statistical documents. For ourselves, we may remark that the evidence supplied by this valuable and elaborate Report is satisfactory and conclusive. The facts stated by competent observers, who cannot be suspected of an intention to deceive the public, leave no room for reasonable doubt that *the separate system may be safely carried out without incurring greater risk to bodily and mental health than is inseparable from imprisonment under any conditions whatever.*

It is, however, a remarkable fact that, despite these proofs of the superiority and comparative safety of the separate system, even for long periods of confinement, the Parliamentary Committee have come to the following conclusion:—

"That separate imprisonment ought to be applied to prisoners under long sentences, during the earlier period of their imprisonment; but this Committee does not recommend that it should, in ordinary cases, be enforced for a longer period than twelve months."

VELOCITY OF THE GALVANIC CURRENT IN THE ELECTRIC TELEGRAPH.

It was stated by the Earl of Rosse, at the recent meeting of the Royal Society, that some experiments had been recently made in France to determine the velocity of the galvanic current in the electric telegraph, and the result was that this velocity had been greatly overrated. It has been found not to exceed a rate of from 11,000 to 23,000 miles in a second of time.

Rebiews.

The Surgeon's Vade-mecum. By ROBERT DRUITT, F.R.C.S. 5th edition. 8vo. pp. 600. London: Renshaw, and Churchill. 1850.

WHEN a medical work has reached its fifth edition, it is merely necessary to make the announcement of its publication. Mr. Drutt's manual is a well-known student's companion; and it has, we believe, had a larger sale than any work of its class. It is a neat compendium of surgical information, not intended or fitted to take the place of larger treatises, but still well adapted to guide the student to a sound knowledge of the rudiments of surgical science and practice. The present edition has been carefully revised; it has attached to it a well-written chapter on the use of chloroform and other anæsthetics in surgery, and it is profusely illustrated by numerous well-executed wood-engravings. We have no doubt that the appearance of this new edition will be acceptable to the greater number of students who have just commenced their attendance on practice and lectures.

Atalektasis Pulmonum; or, Closure of the Air cells of the Lungs in Children.

By GEORGE A. REES, M.D., Lond. Pamphlet, 8vo. pp. 42. London: Highley. 1850

THE description here given of a form of disease until recently unnoticed, is clear and full. Dr. Rees was one of the first to draw the attention of the profession in England to the morbid state of the lungs known as atalektasis; and, from the attention which he has given to this subject, his opinions thereon are trustworthy. The disease is distinct from pneumonia, according to Dr. Rees, although by some it has been regarded as that affection modified by the peculiar state of the lung before and immediately after birth.

The symptoms enumerated by the author as characterising or occurring with atalektasis, are, altered movement of the ribs on respiration, laryngismus stridulus, cough, dyspnoea, emaciation, palpitation, cyanosis, and deformity of the chest.

Dr. Rees's remarks on the diagnosis, prognosis, morbid anatomy, treatment, and appended cases, are brief but good. The entire pamphlet constitutes an excellent practical monograph.

Proceedings of Societies.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, Nov. 26, 1850.

Dr. TODD, M.D., F.R.S., TREASURER, in the Chair.

A Case of Softening of the Spinal Marrow in a Boy affected with Chorea. By ROBERT NAJENNE, M.D., Physician to St. George's Hospital.

A BOY, aged 17, was admitted into St. George's Hospital, on the 27th of June last, with symptoms of acute rheumatism, and presenting at the same time well-marked symptoms of chorea. A mitral systolic murmur could be heard. The boy had suffered from previous attacks of rheumatism since the age of twelve, but his friends had never seen him "in this way" before. The last attack of rheumatism was about Christmas, 1849. During five weeks before he came to the hospital convulsive motions of the hands had been observed. During the last two weeks he had stuttered much, and frequently contorted his mouth. Five days before admission he had been to market in a cart; complained much of the jolting, and the next day was unable to leave his bed. He did not attempt to walk afterwards, and he was said to have had a fit on the morning of the 24th.

He was somewhat relieved by the treatment employed during the first day or two; he then became worse, more restless, delirious, feebler, and violently convulsed. For two days he passed his motions under him. Some hours before his death the convulsive movements ceased, and he became comatose.

The body was examined 12 hours after death. There was congestion of the veins of the spinal canal, and of the veins and sinuses within the cranium. About an inch of the *entire thickness* of the spinal cord, opposite the third and fourth dorsal vertebrae, was *white, softened, almost diffluent*, the rest of the cord being in its natural state. The brain was of firm consistence and injected with blood. The pericardium adherent; the mitral valve fringed with small vegetations; the lungs congested.

The author remarks upon the points of interest of the case—chorea associated with acute rheumatism, and palsy of the lower extremities depending upon softening of the spinal marrow. He refers

to four cases on record where softening of the spinal marrow existed in persons who had been affected with chorea. He does not consider there is any reason for supposing that in these cases the softening of the spinal marrow was the cause of the chorea. His observation of this disease would deter him from connecting it with disease of the brain or spinal cord. He believes a careful study of the phenomena of the disease during life to be the best means of gaining a truer insight into its nature than we are possessed of at present. He refers to three other fatal cases of chorea which have been observed by him in the wards of St. George's Hospital. One occurred in his own practice, and the other two were patients of Dr. Macleod.

He concludes by remarking, that the curious fact of the existence of sensation, and the transmission of nervous impressions to the palsied muscles of the lower extremities, although the spinal marrow had undergone the change of structure above described, shows that there is yet much for us to learn respecting its functions.

PATHOLOGICAL SOCIETY OF LONDON.

DR. LATHAM, PRESIDENT.

November 5, 1850.

DR. QUAIN exhibited a

Specimen of Fibrous and Fatty Degeneration of the Heart, with the following History.

LADY H—, æt. 58, on rising from bed, on the morning of Oct. 21st, went to the night chair for the purpose of emptying her bladder, then laid down again, and died immediately.

When about thirty years of age she went to India, where she remained fourteen years without any serious illness or inconvenience, and returned to this country some fifteen years ago. She enjoyed good health, save that she complained of headache occasionally, until about seven years ago, when, in her 51st year, the menstrual functions being still regularly performed, she had an apoplectic seizure one evening after returning from a concert. This caused a confusion of intellect and a loss of speech, which lasted for eight or nine days, but no general paralysis of sensation or motion. About eighteen months after she had a second, but a less severe attack, followed by slight weakness of the right upper and lower extremities. Within a similar period she had a third attack

this more resembled the first. She was then in her 54th year, and the menstrual functions began to be irregularly performed. Each attack was treated on the usual antiphlogistic plan, with mercury, and it was remarked that she bore bleeding badly. Soon after the last attack, that is, four years ago, she began to complain of shortness of breathing on exertion. This and the partial paralysis interfered with her exercise, and she became stouter. She continued pretty well until October 1849, when she was one night seized with a most distressing attack of difficult breathing, which lasted about five hours, and was relieved by ether and ammonia. She had no return of the distress, and did well, with the exception of an attack of common bronchitis in February last, until the commencement of September last, when being at Tunbridge Wells she had several attacks, at intervals of two or three nights, of the same difficulty of breathing. For three of these attacks she was rather freely bled. As she still continued very ill she came to town, and I saw her soon after, in consultation with Mr. J. C. Langmore. She was in bed, suffering from a sense of faintness or exhaustion consequent on a distressing attack during the night. These attacks generally came on about two o'clock in the morning, and were of a very alarming character. The extremities and face became cold and livid, the pulse extremely feeble and irregular, and the sense of impending suffocation most urgent. A severe pain in the region of the heart and across the chest came on as the sense of suffocation became less. The pulse was 68, and compressible; it was generally about 60. She was free from giddiness and noise in the ears. She slept well, except when disturbed by these attacks, and was not subject to faintness. She complained of slight pain in the region of the liver, and the motions were pale and lumpy. Mr. Langmore described the urine to be healthy. The heart's impulse was weak, but the dulness in this region was more extended than natural. The first sound was remarkably low and prolonged, the second clear, and hard over a considerable extent of the upper part of the chest. There was no murmur. Some slight sonorous rhonchus was heard in the back. Considering the age, conformation, and sedentary habits of this lady, and also the characters of the symptoms, for which, in the physical signs, no other explanation could be found, the conviction was, that the disease was fatty degeneration of the heart to a moderate extent,—an opinion strengthened by the presence of an *arcus senilis*. The result justified this opinion. She died in about five weeks after my first seeing her.

The treatment consisted of mild mercurials, with taraxacum and aperients; alkalies, with bitter tonics, and a few leeches to the region of the heart, to be followed by iron and antispasmodics during the paroxysms, which produced very beneficial effects. The fits became much less frequent and severe before her death.

Post-mortem.—The body and viscera generally covered with much fat. The calvarium was thick, firmly adherent, and deeply marked inside by vessels. There was more serum than usual in the cavity of the arachnoid and ventricles. On slicing the brain, the remains of the causes of the three apoplectic seizures were found: one, a small spot, the size of a pea, situated in the fore part of the left hemisphere, towards the base, was composed of a transparent greyish substance, like cellular tissue, infiltrated with serum. In each corpus striatum, towards its upper part, was found an orange-coloured spot, about the size of a haricot bean, soft, and in the left having somewhat of a cystic appearance. The white fibres were here broken up and distorted. The arteries of the base were studded with atheromatous or fatty matter.

The lungs healthy. The heart presented rather more fat than usual on the surface, and might weigh about 14 oz. The anterior branch of the left coronary artery was extremely ossified. The right auricle was dilated, thickened, and filled with dark blood. The right ventricle not dilated, but its walls thickened. Their tissue appeared healthy. The left auricle and ventricle were contracted, and nearly empty. The wall of the left ventricle, at its thickest part, measured thirteen lines. The valves healthy. The muscular substance of the septum towards the apex, and the wall of the left ventricle towards the anterior surface of the heart, presented the pale buff colour of fatty degeneration, which passed into and contrasted with the healthy flesh colour of other parts of the heart. A species of ecchymosis, scarcely amounting to hemorrhage, appeared in the diseased part of the wall of the left ventricle. Under the microscope the usual characters of fatty degeneration were found; but there existed also a large quantity of a fibrous texture mixed with the fatty degeneration.

The liver was enlarged, lobulated, hard, and moderately granular. Both kidneys rough and granular, one being larger than natural, the other smaller.

The importance and the possibility of recognising this disease during life was pointed out, even when not very considerable. The treatment, which seemed to a certain extent to have controlled the disease, was referred to, and allusion made to the relation which existed between the

ossification of the coronary artery and the portion of heart affected; showing clearly that the degeneration was due to impaired nutrition.

Nov. 19, 1850.

MR. PRESCOTT HEWETT presented two specimens of

Aneurism of the Mitral Valve.

and a third specimen, in which the appearances so exactly resembled this affection that they were at first mistaken for it.

In the first preparation two well-marked aneurismal pouches existed in the anterior flap of the valve: the larger pouch, of the size of a filbert, was situated close to the attachments of the chordæ tendinæ, and formed a projection into the left auricle; the smaller one, situated a little higher up, presented a jagged opening, which measured four lines in its greater diameter, and two in its lesser. The pouch itself, of a vermiform appearance, and six lines in length, projected also in the left auricle. The internal membrane of the ventricle was perfectly continuous with that lining these pouches, both of which still presented some remnants of coagula. The apices of both aneurisms had been destroyed, having apparently been cut off. In this case the middle aortic valve presented a large ulcerated opening, the margins of which were covered by long soft vegetations; the other valves were somewhat thickened, as well as the root of the aorta, by atheromatous deposit.

In the second preparation the aneurismal pouches were much smaller, but still well-marked. Two in number, they were situated in the anterior flap of the mitral valve, nearer to the aortic opening: they were partly covered with the remnants of coagula, and projected slightly into the auricle. The mitral and aortic valves were covered with long and very soft vegetations.

In presenting these preparations, Mr. P. Hewett regretted that he had no notes of the history of the cases: having been requested by Dr. Peacock, he had brought them forward, as they were well-marked specimens of an affection which was considered as somewhat rare; but he added that, in addition to the cases now before the Society, he had met with four or five others of a similar nature.

In the third preparation—the only one of the kind which had fallen under Mr. P. Hewett's notice—the anterior flap of the mitral valve presented, towards the centre of its ventricular surface, an opening of the size of a pea, with perfectly round margins, leading into a cavity, which at first appeared to be that of a small aneurismal pouch, projecting into the auricle. On

further examination, however, these appearances were found to be produced by a coagulum of blood, which had become adherent to the margin of the opening in the valve; while in the centre of this coagulum there was a small cavity, occupying nearly its whole length. One side of the coagulum was easily detached from the opening, and the true nature of the appearances was at once made evident. The mitral valve was otherwise quite healthy. The heart itself was dilated in all its cavities, and its muscular structure, much atrophied, was flaccid, and easily lacerated. This preparation had been taken from an elderly gentleman, who died of acute inflammation of the membranes of the brain, and had never presented any heart symptoms.

Dr. RISDON BENNETT exhibited (for Mr. Middleton) a specimen of

Carcinoma of the Lung.

A person, æt. 27, had an acute attack of pleurisy in October 1847, which yielded readily to treatment. In March 1848 he was under treatment for secondary symptoms. The following year, in April, he had a second severe attack of acute pleurisy, from which he recovered in about five weeks.

Dec. 1st.—He was suffering from cough, expectoration, night sweats, and emaciation. There was dulness on percussion at the upper part of the left lung, and but little air entered that organ. There appeared every indication of solid deposit taking place: this spread downwards so rapidly, that on the 16th January (six weeks afterwards) it was pronounced to be effusion into the pleura,—so perfectly dull now was every part of the left chest. The expectoration soon ceased while taking cod-liver oil. On the 9th February the diagnosis was supported by a second opinion; and he was treated more actively, but the disease remained stationary: his general health improved, but the complete dulness remained, with absence of respiratory murmur. After being five months in bed, on the 7th May he left town: his appetite improved; he gained flesh; and, in a short time, could walk three or four miles. He returned in August, without any alteration in his chest symptoms, but complaining of a tumor, about the size of a walnut, anterior to the left ear, attended with considerable pain, and two smaller ones on the left side of the heart; they were of a malignant nature, and quickly increased in size; the pain in them became distressing, preventing sleep: he had two or three attacks of loss of consciousness, and was for a time imbecile. At the beginning of November he was expectorating copiously an offensive

matter which rendered the room almost unbearable. There was no doubt gangrene had taken place. He died on the 14th.

Examination.—The right side presented adhesive bands between the surfaces of the pleura, allowing, however, free play to the lung: its upper lobes were emphysematous, but otherwise healthy; the lower one was affected by gangrene. The left lung was connected by firm adhesions to the walls of the chest: the whole lung was solidified, the upper third forming one solid mass of malignant disease, pressing upon all the large blood-vessels, and obliterating the left bronchial tube within an inch and a half of its origin. There was also an enlarged bronchial gland at the bifurcation of the trachea, containing calcareous matter. There was no fluid in the cavity of the pleura.

LIVERPOOL MEDICAL AND PATHOLOGICAL SOCIETY.

November 1850.

Tetanus in an Infant. BY DR. NEVINS.

THIS case occurred in a negro child, about fifteen months old. It awoke in the night, crying, and complaining of pain in the abdomen, which was supposed by the nurse to be colic, and excited no attention. In the morning the feet were rigid and swollen, the great toes were drawn under the adjoining ones, and the soles were hard and tense. The knees were bent, and the fingers were rigidly contracted, the thumbs being drawn into the palms. There was a frequent slight spasmodic movement of the under eyelids, with the exception of which the head was unaffected. The child protruded its tongue when ordered to do so, and there was no curvature of the spine, either backwards or forwards. The flexor muscles of the extremities seemed alone to be affected, and pain was excited by any attempt to straighten them, but the child did not cry when left at rest.

He had had a slight bowel complaint for a week previously, but had not been troubled with worms, nor exposed to cold, nor to insufficient ventilation. I could not discover that he had had any accident; and the nurse said he was quite well, except the diarrhoea, until he awoke in the night, crying. I find, however, on further inquiry, that he had complained of pain in walking for nearly a week; but this was attributed to his shoes having been too small; and a larger pair had been ordered.

On percussing the spine, there was some degree of tenderness over the two lower cervical and one or two upper dorsal ver-

tebrae, but not in any other part. There was no febrile excitement.

Treatment.—He was kept completely under the influence of chloroform above a quarter of an hour without the slightest effect, the rigidity continuing unabated. Three leeches were then ordered to be applied to the tender part of the spine, and a poultice afterwards. He was immersed in a hot bath, gradually raised in temperature, for above half an hour, and took *two minims of laudanum*, and four of spirit of sulphuric ether, *every hour*. In the evening the twitching of the eyelids had slightly increased; he had sweated profusely, and dozed at intervals, but the rigidity was not at all diminished.

Three leeches were applied to the occiput, and his head was shaved and kept cold.

Next day he was still free from pain, and intelligent; but the rigidity was not at all diminished. He showed no signs of being under the influence of opium, though he had taken about *forty minims* of the tincture. The ether had evidently produced no benefit, and the rigidity was too constant to arise from mere spasmodic contraction. The pulse was feeble, and further leeching did not appear indicated by the tenderness of the spine, but a blister was applied along its whole course. He took *four minims of laudanum*, and twenty of antimonial wine, *every hour*, with the sixth of a grain of calomel; and milk diet was given.

In the evening there was little or no amendment, but he was not worse. As there seemed to be a possibility that it might be owing to the irritation of worms, a turpentine clyster was administered; the other remedies being continued hourly.

Next day the rigidity had much abated; there were no worms in the evacuations; no signs of narcotism, though he had taken *two drachms of laudanum* in *two days*; he was not sick, and was still intelligent; the motion of the eyelids was very trifling, the blister was very sore. It was ordered to be poulticed; and the opium, antimony, and calomel were to be given every hour and a half.

The next day there was continued improvement, and the remedies were given every two hours; but the following day he was worse. The rigidity had so far abated that the feet were soft, and all the fingers could be straightened without difficulty; those of one hand remained straight, but the other contracted on removing the band. he sat up in bed, and played with one hand. To day, however, not do this. though not bowels were to this circ

off. The calomel and antimony were therefore discontinued, but the laudanum, in four-minim doses, was given every hour for a few times, and then every two hours. The opinion formed seemed to be correct; for, as the bowels became tranquil the rigidity again abated, and from that time his improvement has been steady. His hands are open, and his feet soft, and the toes pliable. He can move his legs, as in walking, but can not yet (ten days after the commencement) bear his own weight. His muscles are all soft and moveable, and he takes his food and chatters as usual. All treatment is for the present discontinued, and there appears every probability of entire recovery in a few days.

About a week after this report the child was thoughtlessly taken out of the house, the day being cold, and all his symptoms returned, though in a slighter degree than before. Opium, and small doses of calomel, were ordered, and the child was clothed in flannel from head to foot. In about a week he was well again; and after an interval of several months he is still strong and healthy, though, like most negro children, very susceptible of cold.

It is not usual in this country to meet with tetanus in a child of this age; trismus nascentium generally occurring when only a few weeks old. I lay stress upon this child's not having been exposed to imperfect ventilation, because of the effect found to be due to this cause in the Dublin Lying-in Hospital. Previous to the mastership of Dr. Collins, the disease was rarely absent from that institution: the old practice prevailed of keeping the women hot and smothered up; and the windows being seldom opened, the wards were extremely close and offensive, and a large proportion of the children (I believe one in three or four) died of trismus, or were affected with it. Dr. Collins ordered the windows to be constantly open: the wards became sweet, and trismus from that time became almost unknown. During my six months' residence in that hospital only one case occurred, and that was in a wretched house in the slums of the city.

The cause of the tetanus in this child is not very apparent. It is sometimes produced by worms, but that cause was absent in this instance. I conclude that the tenderness of the spine indicated some inflammatory action in the theca spinalis, which caused the affection to be limited to the extremities. The head was unaffected the whole time, except the spasmodic twitching of the eyelids, which was so different from the permanent rigidity of the extremities that I question whether they had any con-

nexion with each other. This inflammatory origin of the disease derives some countenance from the effects of treatment; for, whilst antispasmodics produced no benefit, the blister, the antimony, calomel, and opium, were followed by marked improvement.

The effect of cold in producing a return of the symptoms was very strongly marked, and great care has been requisite in keeping him warmly clothed since then.

Dr. MACNAUGHT said that it was extremely difficult to account for the occurrence of trismus amongst negro children. He had observed in the West Indies that on some plantations it was for long periods scarcely ever absent, so that hardly a child could be raised upon them, though every care was taken as to ventilation and treatment, at a time when, being slaves, the children were valuable. Without any assignable cause these estates would then become healthy, whilst others which had previously been free from it became affected, and the children died from trismus in them. He had not been able to discover any satisfactory explanation of these facts.

Dr. RAMSAY found in the tropics that the susceptibility of tetanus was about 8 in blacks to 1 in white men.

Dr. NOTTINGHAM remarked that negroes, after losing their fingers from cold, have become tetanic; but he thought that children generally were liable to it after very small injuries: a child pricked its foot with stubble, became tetanic, and died. The woorara poison had been suggested as a remedy in tetanus, and had been used upon horses, which had been kept free by it from the spasm for three quarters of an hour, but died at last. He had dissected a tetanic foot and leg, and found the nerves swollen in several parts between the knee and foot. He inquired whether injury of the *trunk* of the body ever produced fatal tetanus.

Mr. HARRIS mentioned a man who was burnt over the abdomen: the slough separated, and he was seized with tetanus.

Dr. TURNBULL mentioned a man whose scalp was wounded, which was followed by fatal tetanus.

Hernia through the Linea Alba. By Mr. HARRIS.

A YOUNG woman, after her first labour, which was not unusually severe, felt an abdominal tumor, which proved to be a hernia through the linea alba of such extent as to reach from the pubes to the umbilicus, the recti muscles being separated throughout this extent so far as to admit of four fingers in breadth being placed between them, when the hernia was reduced

Interval of several weeks between the Delivery of Twins.

Dr. NEVINS related the case of a woman who was delivered of a premature fetus, and, after a period of six weeks, was confined of a full-grown child, which was still-born.

The case gave rise to a conversation on the subject of superfoetation, in which Dr. LANCASTER said that it was reported to be not uncommon in Australia. When he first arrived there he did not believe in its existence; but so many cases were related to him, by surgeons, of full-grown children being born some weeks after delivery of apparently full-grown fetuses, that he could not doubt nor dispute the evidence.

Dr. MACNAUGHT had made careful inquiry on the plantations in Jamaica, on which cases of superfoetation related by authors were said to have occurred, and he could gain no information of any such events within the recollection of any living person.

Perforation of a Large Artery in the Stomach.

Mr. HARRIS showed the stomach of a young man who had had several attacks of hæmoptysis, varying from a pint to two quarts at a time. An ulcer near the cardiac orifice of the stomach was found after death, which had pierced an artery of sufficient size to admit of a common probe being passed into it.

Scirrhus Mamma.

Mr. BANNER showed a breast in which cancer had proceeded too far, before being placed under his care, to admit of operation. The patient had for many years been subject to pulmonary affections, and the surface of the lungs was found after death to be studded with hard tubercular deposits, which would probably have prevented a favourable result had the breast been removed by operation.

Cancerous Testicle. By Mr. HAKES.

The testicle had been seized hold of some years previously, and had gradually swollen to a large size, accompanied by considerable pain, and still more inconvenience from its weight. The cord was entirely unaffected. On removing it, it was found to contain numerous cavities resembling hydatids, and to be in a state of cerebri-form cancer throughout.

On the Efficacy of the Tampon in Uterine Hæmorrhage.

A paper was read by Mr. JOHN TAYLOR in which he maintained that, in every case of gestation, the tampon acts as a mechanical plug, arresting hæmorrhage:

ture's own way—that of producing a coagulum, without the commonly apprehended danger of converting an open into a concealed hæmorrhage. It has a two-fold action, according to the manner of using it: that is, when the vagina is fully plugged, so as to afford some degree of pressure posteriorly, it invariably produces uterine contraction, terminating in expulsion of the ovum; and, when used more slightly, as a mere pledget, it serves to promote coagulation and stop the bleeding vessels. Different effects can therefore be obtained, according to the intention in view. When efficiently used during parturition, it converts rigidity into softness and expansion, thereby favouring the operation of turning. Contingent on its power of arresting hæmorrhage, it prevents alarming syncope and convulsions, the lacerable condition of the cervix ensuing upon long drainage from the part, and also all the ill consequences resulting from large losses of blood in the early stages of labour, before safe delivery can be accomplished; and he showed that it is equally applicable in the accidental and the unavoidable forms of hæmorrhage before delivery, cases being related from his own practice in proof of these views.

The great danger of forcible delivery under rigidity, and its unprevented sequence—inertia, were pointed out, with the opinions of various authors thereon, both as respects injury to the uterus, and from the often fatal shock of sudden delivery. On these points the tampon was shown to be frequently superior to forcible delivery, on account of the great security it affords from flooding, its slower action in expanding the os, and its being generally more imitative of nature; the apprehension of danger from concealed hæmorrhage being ideal.

The author observed that, although the tampon and artificial delivery are considered as rival powers, enlisting supporters on either side, the former should be regarded as the handmaid rather than the rival of the latter, merely arresting hæmorrhage, producing dilatation by the gentlest means, and enabling the accoucheur to make choice of his future mode of action; either continuing the tampon for the support of uterine action, retaining or loosing the liquor amnii at his pleasure, and relying on the natural powers for the expulsion of the fetus, or turning, as may be thought most expedient, after the necessary amount of expansion has been acquired.

Several fatal cases recorded in the standard have been saved, had the tampon been earlier

NEWCASTLE AND GATESHEAD PATHOLOGICAL SOCIETY.

March 1850.

Case of Closure of the Pulmonary Artery, with Obliterated Ductus Arteriosus and Open Foramen Ovale—Cyanosis.

MR. WALLIS, of South Shields, communicated the following history of this very remarkable and interesting case:—

M. A. Cleet, at 18 years, presented the livid hue of morbus cœruleus, which was permanent and increased by exertion or mental emotion, even the nails assuming a dark livid tinge on such occasions, and the dyspnoea, which distressed her much, then becoming aggravated.

These cyanotic symptoms first appeared twelve years ago, after a fall into a deep pond, in which she was immersed over head, but for how long could not be ascertained: had scarcely been able to go across the room for the last eight weeks preceding her death: the act of rising from the bed brought on an attack of orthopnoea: was able, a few years ago, to walk in the streets, but with great difficulty: employed herself in the house with knitting, sewing, &c.: her disposition, formerly haughty, was latterly more subdued, and she complained of pains in the shoulders, and about the region of the heart, and severe headache, with more than usual oppression about the chest. Nothing abnormal could be heard in the heart's sounds, except that they were sharp and rather distant. Pulse quick, weak, but regular.

On the 28th February, 1850, she was seized with convulsions about 2 o'clock P.M., after which she was comatose, and occasionally convulsed during the evening: moaning on expiration, and died at midnight.

Her mother and friends positively assert that she was perfectly healthy until she fell into the pond when six years old; was ruddy and playful; had neither asthma nor shortness of breath. Since that time she had been afflicted with blueness, palpitation and dyspnoea: menstruated regularly; began three years ago: generally appeared worse after the catamenial loss, and expected the discharge on the day preceding her death.

Post-mortem examination thirty-six hours after death.—Body tolerably well developed for her age; short, and rather stoutly built; mammae and chest of usual size. On removing the sternum the lungs appeared much collapsed, on the left side especially, leaving the heart large and prominent:

lungs crepitant throughout; edges of left lobes indurated and semi-hepatized: the pericardium contained a little more fluid than usual, and the heart appeared large, distended, and considerably loaded with fat; coronary veins dilated and prominent. The heart was removed, and numerous coagula escaped from the distended auricles, which were found connected by a wide opening (the foramen ovale), through which two, or even three fingers, could be passed with ease. The auricles, in fact, formed one large oblong cavity, and a smooth fold of the endocardium, one-sixth of an inch in depth, hanging from the roof of the auricle, was the only indication of the annulus ovalis. The heart was brought away for further examination. Liver large, extending over to left hypochondrium; mesenteric and other veins of abdomen large and turgid;—indeed the whole nervous system was in the same engorged state: spleen a little enlarged; kidneys large and congested; capsules easily peeled off; cortical portion in excess, and enveloping the tubuli to their apices; external aspect of kidneys presented numerous fissures and lobules: each ovary had two projecting vesicles about the size of a large pea, and one or two indistinct cicatrices on its surface.

The heart, on further inspection, presented a very small auriculo-ventricular opening on the right side, scarcely admitting more than one finger, and a few vegetations adhered to the edge of the tricuspid valves: the right ventricle could not contain more than a fluid ounce; its walls were much hypertrophied, half an inch thick; water poured on the tricuspid valves passed slowly, if at all, into the auricle. The most extraordinary circumstance was, that no communication existed between the right ventricle and pulmonary artery, a hard callous contraction precluding all exit in that direction, and a yellowish tendinous state of the endocardium was also observed at the apex of the infundibulum; the pulmonary artery was small, but admitted a finger easily, and ended in a conical *cul de sac*, just above the right ventricle. Air blown into the pulmonary artery found no way of escape. On slitting open the vessel three pouches were seen formed by the adhesion of the semilunar valves to the sides of the artery, and a cartilaginous callosity blocked up the commencement of the artery. A probe did not pass further than a quarter of an inch into these valvular pouches. The left ventricle was dilated to about twice its normal size, its walls being about the thickness of those of an ordinary right ventricle; the proportional thicknesses of the ventricular walls were completely reversed; left auriculo-ventricular opening was healthy, as were the aortic

valves. As the ductus arteriosus had been left in the body, and it was thought that it might have been open, and thus afforded a passage from the aorta to the pulmonary artery, access was again obtained to the body, and the arch of the aorta removed, with what remained of the impervious ductus arteriosus.

Both the heart and the arch of the aorta last obtained were forwarded to Dr. Emberton for more minute examination, and the result of his dissection is now submitted:—

"April 9th, 1850.—The heart sent to me by Mr. Wallis, of South Shields, appears altogether larger than is usual at the patient's age, notwithstanding that it has been some time out of the body, and steeped in spirit and water. Its weight is, however, 8 oz., not more than the average as given by Dr. Clendinning, but rather above the averages of Dr. J. Reid and Dr. Peacock.

The right cavities are totally diminished in capacity, the walls of the ventricle very much thickened, being from $\frac{1}{4}$ to $\frac{1}{2}$ of an inch thick, those of the auricle not so evidently hypertrophied.

The orifice of the pulmonary artery is quite obliterated by adhesion of the free margins of the valves to each other, and to a central coagulum or plug, now quite firm, and undistinguishable in colour and apparent texture from the valves. The calibre of the tube is much diminished at this part, and the valves, though evident enough in outline and having their pouches, are much shrunk in size. The pulmonary artery beyond this occlusion divides, as usual, below and within the arch of the aorta; but the trunk before bifurcation, and the succeeding branches, are all of much smaller dimensions than usual.

The right auriculo-ventricular orifice is very small, corresponding to the state of the ventricular cavity; it is not one-third the size of the left auriculo-ventricular opening. The tricuspid valve is thickened and contracted. The foramen ovale is quite open, and of considerable size. Distinct remains of the valve that normally closes the opening after birth are visible in the form of a narrow, thin, sharp-edged fold of membrane, unbroken, however, and apparently not cicatrized, hanging from the upper and anterior part of the foramen.

The left ventricle appears to encroach upon the right a good deal, and to cause a bulging out as well on the left side of the heart. Its cavity is dilated, and its walls thickened to $\frac{1}{2}$, and even $\frac{3}{4}$ of an inch. The left auricle does not appear materially enlarged; but its walls are thickened. The two auricles form, as it were, only one hamper.

No opening can be discerned between the ventricles.

The aorta appears normal as far as it goes; its arch has the usual primary branches, and it takes the usual course.

The bronchial arteries are normal in all respects, as are also the four uppermost pairs of intercostal arteries; below these the fragment of aorta does not extend.

The ductus arteriosus is quite closed, and contracted at its middle for some distance; it is infundibuliform, and wider below than above. A probe can be passed a short way into it from the aorta, and also from the pulmonary artery; but beyond these parts the ductus is quite impervious. This being the case, and the pulmonary artery being also closed, there seems no route left by which blood could have passed during life from the heart to the lungs for the purposes of respiration, unless the foramen ovale being open, we suppose that it was conveyed in some imperfect way along some of the trunks of the pulmonary veins to the lungs, and returned again by others.

It may be remarked, that both the obliteration of the cavity of the ductus arteriosus and the occlusion of the pulmonary artery had the appearance of long standing."

Disease of Pulmonary Valves reducing the Orifice to the size of a common Quill.

This case occurred in a carman, aged 24, who died in the Newcastle Infirmary, under the care of Dr. White. When admitted he was labouring under general anasarca, with some ascites, had a dusky, puffed out, congested countenance, with blue lips, and complained greatly of orthopnea, palpitation, general oppression of chest and abdomen, and the usual symptoms of confirmed heart disease. The pulse was frequent, weak, and regular. He had been ill for two years, but had only been dropsical for the last three months, and could assign no reason for his illness. The heart was found by percussion to be very greatly enlarged; and a loud murmur was heard at the base of the heart with the first sound, which extended upwards along the sternum: the second as well as the first sound of the heart was very indistinctly heard, the murmurs appearing to come from a considerable distance behind the sternum. His urine was found to be highly albuminous, but of sp. gr. 1.020. Diaphoretics, alteratives, and occasional purgatives, were used for three weeks after his admission, with considerable improvement, until three days before his death.

The autopsy was made twelve hours after death. The pleura and lungs were found to be perfectly healthy; the pericar-

dium enormously distended with six oz. of clear serum, and an immense heart. On opening the pericardium, the right ventricle of the heart were seen of an extraordinary size, occupying the whole of the anterior of the heart, and constituting the apex and principal part of the body of the organ; the left ventricle was with difficulty made out, and existed at the posterior aspect of the heart as a small appendage to the right side. The roots of the great vessels, with the base of the heart, were thrown back to a considerable distance, owing to the great bulk of the body of the organ occupying so large a space immediately behind the sternum. On laying open the pulmonary artery the valves were found to be completely and inseparably glued together, forming a firm and perfect cartilaginous ring, which projected into the tube of the vessel at a right angle to its sides; and, with the exception of a small round aperture in the centre which barely allowed of a common quill to pass through it, completely blocked up the vessel. The artery in front of the valves was very small. The right ventricle was found full of blood, its walls hypertrophied to three or four times the normal thickness, and its cavity capable of holding a very large fist; the auriculo-ventricular opening admitted the whole of the fingers to pass through with ease, and its valves, although healthy, were of course incapable of closing the aperture. The right auricle was dilated to nearly the same size as the ventricle, and was very much hypertrophied: it was distended with blood, and the openings into it of each of the great veins allowed all the fingers, together with the point of the thumb, to enter them, so greatly were they dilated. A firm clot of lymph partially filled the auricle and ventricle, was firmly adherent to the walls of the cavities, and appeared to have been formed some time before death. Both the left auricle and ventricle were much smaller than they ought to have been, appearing as if atrophied, with attenuated, weak, and flaccid walls, and very small cavities; the valves were healthy; the tube of the aorta was only capable of admitting the fore-finger, and had very thin coats. The liver was of great size, nodulated, and presented numerous large hemorrhoidal projections of dilated veins on the surface of the organ; the structure of the viscus was firm, fleshy looking, greatly congested, and the whole hepatic system of veins very greatly dilated and gorged with dark venous blood. Kidneys large, firm, fleshy, and of a dark venous hue, presenting in many parts the cavities of dilated veins.

Mr. Armstrong, of South Shields, related the following case of

Ossification of the Aortic Valves.

Jas. Dodds, *æt.* 49, sailmaker, having applied about twelve months ago with cough, dyspnoea on exertion, &c., was found on examination to be labouring under disease of the aortic valves, accompanied with bronchitis. He was considerably relieved by treatment, but continued ailing, though going about until February 22d, 1850, when he was seized with a smart febrile catarrh, which terminated in pleuro-pneumonia of the right side. His feet and legs became oedematous, he had effusion into both chests, great dyspnoea, and copious expectoration of frothy mucus at first, but afterwards of rusty bloody sputa. In the midst of this attack he was taken with gout in the right and afterwards in the left toe, but it was of short duration. The heart disease and pneumonia formed a serious complication; and, after a protracted struggle, he died on the 16th March. To connect the sounds during life with the appearances after death, Mr. Armstrong read the two following extracts from the history of the case:—"March 4th. Loud rasping murmur with the first sound of the heart, most intense at the arch of the aorta; second sound inaudible; a continuous soft murmur follows the first sound." These sounds were to be accounted for without much difficulty when it was perceived to what an extent ossification of the valves had proceeded. They are described as "forming a rugged chalky ring in the place of the aortic valves, and so contracted as not to admit the finger: this ring is slightly moveable, but could not possibly be closed nor yet much opened during life." "The right auricle and ventricle were much dilated; tricuspid valves healthy; left auricle capacious; left ventricle enormously hypertrophied, walls nearly an inch thick. One of the mitral valves contained a little indurated deposit in its free border."

ACADEMY OF MEDICINE, PARIS.

Nov. 19, 1850.

Local Anæsthetic Medication.

DR. ARAN, member of the Central Board for Hospitals, addressed a note on the value of various anæsthetic agents in reference to local medication. The following are the chief of Dr. Aran's conclusions:—

1. All those volatile agents that are recognized as general anæsthetics possess the same property when applied locally.

2. This local power is not in direct relation to their general anæsthetic power.

but is in an inverse ratio to their volatility. Thus the Dutch liquid, or chloride of olefant gas, exceeds all others; chloroform excels ether, &c. &c.

3. Some of these agents do not produce any irritation of the skin—*e. g.* ethers, aldehyde, and benzine,—while others, as chloroform and Dutch liquid, cause a sense of burning, and, if applied long enough, produce vesication.

4. Therefore the Dutch liquid is the best local anæsthetic agent: its action endures a longer time, it produces only slight stimulation of the surface, and has a less disagreeable odour than the ethers, &c.

5. It is not necessary to employ large quantities for obtaining these local effects: from fifteen to thirty drops of the chloride of olefant gas, covered with a moist compress, will suffice to allay pain in most cases.

Cedema of the Larynx.

M. SESTIER read an essay on this subject, in which he endeavoured to point out its diagnosis and treatment.

Cedema of the interior of the larynx, M. Sestier observed, renders obscure the diagnosis of *angina cedematosa*, by changing one of the principal symptoms of that affection—the facility of expiration as contrasted with the difficulty of inspiration: it further aggravates the danger of cedema of the upper folds of the larynx. In these cases the local applications to the upper part of the larynx are of little use without also active general treatment, such as leeching, croton oil, blisters, &c. Bronchotomy is often to be practised in these cases, as the only means of saving life. According to M. Sestier, however, there is no certain means of the diagnosis of this form of cedema, but it must be gathered from the symptoms generally.

* * There is no form of disease of the air-passages in which the topical application of a solution of nitrate of silver is more beneficial than in cedema about the larynx.

Extraction of a Calculus from the Prostatic Portion of the Urethra.

M. BONNAFORT, surgeon to the hospital at Arras, presented a calculus which weighed twenty-six grammes (=6682 Eng. drachms), was eight centimetres in length (=3.149 Eng. inches), and about two centimetres (=787 Eng. inches) in width at its broadest part. It had the general form of a fish. The patient was thirty-three years of age. He had been operated upon by lithotripsy when six years old, and at the age of thirteen by the

lateral operation; but the calculus being brittle it broke, and the fragments were not all removed. Six months afterwards he experienced great pain at the neck of the bladder, with difficulty in passing water. Suddenly, after strong expulsive efforts, the calculus passed from the bladder into the urethra: some days afterwards it again passed into the bladder. It was subsequently returned into the urethra, where it had remained nine years. Its urethral extremity was round, its vesical extremity thin and pointed.

SURGICAL SOCIETY OF PARIS.

Nov. 20, 1880.

Tumor difficult of diagnosis.

M. MOREL-LAVALLÉE presented a man, thirty years of age, who had been received into La Charité for an inflammation of the arm, of which he had been cured. A tumor, the size of a fist, was observed near the right breast. This growth had existed several years; it was free from pain, and the integuments that covered it were not altered in appearance. On taking it between the fingers it gave a feeling of softness—almost of fluidity; if pressed harder, it was found to consist of granules or lobules, each about the size of a grain of maize. If moved about, it communicated a crepitating sensation similar to that of certain tumors of the wrist described by Dupuytren. A small explorative puncture was made with a lancet, from which there escaped a considerable portion of the tumor, which was thus seen to be a lipoma of an unusual character. The portion that escaped resembled a bunch of grapes, each lobule being attached by a separate stalk to a central stem formed of condensed cellular tissue.

M. LEBERT had examined the structure under the microscope, and found it to consist of fatty and cellular tissue.

M. MOREL-LAVALLÉE had advised the patient not to have the tumor removed.

A Monstrous Fetus.

M. DANTAN presented a monstrosity (of the family *Symeliæ*, genus *Sirénomeles* Geoff. St.-Hilaire). This fœtus was born dead, at the full period, of a healthy mother, and was perfectly well formed in the upper part of its body. The lower part of the trunk terminated in a conical prolongation instead of lower extremities. This prolongation was composed of a thigh, a knee-joint, and a portion of leg about three centimetres (=1.181 Eng. inch) in length, formed of a single pointed bone. The

patella, larger than usual, was not placed on the front but to the left side of the joint. No anal orifice and no external genitals existed. In the place of the latter was a slightly wrinkled fold of integument.

The dissection of this fœtus was not yet complete, but had shown the thigh to be furnished with muscles normal in appearance, but some displaced; the anterior muscles being placed laterally, while others were wanting, or present only in a rudimentary state. The vessels and nerves were placed on the left side. The bone was smaller than usual. In the abdominal cavity the following malformations were observed:—The intestine terminating at the beginning of the rectum by an unattached *cul-de-sac*; the intestinal tube contained meconium; the two kidneys, smaller than natural, were in their usual place, but the ureters and bladder were entirely deficient; the testicles were found in the lower part of the abdomen; the pelvis was a solid mass, with no recognisable form except that of the femoral articulation.

Gun-shot Wound in the region of the Heart.

M. DREUISE exhibited a pathological specimen, of which the following is a brief account:—

An individual was found in the Forest of Vincennes having an extensive wound in the chest: he was taken to the Hospital of Charenton, where he died in about half an hour. On the level of the left breast was an opening of about three centimetres in diameter (≈1·181 Eng. inch), through which the interior of the chest was visible. Portions of the third, fourth, and fifth ribs were removed. The cavity of the pleura contained very little blood. The pericardium presented an opening anteriorly. A bullet was seen to have pierced the wall of the left ventricle, which it had traversed for some distance, but without having entered the cavity of the heart; then passing out, it had again passed through the pericardium posteriorly, and, piercing the lung, was found lodged on the left side of the vertebral column, near the eighth and ninth ribs. The ball was small and round. Near the person of the deceased had been found a brace of small pocket pistols, which had both been recently discharged.

The most remarkable points in this case were the great extent of the injuries inflicted, the absence of hæmorrhage from the lungs, and the duration of life several hours after the receipt of such injuries.

ACADEMY OF SCIENCES, PARIS.

Nov. 18, 1872

Transmission of Sensitive Impressions in the Spinal Marrow.

M. BROWN-SEQUARD read a memoir in which he stated, contrary to the generally received opinion, that the transmission of impressions received by one lateral half of the body is effected in a crossed manner—that is to say, that the right side transmits for the greater part the impressions made upon the left side of the body, and vice versa. According to the opinion which regards the transmission of impressions in a direct line through the cord, the explanation of the phenomena of hemiplegia must be sought in the interlacement of the fibres of the cord in the medulla, pons Varoli, &c. M. Brown-Sequard, therefore, in demonstrating the interlacement of the fibres in the cord itself, has offered a new solution of the phenomena of hemiplegia.

The following are the principal facts on which M. Brown-Sequard founded his opinions:—

After having divided a lateral half of the spinal cord in a mammiferous animal on a level with the tenth dorsal vertebra, it was observed that the limb on the side corresponding to the section of the cord was not only very sensitive, but that it was even more sensitive than in the normal state, while the opposite extremity was very much less sensitive than natural.

The same phenomenon was observed with regard to both limbs on the side of the section of a lateral half of the cord in the cervical region.

If repeated sections of one side of the cord be practised, sensibility remains entire in the limb of that side.

M. Brown-Sequard concludes that the fibres of the cord proceeding from the limbs, and having a connection with the encephalon, must be very few in number.

Employment of Cold Douches in the Lymphatic Temperament.

M. SERRS communicated, in the name of Dr. Louis Fleury, a note on the employment of cold douches in the lymphatic temperament, anæmia, and chlorosis. The following summary presents the chief contents of Dr. Fleury's essay:—

1. That cold douches should be placed in the first rank among restorative remedies by reason of the action which they exert on the capillary circulation, and consequently on the composition of the blood, animal heat, nutrition, and innervation.
2. The lymphatic temperament is modified

fied by those hygienic and therapeutic agents which substitute for it an acquired sanguineous temperament. This agency is attributed by Dr. Fleury to a double action—one on the processes of nutrition and composition of the blood, the other on the capillaries themselves—so influencing their vital properties and contractility, that red globules penetrate where serum alone was admitted previously.

3. The effects of this treatment are manifested first on the digestive organs and muscular system, then on the nervous and circulatory systems.

By these means chlorosis and anemia are also equally effectually cured.

Syphilitic Inoculation.

M. AUZIAS-TURENNE transmitted a note containing his results on this subject. M. Turenne declares the identity of the disease in man and animals. M. Robert de Welz had produced chancres on his arm by inoculation with the matter of chancres previously produced by inoculation in the monkey and the cat. M. Turenne states that, in the successive inoculation of animals with syphilitic poison, each consecutive sore is less inflamed and less characteristic, until at last, after about the fourth inoculation, no effect follows the insertion of the virus. To the state thus induced M. Turenne gives the name of *syphilitization*, from its analogy with vaccination,—as, during its existence, the constitution is protected from syphilitic contagion.

Exaltation of the Sense of Hearing in Paralysis of the Facial Nerve.

M. H. LAXDORFF, Professor of Medicine in the Medical School at Rheims, transmitted an essay containing the following conclusions:—

1. Exaltation of the sense of hearing on the affected side is an almost constant symptom in facial hemiplegia not depending on cerebral disease.
2. This exaltation appears and disappears with the paralysis.
3. It is attributable to paralysis of the muscle of the hammer in the cavity of the tympanum.
4. It indicates that the seat of paralysis is not below the first elbow (*coude*) of the seventh pair.
5. It may exist without facial paralysis.
6. It disappears spontaneously and completely within a period of from fifteen days to three months.
7. To ascertain its existence it is necessary to make an impression on the organ the louder the longer it has existed.
8. Special treatment is useless; cold douches or galvanism have been found useful.
9. This affection lends confirmation to the views of M. Longet on the existence of a motor nerve of the ear analogous to the motor nerve of the eye.

Note on the Contagion and Treatment of Scabies.

M. Bazin addressed a note to the effect that the essay by M. Bourguignon, read at the last meeting of the Academy, contained erroneous statements, and had withheld the fact that to M. Bazin was due the credit of having pointed out the importance of general, as opposed to local friction, in the treatment of scabies.

Hospital and Infirmary Reports.

KING'S COLLEGE HOSPITAL.

Operations for Malignant Growths.

THERE are few operations the surgeon is called upon to perform more frequently than those which are put in force for the extirpation of abnormal growths of the body, which are, or are supposed to be, of a malignant nature; and this is the case, although there has been, and still is, so much difference of opinion regarding the propriety of removing certain malignant tumors. There are those, whose experience and character entitle their opinion to great respect, who strongly advise that malignant tumors should not be meddled with at all, or only under very peculiar circumstances; other surgeons, again, of great repute, do not hesitate to give their patients the benefit of a chance, by employing the knife in a great number of these sad cases. This difference of opinion on such an important point as the extirpation of malignant disease by operative proceeding, depends partly, no doubt, upon the relative amount of success which the individuals in question may have had; and the greater or less success has probably been the result of a more or less happy and judicious selection of the cases for the cure or alleviation of which operations have been performed.

It is not difficult to understand that that surgeon who has had a number of cases of malignant disease to operate upon, which have been of a very bad description, and not well adapted for operative measures, may have formed an opinion that it is better to let such growths alone: a fortunate event has not taken place in any, or but very few cases, and it is but natural that such an opinion should be arrived at. But another surgeon, equally experienced, has, from particular circumstances, the same number of cases under his care,—the disease even may be of the same nature, yet not so inveterate, or of so long standing,—the opera-

tions are here followed by a much greater amount of success: he finds that a fair majority of his patients are relieved of a great deal of suffering, have their lives prolonged, and in some instances are cured, at least for years, by the operative proceedings which have been put in force. Under such circumstances, an opinion more or less favourable to operation is entertained. To a certain extent, at least, the difference of opinion on this point must in this manner be accounted for. It is impossible to ascertain with correctness which opinion prevails most,—that for interference, or that for non-interference with malignant tumors, by the knife. Theoretically, it is probable that the latter does, but practically it must be confessed that the former is in the ascendancy, for a great number of surgeons are in the habit of operating for the removal of malignant growths from the system,—we dare say, too often,—but on the other hand, it is not improbable that many patients suffer a great deal more than they need, and die much earlier, in consequence of neglecting a timely and suitable operation. The best plan, therefore, perhaps in this matter, is to act upon the old maxim, "*in medio tutissimus ibis*;" it is most certainly found to be an advantageous one, in many difficult and important points in medicine and surgery.

Mr. Fergusson is one of those who act cautiously, and steer a middle course relative to this difficult and unpleasant point, of the extirpation of cancerous and other malignant growths. He, on the one hand, does not refuse to operate altogether, nor on the other does he hastily resort to the knife in any case which may present itself. He considers that under favourable circumstances an operation may benefit his patient, and give him a chance of prolonging and saving his life, and he therefore hesitates not, on any suitable occasion, and when there are no serious contraindications, to use the knife.

The practice of this hospital, as of others, has afforded many opportunities of seeing malignant disease, and the results of operations for their relief and cure; and our observation for several years, and of many cases at this hospital, tells us that benefit does accrue to the patients who are operated on for malignant growths; more or less relief is most assuredly given; but it cannot be denied that after a time, when the disease removed has been really malignant, a return in a great number of instances takes place, either rendering second operations necessary, or destroying the patient.

One of the most striking instances calculated to show these truths—viz., that malignant disease may be eradicated for a time by the surgeon's knife, and that life

may be considerably prolonged, and that a recurrence of the disease is too apt, even in apparently favourable cases, to take place sooner or later, was in the hospital during the last year under the care of Mr. Partridge. The patient was admitted with a large fungoid tumor, which was situated in the thigh, just in front of the femoral vessels, as they run along Scarpa's triangle, and encroaching somewhat on Poupert's ligament. The size of the tumor was very considerable, being nearly as large as a man's head.

Now this patient, who was a man hardly in the prime of life, applied at King's College Hospital three years prior to this, he having at this time a large fungoid tumor, situated in the posterior part of the leg, of the same side. Mr. Partridge extirpated the mass; the man recovered from the operation, returned to his occupation, and continued in health for a long period: a return, however, of the same disease takes place ultimately. The cicatrix of the old wound in the leg was perfectly healthy, and the disease was now entirely confined to the upper portion of the thigh. Mr. Partridge again operated; unfortunately erysipelas took place, and the patient died from this cause.

This case itself is an argument in favour of removal of malignant growths, when they can be entirely taken away, although the same kind of disease returned at a subsequent period: had not the original malignant tumor been taken away, it is probable that the patient would not have lived half so long as he did, and, moreover, he would have been in a state of constant pain and misery; but the operation kept him free from suffering for a few years at least, and had not an accidental attack of erysipelas carried him off after the second proceeding, he might have lived for as many years more.

It is to be borne in mind, that there was a greater likelihood of temporary success here, from the situation of the original growth, although it was a fungoid tumor: when a fungoid growth is seated in the orbit, in the substance of the testicle, or within the cavities of the nose, there is a much less chance of success after an operation, because it too often happens that similar deposits are co-existent within the internal organs, or they soon involve them; but when the growth is on one of the extremities, and a considerable distance from the trunk, there is less danger of simultaneous disease in internal organs; consequently the chance is more favourable for recovery after operation.

A case where the arm was amputated for malignant disease was in hospital at the commencement of the present year under the care of Mr. Fergusson: it well illus-

trates the peculiarity of malignant disease to return after a time, as well as the relief which is given by operation. The patient was a healthy-looking labouring man from Kent; and several months previous to the amputation being resorted to, he applied, in consequence of an ulcer situated on the back of the wrist; it was as large as a crown-piece, and it presented that peculiar appearance which denotes malignant action: it caused severe pain; and moreover it was entirely intractable to the remedial measures which were employed. Mr. Fergusson therefore removed the entire ulcerated surface with the knife, carrying the instrument well beyond the edges of the sore, so that none of the disease should be left behind. After the operation the wound rapidly cicatrized, and it was almost entirely healed up, and presented a healthy cicatrix, when the patient left the hospital. In the course, however, of a few months he returned again with a sore on the same spot, and of the same appearance. It produced considerable pain, and shortly before his application a considerable hæmorrhage had taken place, which had caused the man some fear. It was very evident that it would be useless to attempt to remove the disease entirely without removing the arm as well. Mr. Fergusson therefore amputated in the forearm sufficiently high up for the flaps to be away from the disease. A speedy cicatrization of the stump took place, and the patient left the hospital in a few weeks very much improved in his health. This man lately presented himself, when it was seen that the stump of the amputated arm continued in a perfectly healthy condition, and there was not the least appearance of implication of the cicatrix in the original disease. The patient presented all the appearance of vigorous health. Just over the inner condyle of the humerus, however, is a swelling of some size, painful on pressure, and soft. This he has noticed now for some months, at first being very small and hard. It is impossible to state at present whether this is merely an enlarged and inflamed gland proceeding to suppuration, or whether it is a return of malignant disease in that situation. The hopes of the surgeon suggest the possibility of the former, but his fears lead him, not without reason, to expect that it may probably be the latter. The patient has been desired to return again shortly; and we shall make a point of watching and recording the issue of this case.

The observations which have been made with reference to the nature and treatment of cases of malignant growth apply in particular to scirrhus tumors of the mammae. There have been, and are now, in the hos-

pital several cases of interest, and illustrative of what has been remarked with respect to the propriety of removing certain cancerous growths with the prospect of giving relief for a time; also as regards the probability of a return of the disease sooner or later after operation.

There is one case in particular now under treatment which shows the validity of these assertions. A patient, of the name of Peel, a married woman, aged 32, without family, was admitted into the house in October for the purpose of having a morbid growth removed from her right axilla. Her history is as follows:—

About three years ago she first noticed a swelling in her right mamma, which gradually increased in size, and at the end of a year produced so much uneasiness, that she applied to Mr. Fergusson, who pronounced the swelling to be a cancerous tumor of the mamma, and advised operation at once, as there was no implication of the axillary glands, and the health was in other respects good. The operation was performed in February 1849. The tumor was found to be scirrhus. The wound healed up kindly, and the patient was discharged the hospital, and her health soon became perfectly restored. About three months afterwards, however, she felt a small lump in the axilla of the same side. This gradually and slowly increased in size, and latterly gave her pain. Her health nevertheless continued excellent. At the expiration, however, of two years, she became more anxious about the swelling, as it had increased in size, for it had become as large as an egg. Mr. Fergusson therefore advised her to come into the hospital and have the tumor removed, as he had little doubt of its being a scirrhus gland. The cicatrix of the wound made in the old operation was perfectly sound, and there is not the least appearance of return of the disease in that spot. The operation of removing the gland from the axilla was performed on November 16th. The morbid growth was situated so close to the large vessels and nerves in this important space, that Mr. Fergusson was obliged to dissect with very great caution: in fact, the tumor lay just over the axillary artery; and when it was removed the pulsations of the latter could be easily seen in the wound, and the vessel might readily have been wounded without due caution having been exercised. On cutting into the mass it was found to be a gland degenerated into scirrhus. One or two detached and smaller glands were taken away, so that none of the disease should be left behind. This case also will be a very interesting one to watch. This poor woman has certainly received great benefit from the original

operation, and it is to be hoped that she may be spared for some time, or altogether, from the further effects of the disease.

The success after the excision of a scirrhous breast will depend, in a great measure, upon the period at which the operation has been performed—much, also, upon the condition in which the parts are at the time. This of course holds good not only with reference to cancer of the breast, but in like manner to the similar affections in other portions of the body. It would be useless to extirpate, with any hopes of success, a cancerous tumor of the lip, if the sub-maxillary glands are at the same time affected with the disease, or if the growth itself has extensively involved the mucous membrane of the mouth and gums. In like manner, no ultimate benefit can be expected if an operation should have been performed for a cancer of the penis, if at the same time the glands of the inguinal region are contaminated by the disease. It is of equal importance to select the cases of scirrhous mammae. There will be little hope of doing good if the tumor has existed for a long time, and has become adherent to the skin above, and if at the same time the glands of the axilla are affected by the disease; but if the growth is not of long standing, is perfectly circumscribed, and the skin over it is not adherent or diseased, and if there is no implication of the axillary glands, and no symptom of constitutional disease, the surgeon may operate with the reasonable hope of benefiting his patient; and, if she be willing and anxious to have it done, the surgeon is only doing his duty in according to her wishes in such a case. The following is an instance where everything was so favourable for an operation, inasmuch as the disease was entirely confined to the breast, was not of very long standing, and there was nothing to contraindicate the proceeding.

Sarah Pratt, aged 45, unmarried, menstruates regularly, was admitted into King's College Hospital, Nov. 6th, under the care of Mr. Fergusson, with a tumor of the right mamma. She states that 15 months ago she first felt an aching pain in that situation, and subsequently a lump made its appearance on the outer side of the mamma, below the nipple. When first perceived it was the size of a walnut: it increased in size, and the pain in it became constant, and this was aggravated when anything pressed upon it. It continued, however, stationary, for some six months, but during the last nine months it has increased in size with rapidity; the pain has become worse, and changed its character from aching to lancinating. On her admission the tumor was found to be about the size of an orange; it was excessively

hard to the touch, and perfectly circumscribed, very moveable, and unadherent to the skin, except just at the lower part, where the latter is slightly discoloured; the nipple not retracted; no enlargement of the axillary glands; health very fair. Here was an instance where an operation might be performed: there could be no doubt of its being a scirrhous tumor. Mr. Fergusson therefore removed it in the ordinary manner; and, on cutting into it, the whole of the gland was not affected; the upper part was healthy, but the lower portion was decidedly cancerous. This patient is doing well; and it is to be hoped that some immunity from the terrible disease will be granted her through the instrumentality of this operation.

Although in this case the entire mammary gland was not involved in the disease, it was deemed advisable that the whole of it should be taken away; and there can be little doubt that in the majority of instances where a portion only is affected—of course it being plain that the disease is cancer—the entire gland should be extirpated. If only the apparently affected part be cut away there is a very great danger of its speedy return; for during an operation, when there is a good deal of bleeding going on, and the normal appearance of parts is altered, it is not an easy thing at all times to distinguish with accuracy the healthy from the diseased tissues: a part of the mammary gland may still be contaminated by the scirrhous deposit, and be left behind in this condition. In such a case there will certainly be a speedy return. It is therefore more safe, and more humane towards the patient, to extirpate the entire gland. Cases, of course, will sometimes present themselves where there is only a very small and circumscribed induration, and where it is very possible to remove the disease effectually without amputating the whole breast, but these are not frequently seen, especially amongst hospital patients, as they too often do not apply for advice until the tumor has involved, more or less, the whole of the organ. When, therefore, there are doubts as to the extent of the scirrhous deposit, it will be far better to cut out the whole mamma. Cases are every now and then recorded where operations have been performed for cancer of the breast, and where only a portion of it has been removed. The disease has returned, and the patient has been compelled to submit to a second operation, at which the whole has been removed, and with very good and in some cases lasting success.

However unpleasant the task, and however melancholy it is to reflect that in a large number of cases a return of cancer is to be expected after an operation has been

put in force, it is nevertheless incumbent upon the surgeon to give the patient the chance of an operation, if she is anxious to have it performed, and if the circumstances of the case are in any way favourable for a good result. Operations of this nature are not generally very willingly undertaken by the surgeon, nor are they in any great way creditable to surgery; but it is the duty of the former to give relief even if he cannot cure: and it should be borne in mind that now-a-days, under the use of chloroform, these operations are divested of the greatest amount of horror and aversion by which they were contemplated by the patient. This is an additional argument in favour of operating for cancer of the breast. It was perhaps a question in former days, in a great number of cases at least, as to whether the relief which was given through the use of the knife was a sufficient compensation for the terrible suffering endured from it; but this state of things fortunately no longer obtains, and the surgeon has the consolation of feeling that, although he is performing a disagreeable task in operating for cancer, the proceeding itself is now robbed of the character which but lately formed the main, or one of the main objections to it.

HÔPITAL DE LA CHARITÉ, PARIS.

A Case of Pneumo-Thorax, with Remarks on the Disease. Under the care of M. RAYER.

THE labours of modern pathologists have left few points respecting this serious disease in obscurity. It would seem, indeed, that from the researches of M. Saussier, in 1841, the more recent investigations of Dr. H. M. Hughes (LONDON MEDICAL GAZETTE, 1844), and the thesis of M. Marais, in 1846, that there remains but little to be pointed out of interest or importance regarding it. Nevertheless, as this disease, without being rare, is happily far from frequently met with in practice, it is as well from time to time to publish its more striking instances, in order to familiarize the professional mind with its characters, and to elucidate such questions connected with it as may yet possess some obscurity.

According to the investigations of M. Saussier, the occurrence of pneumo-thorax with phthisis takes place twice as frequently on the left as on the right side. Of eighty-one cases collected by him, this disease was found to exist fifty times on the left side, and once on both sides, which he attributes to the greater frequency and more rapid progress of phthisis in the first

than in the second cases. He admits that perforation may take place where phthisis is but little advanced, if the tubercle be near the surface of the lung. Dr. Hughes, to whom science is indebted for a very remarkable contribution on this subject, states an opposite conclusion—viz. that pneumo-thorax is more frequently met with on the right than on the left side, and, moreover, that it co-exists now and then with a single small pulmonary cavity. M. Chomel goes even farther than the English pathologist, and asserts that it may occur where tubercular disease in the lungs is but very slightly advanced, or is inconsiderable in its extent. It has occasionally happened, M. Rayer added, that opportunity has occurred of confirming the justice of the remarks of the celebrated professor of the Hôtel Dieu.

In the case now under consideration the morbid changes were even less advanced on the side where the pneumo-thorax had occurred; affording a striking illustration of where the truth lies—whether with M. Chaussier, or with MM. Hughes and Chomel; nevertheless further documents are wanting to decide the point.

This disease has usually been considered of so grave a character as to be always fatal. This has been somewhat exaggerated. A young man under M. Gendrin's care was quickly cured by paracentesis, although presenting evidence of phthisis. Its curability is obviously dependent on the closure of the fistulous opening from the lung; if this can be obtained, and if it coincides with only a few tubercles, or a small cavity, there is no reason why the cure should not take place, as we know sometimes happens with tubercular cavities. Let us see, then, if pulmonary fistulae of a tubercular nature be susceptible of obliteration. On this point M. Saussier observes, that these fistulae are only apparently closed, in consequence of the development of a second false membrane more or less thickly deposited over their orifice, and by which communication between the pleura and the cavity is cut off. This question also requires further investigation: in the meantime M. Chassier's solution may be admitted with reservation. Doubts on this point are warranted by three cases related by M. Marais, two of which are published as cases of pneumo-thorax without perforation. Although the autopsy may have been conducted by the most competent persons, it is impossible to admit that pneumo-thorax can be produced in patients whose lungs are healthy, and cavities, at places had simply existed in the

third case, where the adhesion of the membrane was so firm that inflation of the lungs made no impression upon it. In other cases the healing of the fistulae must be admitted.

In the following case death took place so rapidly that treatment was useless:—

A man, aged forty-two years, was received in the hospital of La Charité (Saint Michael's ward, No. 33), who had for several years presented signs of phthisis. While under M. Rayer's care the existence of a cavity was ascertained at the upper part of the right lung; under the left clavicle only dulness on percussion and deficient respiratory murmur were discovered. In the night of the 5th of November he suddenly experienced severe pain at the lower part of the left side of the chest. The expectoration, which had before been abundant, ceased from this time. A tympanic sound was audible in the lower part of the chest on the left side, with enlargement of the thorax at that part. The respiratory murmur was replaced by an amphoric sound, accompanied frequently with slight metallic tinkling. It was not possible by succussion to produce thoracic fluctuation. Death took place on the evening of the same day.

On examination of the body, the right lung was found riddled with tubercles, hard and soft, with close adhesion of the surfaces of the pleura. On puncturing the left side of the thorax an escape of gas took place, producing its characteristic whizzing sound: the left lung was pressed back against the vertebral column, and was only slightly adherent at its upper part. By inflating the lungs, a perforation of about two millimetres (= .7874 Eng. inch) was found at the upper part of the lower lobe, which communicated with a small cavity; this, at its outer side, had been only separated from the cavity of the pleura by the serous membrane.

A child was brought to the hospital in August 1849: it was then three weeks old: the mother had perceived some hardness about the neck for several days, of which she took little notice until the skin began to look red. Upon examination, the sterno-cleido-mastoidous on the right side, especially in its lower half, was hard and well defined: the child evinced considerable uneasiness upon manipulation: the surface was red, and the heat of the skin increased, with marked constitutional symptoms. An evaporating lotion, with poultices at night, and alterative doses of Hyd. c. Creta, were prescribed. The application of leeches was suggested, but deferred on account of the age of the child. In two or three weeks it was so much better that a liniment was ordered, and the case marked off the book. Upon subsequent examination it was found that no induration or perceptible change remained. The child, a male, presented with its breech, was above the average size, and more than usual difficulty was experienced in its extraction. The woman's two previous labours had been of a similar character: the children were both born dead: she was attended by an experienced practitioner, who considered the brim of the pelvis small.

The affection at the time was considered to have arisen from injury received in the birth.

I should scarcely have considered it worth narration but for the mystery thrown over it by Mr. Curling's note. I make no doubt similar cases arise from violence applied in one way or other. The exact condition of the muscle is not so easily ascertained, as it is not very likely to fall under the scrutiny of the pathologist. If this is sufficient to remind some of my fellow junior practitioners of the sage aphorism about meddling midwifery, my purpose will be abundantly served.

J. BASSETT,

Late Resident Medical Officer to the Birmingham Lying-in Hospital.

Great Hampton Street, Birmingham.

Correspondence.

ON INDURATION OF THE STERNO-CLEIDO MASTOID—LETTER FROM MR. BASSETT.

SIR,—The communication of Mr. Curling in your last number, respecting induration of the sterno-cleido-mastoidous muscle, caused me to refer to my note-book, in which I find an account of a case which occurred during my residence at the Lying-in Hospital in this town, of, I apprehend, a similar nature, except that it was in an inflammatory condition, the cause appearing quite obvious.

ON INDURATION OF THE STERNO-CLEIDO MASTOID IN INFANTS.

SIR,—In your last number there is a short paper from Mr. Curling on Induration of the Sterno-cleido-mastoidous muscle in Young Infants, in which he states that the left muscle was affected in all the cases he saw, and throws out a suspicion that it arose from some injury during its birth. The following case will, I think, render it very probable that Mr. Curling's suspicion as to the cause is correct: at the time of its occurrence I had no doubt that laceration of some of the fibres of the muscle

was the cause, and mentioned it to the parents; but in this case the right sterno-cleido-mastoideus muscle was the seat of the affection.

A lady in labour with her first child, which presented with the breech, progressed slowly but favourably till the expulsion of the breech, when, after waiting some time, and no progress being made, I brought down a fold of the funis, and found the funic arteries beating very quick and intermitting. For the safety of the child, it was necessary to expedite the labour. I drew down the body as far as I could, disengaged the arms, and then proceeded to deliver the head: it was situated high up, and required its direction to be changed: in doing this, difficulty was experienced, but the child was speedily born alive. Upon examining the child a short time after its birth I discovered a small swelling in the situation of the middle third of the right sterno-cleido-mastoideus muscle: on slight pressure the child showed signs of uneasiness. The impression on my mind at the time was, that in the forced expulsion of the head a few fibres of the muscle were lacerated, and perhaps a little blood effused at the same time. The next day the swelling continued, and the muscle was found hard, defined, and painful, and the child naturally kept the head to the right side. Fomentations were applied. This state continued for many weeks, when the induration gradually subsided; but the swelling continued for eighteen months after birth, and then by degrees disappeared. I saw the child a day or two ago, now upwards of three years of age, and there is no difference between the two muscles.

Yours respectfully,

SAMUEL BERRY,

Professor of Midwifery, &c., at Queen's College, Birmingham, and Surgeon-Accoucheur to the Queen's Hospital.

Birmingham, Nov. 27, 1850.

Medical Intelligence.

UNIVERSITY OF LONDON.

M.D. EXAMINATION—1850.

Examination for Honours.

Monday, November 25.—Morning, 10 to 1.

Elements of Intellectual Philosophy, Logic, and Moral Philosophy.

Examiner, Mr. BURMAN.

1. "There are and can be but two ways of investigating and discovering truth."

What are these two methods, and what is the difference between them as regards the process of the mind?

2. One of the operations which the word Induction has been used to designate, is "the *material* illation of a universal from a singular, as warranted either by the general analogy of nature or the special presumptions afforded by the object-matter of any real science." Show that this is the process of Bacon, and distinguish between it and the Aristotelian Induction.

3. Enumerate the causes or sources of the *Idola Tribus* and the *Idola Theatri*.

4. Give Stewart's definition of consciousness. How, according to him, do we get the notion and conviction of our personal identity? Give any other solution of the question.

5. "It is to the use of artificial signs that we are indebted for all our general conclusions, and without it our knowledge would have been entirely limited to individuals." Give reasons for or against the above proposition.

6. "To what part of our constitution is the origin of our ideas of right and wrong to be ascribed?" How is this question answered by Hobbes, Cudworth, Locke, and Hutcheson? How do you answer it yourself?

Cousin and Butler.

Examiner, Rev. H. ALFORD.

1. What, according to Locke, are the sources of all our ideas? How does he on his theory account for the ideas of space, time, infinity, personal identity, substance, causality?

2. What fundamental error pervades the whole of this part of Locke's system? How has it been carried out since his time, and by whom? What are, according to Cousin, its principal merits and demerits?

3. Give Cousin's refutation of Locke's view, that all judgment is founded on comparison.

4. What are the presumptions that we shall live after death, deduced from our present physical, intellectual, and moral state?

5. How is God's government by punishments justified by analogy? "In the natural course of things, virtue *as such* is rewarded, and vice *as such* punished." With what exception is this true? What may be inferred from the rule, and what from the exception?

6. Show that the government of God must be a scheme incomprehensible to us at present.

[To be continued.]

ANNIVERSARY MEETING OF THE ROYAL SOCIETY—ELECTION OF OFFICERS.

THE anniversary meeting of this Society was held on Saturday last, the 80th ult., being St. Andrew's day. There was a good attendance of Fellows, among whom we observed the Marquis of Northampton, Chief Baron Pollock, Sir R. Inglis, and Professor Faraday.

The President, the Earl of Rosse, read the annual address, which gave a retrospective view of the progress of scientific discovery during the past year, chiefly in the sciences of physical astronomy and mathematics. It adverted to the deaths of two eminent foreign Fellows—namely, MM. Gay-Lussac and Blairville. After the reading of this address, the Copley medal was awarded to Professor Haunsteen, of Denmark, for his researches in astronomy; the Rumford medal, to M. Arago, for his numerous discoveries and improvements in optical science. The Royal medals were awarded to Professor Graham, of University College, for his contribution to the Transactions of a paper on the Motion of Gases through Tubes; and to Mr. Brodie, son of Sir Benjamin Brodie, for his paper on the Chemical Constitution of Wax.

An obituary memoir of Gay-Lussac was read in its original French by Mr. Christie, one of the secretaries.

Lord Northampton moved a vote of thanks to the noble President for his valuable address, and a resolution to the effect that it should be printed for the instruction of the members. This was seconded by Sir R. H. Inglis, and carried unanimously.

The balloting for officers for the ensuing year was commenced, and the following noblemen and gentlemen (constituting what is termed the House list) were elected:—

President.—The Earl of Rosse, K.P., M.A. *Treasurer*.—Lieut.-Col. E. Sabine, R.A.—*Secretaries*.—Samuel Hunter Christie, Esq., M.A.; Thomas Bell, Esq., Foreign Secretary.—*Captain W. H. Smyth, R.N.* *Other Members of the Council*.—John Joseph Bennett, Esq.; William Bowman, Esq.; Sir Benjamin Collins Brodie, Bart.; The Rev. Professor Chalkis, M.A.; Lt.-Gen. Sir H. Douglas, Bart., G.C.B.; Sir P. de Malpas Grey Egerton, Bart.; John Forbes, M.D.; Marshall Hall, M.D.; Gideon A. Mantell, Esq., LL.D.; Professor W. Hallowes Miller, M.A.; Sir R. Impey Marchison, M.A.; Richard Phillips, Esq.; Rt. Hon. Sir Frederick Pollock, M.A.; George Rennie, Esq.; Edward Solly, Esq.; Lord Wrottesley.

The Fellows whose names are in italics were not members of the last council.

The funds of the Society appear to be in a flourishing condition. During the last seventeen years there has been a steady increase in the annual contributions from Fellows. We subjoin an account of the contributions during the last ten years:—

1841	815	12	0
1842	910	8	0
1843	933	16	0
1844	1025	16	0
1845	1010	0	0
1846	1074	0	0
1847	1116	8	0
1848	1122	16	0
1849	1130	16	0
1850	1146	4	0

The total receipts during the past year, exclusive of the Balance, amounted to £3176. 19s. 6d.

The Expenditure during the same period, exclusive of the sum of £600 invested in the Funds, was £3000. 5s. 7d.

Under the head of *Donation Fund*, the Society has paid £250 during the past year to the under-mentioned gentlemen:—

Donation Fund.—Dr. Hofmann, for Chemical Investigations, £100; Mr. Miller, for Meteorological Observations, £50; Mr. Newport, for Physiological Investigations, £50; Dr. Frankland, for Chemical Investigations, £50.

The oppressive character of the Income tax is shown in this as in other scientific societies. Upwards of forty pounds per annum are paid for this impost out of the funds devoted to *scientific* purposes! In a revision of this most unjust and oppressive, tax we consider that the property of all societies whose funds are devoted to the purposes of science, should be exempted.

BOTANICAL SOCIETY OF LONDON.

THE fourteenth anniversary meeting was held on Friday evening last at the Society's rooms, 20, Bedford Street, Strand, Mr. J. E. Gray, F.R.S., president, in the chair. Mr. G. E. Dennes, the secretary, read the report of the Council, from which it appeared that fifteen new members had been elected since last year, and that the Society consisted of 250 members. Many thousands of specimens of British and foreign plants had been distributed to the members, and increased exertions had been made this year to extend this important department of the Society's operations. The report was unanimously adopted, after which a ballot took place for the Council, when the president was re-elected, and he nominated Mr. J. Miers, F.R.S., and Mr. A. Henfrey, F.L.S., vice-presidents. Mr. J. Reynold and Mr. G. E. Dennes were elected treasurer and secretary. The members afterwards supped together at

he Sussex Hotel, Bouverie Street, Fleet Street.

LAW OF LUNACY.

WE understand that, at the instance of an influential member of the American Government, Dr. Forbes Winslow is actively engaged in preparing an analysis of the English Law of Lunacy. Dr. Winslow is requested to make any suggestions that may occur to him in the course of the inquiry, relative to an amendment of the law. We trust, when Dr. Winslow's labours are brought to a conclusion, that the results of the investigation will be submitted to the British Government, in order that they may adopt any valuable suggestion it may contain for an amendment of our own defective laws, in regard to the arrest and detention of persons alleged to be insane.—*Morning Chronicle*.

CHARING CROSS HOSPITAL.

DR. ROWLAND has been recently elected Assistant-physician to this Hospital.

MEDICAL STUDENTS IN PARIS AND LONDON.

L'Union Médicale reports that there has been a great increase this year in the number of students attending the medical schools of Paris. In 1849 there were 880 inscriptions only: in 1850 the number has risen to 1223. The registrations in London for the present session are said to amount to 1035—a number which is larger than the average.

APOTHECARIES' HALL.

NAMES of gentlemen who passed their examination in the science and practice of medicine, and received certificates to practise, on Thursday, 28th November, 1850:—William Frederick Bassett; Mark Rowland Day, Oxford; George Frederick Gwyn, Long Stratton, Norfolk; Charles William Hammond, Ipswich.

OBITUARY.

At St. Omer, on the 24th ult., Maitland Maitland, M.D., in the 91st year of his age.

Selections from Journals.

CASE OF DISEASE OF THE LYMPHATICS. BY DR. FETZER, STUTTGARD.

ALTHOUGH cases are on record in which, as the consequence of wounds, the fluid of the lymphatics has been seen to flow directly from those vessels, observations are wanting of the spontaneous affection of one or more lymphatic vessels attended

with escape of their lymph. The following case is therefore interesting, and the more so as it is authenticated by Prof. Schlossberger's analysis of the fluid:—

A girl, 16 years of age, had not menstruated; she was otherwise in good health, but had been the subject of double crural hernia since the age of eight years. In the course of the year 1847 she noticed several small mamillary prominences on her abdomen. She, however, took no notice of them, until, towards the end of Jan. 1847, she was persuaded to consult Dr. Fetzer. On examining the abdomen, there was discovered a long brownish-coloured stripe, about three fingers in breadth, extending to an inch below the umbilicus to the left of the middle line, and upwards and backwards towards the dorsal vertebra, but gradually decreasing in size and colour. About an inch below the umbilicus, and two lines to the left of the linea alba, was situated a mass of tumors, eighteen in number, of the size and shape of the female nipple; their colour was the same as that of the surrounding integuments, insensible to the touch, and it could easily be made to disappear by pressure; but they quickly reappeared on removal of the pressure. The patient had for a long time experienced slight lancinating pains in the course of the strip of discoloration, but had not paid any attention to them.

In July, whilst out walking, and being in great haste to return home, a quantity of fluid escaped from the surface of the abdomen. This fluid, at first milky, became grumous. Three days afterwards this flow returned, and continued three days. A milky fluid was seen to ooze drop by drop from the centres of two of the largest tumors. According to the mother's testimony, the girl had lost at least a litre (= 1.76 Eng. pint) of fluid. By compression of the tumors the quantity could be increased. By the aid of a lancet a minute red point moistened with fluid could be detected on each nipple. The fluid, when collected, separated, at longer or shorter intervals, into a turbid milky serum, and a clear, tolerably abundant coagulum, which became reddened by exposure to the air.

Dr. Fetzer removed one of these tumors with a pair of scissors: it was formed of the layers of skin somewhat thinned. The duct being open, a probe could be passed in to the distance of an inch, either right or left. A quantity of fluid flowed from the orifice, which neither pressure nor strong solution of alum arrested, and which yielded only to the repeated application of nitrate of silver. It was of importance to arrest the flow, as the patient felt weakened thereby, and the pulse was becoming smaller. After the eschar formed by the nitrate of silver

had been cast off, the former tumor remained, and a number of smaller growths, varying from the size of a millet seed to that of a lentil, also appeared.

The fluid which exuded presented all the characters of lymph. It was constantly of a turbid milky aspect, whether collected after a meal or fasting. The turbidity was owing to the presence of fatty particles: ether rendered it clear.

The tumors were probably dilatations of the lymphatic vessels consequent on obstruction of a trunk—the effect of slight inflammation indicated by the previous lancinating pains. Examined microscopically, a great number of fat-granules were observed in the liquid, having an active molecular movement. Lymph-globules, nearly spherical, slightly yellowish, and containing nuclei of various sizes, were also seen; also unnnucleated corpuscles, fewer in number, less regular, of a yellow colour, smooth surface, seeming to constitute a transition state to blood-globules, and to which might be owing the reddening of the liquid on exposure to the air.

Prof. Schlossberger's analysis gave for the composition of 1000 parts of the blood—

Water	930.68
Albuminous substances	47.26
Fat	0.29
Extractive	8.80
Salts	9.54

—*L'Union Médicale.*

x

BOOKS & PERIODICALS RECEIVED FOR REVIEW

DURING THE LAST TWO WEEKS.

The Correlation of Physical Forces. By W. R. Grove, M.A. F.R.S. &c.

The Fifth Annual Report of the Commissioners in Lunacy presented to the Lord Chancellor, June 1850.

The New York Medical Gazette. Nos. 11 to 16. (N.B. Nos. 3 to 10 inclusive have not reached us.)

London Journal of Medicine. Dec. 1850.
Edinburgh Monthly Journal of Medical Science. December.

Pharmaceutical Journal. December.

Comptes Rendus. No. 20; Nov. 11.

Casper's Wochenschrift. Nos. 42, 43, and 44; 19th October to the 2d November.

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer	29.59
Thermometer	40.5
Self-registering do.	Max. 22°
Min. 22°	
From 12 observations daily.	Sum.

RAIN, in inches, 0.68.—Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was 2° below the mean of the month.

BIRTHS & DEATHS IN THE METROPOLIS During the Week ending Saturday, Nov. 30.

BIRTHS.		DEATHS.	
Males....	773	Males....	483
Females..	709	Females..	408
	1482		891

CAUSES OF DEATH.

ALL CAUSES	861
SPECIFIED CAUSES	860
1. <i>Zymotic (or Epidemic, Endemic, Contagious) Diseases.</i>	196
<i>Sporadic Diseases, viz.—</i>	
1. Dropsy, Cancer, &c.	41
2. Brain, Spinal Marrow, Nerves, and Senses	116
4. Heart and Bloodvessels.	35
5. Lungs and organs of Respiration	160
6. Stomach, Liver, &c.	49
7. Diseases of the Kidneys, &c.	7
8. Childbirth, Diseases of Uterus, &c.	13
9. Rheumatism, Diseases of Bones, Joints, &c.	13
10. Skin.	0
11. Premature Birth	27
12. Old Age	24
13. Sudden Deaths.	3
14. Violence, Privation, Cold, &c.	24

The following is a selection of the numbers of Deaths from the most important special causes:

Small-pox	14	Convulsions.	35
Measles	25	Bronchitis	64
Scarlatina	32	Pneumonia	67
Hooching-cough	33	Phthisis	96
Diarrhoea	13	Lungs	3
Cholera	0	Teething	11
Typhus	47	Stomach	3
Dropsy	12	Liver	13
Hydrocephalus	19		
Apoplexy	21	Childbirth	7
Paralysis	22	Uterus	3

REMARKS.—The total number of deaths was 305 below the average mortality of the 48th week of ten previous years.

NOTICES TO CORRESPONDENTS.

We shall have great pleasure in giving insertion to Dr. F. J. Brown's case of Softening of the Cerebellum. It shall appear, if possible, next week.

Mr. R. Oke Clark's communication shall have early insertion.—Owing to the comparatively small number of Subscribers, the plan of publishing a General index has been for the present abandoned.

Dr. J. Pollock's second paper has been received. The first part of Dr. W. S. Kirkes' contribution will be inserted in the following number.

We regret that we are obliged to postpone Mr. Gallwey's paper, and the Guy's Hospital Report, until next week.

The notice of the Reports on Cholera in the Yorkshire Asylum is written, and will shortly be published in an article with reviews of sixteen other works on Cholera. This will explain the delay in noticing this and several other essays on the disease.

Mr. T. W. Nunn.—The report was received too late for this week's number. A private letter will be sent.

RECEIVED.—Mr. Huxley.—Dr. Wright.

Lectures.

CLINICAL LECTURE

ON

ASTHMA.

(Delivered at King's College Hospital, Oct. 18, 1849.)

By R. B. Todd, M.D. F.R.S.

Physician to the Hospital.

[Reported by H. H. Saltzer, M.B., Dem. Anat. K.C.L.]

LECTURE XV.

I SHALL call your attention to-day, gentlemen, to the subject of Asthma; and I shall take, as the text of my remarks, the case of John Shaw, who, you may remember, was admitted into the hospital on the 4th of this month (Oct. 1849), labouring under a very violent asthmatic paroxysm, but who is now, I am happy to say, quite well.

As the case is given very concisely, and yet accurately, I may as well read it to you:—"The patient, by occupation a cab-driver, states himself always to have been a tolerably healthy man, with the exception of two attacks of fever, the last of which was very severe, and for which he was treated some months ago in this hospital. Soon after leaving the hospital he returned to his ordinary occupation, which necessarily exposed him to every vicissitude of weather, and this he makes an excuse for having drunk very freely of rum, and other spirits.

"For the last two or three months he has been gradually becoming asthmatic, having at all times more or less difficulty of breathing, the dyspnoea, however, being aggravated greatly by any exposure to wet or cold, and the paroxysms of dyspnoea always ending in violent cough and expectoration.

"On October 3d he drank pretty freely of spirits, and was greatly exposed on a very wet day—one of those wet days which prove a harvest to the London cabmen: he went to bed early, but found himself unable to lie down, and therefore passed the night sitting up; and, in spite of this position, much troubled with difficulty of breathing. At last he fell asleep, but was awake early the next morning by a fresh paroxysm of asthma. The dyspnoea greatly increasing, he was brought to the hospital at five o'clock in the morning.

"Upon examining his chest it was found

to be large and barrel-shaped, very resonant on percussion at all points, even over the cardiac region; the heart could be felt and heard beating at the scrobiculus, and only in this region or a little to the left of it, but not in the normal situation; very distinct rhonchus could be detected over the whole of the chest, and at some points sibilus could be distinguished. The heart could only be heard in the epigastrium: he expectorated a good deal of thick, viscid mucus, but not without much difficulty and cough." So far for the report.

At two o'clock, when I came to the hospital, I found him sitting up, suffering from extreme dyspnoea, blue in the face, with all the ordinary and the accessory muscles of respiration working actively; and I then determined to try the effect of chloroform, and applied it at once, not pushing the administration of it so far as to procure complete insensibility. After a few whiffs he experienced an evident relaxation of the asthmatic spasm; he breathed slower and easier; his pulse became slower and fuller; and, from that moment, the paroxysm seemed to give way.

After the influence of the chloroform passed off, the dyspnoea returned, although in a much less degree, but only for a very short time: he passed the remainder of the afternoon without suffering, and slept well.

The next morning (Oct. 6th) the following report is made:—"He is decidedly better, has expectorated pretty freely, breathes much more easily, and his pulse has fallen from 104 to 86."

From this time he has had no bad symptoms, and he is now convalescent, and may leave the hospital in a day or two.

Such is the case on which I propose to found some remarks on the nature and treatment of asthma: and first, as to the nature of this disease.

What is asthma?—I shall answer this question by stating first that it cannot be properly called a disease of the lungs—in other words, its primary seat is not in the lungs: it may be defined to be a constitutional disease, which manifests itself by paroxysms of difficult breathing, with intervals of various duration, in which the patient is completely or nearly in a healthy condition.

Let us look at the more prominent points in the clinical history of asthma, and inquire how far we may found upon them an explanation of its pathology.

The way in which the uncomplicated asthmatic attack commences is generally this. A patient, we will say, goes to bed quite well: soon afterwards he finds a difficulty of breathing come on; he cannot lie down; he cannot

go to sleep; the dyspnoea increases, and the attack becomes confirmed: or, what is very often the case, he goes to sleep quite well, and wakes in an hour or two with the attack on him. When ones formed, the asthmatic paroxysm continues for some time, and passes off generally with some cough and expectoration, but sometimes without either. The time of its duration is very variable: sometimes it lasts only a few hours, sometimes many days.

When suffering from the attack the patient cannot lie down. All asthmatics show an instinctive repugnance to the horizontal posture while the attack is on; and even in the intervals of the attacks they like to lie high, and seldom lie quite flat: generally they are content with sitting up in bed or on a chair, or they may sometimes move about the room. The coachman of a neighbour of mine some years ago suffered so much from a paroxysm of asthma, which resisted all treatment, that he was obliged to stand leaning forward on a table for three days; and in this erect posture he passed the whole of this time: at last he became so exhausted that he was obliged to sit down from sheer inability to support himself. The erect or semi-erect posture is no doubt preferred because it enables the muscles of respiration to act with greater freedom, and with more mechanical power.

These attacks are very much influenced by weather, particularly cold and damp, and by locality, as high or low, humid or dry, relaxing or bracing: and there is a remarkable capriciousness in these respects, some persons liking a low, damp, smoky situation, some a high, dry, and clear; that which is fatal to one case will be the very best thing to another, and one person will be well where another cannot live. Sometimes persons living in London, and wishing to try the neighbourhood, will ask you, "Shall I go to Clapham, or shall I go to Highgate—will a high or a low situation be the best for me?" Now this is a question that it is impossible to answer with certainty; for very often that which would seem the worst will turn out the best. I have known asthmatics better on the banks of the Thames than anywhere else; on the other hand, I have known some greatly relieved by going to a high situation. Dr. Watson has some interesting remarks on this subject in his valuable Lecture on Asthma, and relates some curious cases in illustration of the uncertain influence of locality in checking or promoting the asthmatic paroxysm.

Then, sometimes we find that asthma is brought on by certain specific irritating agents. We all know of *hay-asthma*: that if certain individuals venture within the

range of a hay-field, they are seized with sneezing, coryza, profuse lachrymation, and other symptoms of irritation of the mucous membrane, accompanied with a distinct asthmatic paroxysm. Similar symptoms are brought on in other people (but such persons are much fewer) by the volatile effluvia of ipecacuan, or by the presence of very fine particles of dust floating in the atmosphere.

Persons having had this disease for many years may cease to have any attack. I have known many cases of men and women who have lost the disease as they advanced in life. Now, in such cases of recovery, does the disease leave the lungs perfectly healthy? Not unless the attacks of asthma have been mild, and few and far between. Although not necessarily originating in the lungs, the frequent repetition of the asthmatic paroxysm doubtless always leaves some injury to the lung after it, which is the greater in proportion as each paroxysm is longer and more severe.

In asthma the respiratory efforts are greatly exaggerated, in consequence of the increase of the sensation of the want of breath—the *besoin de respirer* of the French; a sensation which any one may easily experience in his own person by simply putting his face into water for a few seconds. Under the influence of this strong feeling of the want of breath, large quantities of air are drawn in, and so great is the effort of inspiration that the noise which it occasions may be heard for a considerable distance. What results? The air-cells of the lungs become dilated, and the whole lung experiences a proportionate enlargement, and the external configuration of the chest is altered. This is well exemplified in our patient Shaw: he has a large, barrel-shaped chest, rounded in front, behind, and at the sides; and, if you watch him breathe, you will see that there is less movement at each respiratory act than there should be; the chest is in a state of permanent dilatation, the ribs are too horizontal, the intercostal spaces too wide, and the accessory muscles of respiration are always more or less in a state of action. The state of lung which is produced by asthma is that which has been called by Laennec *emphysema*—a name not happily chosen, as it implies what really does not exist, namely, the infiltration of the lung with air, the existence of air in its extravascular tissue. If the asthmatic attacks continue for any great length of time, and are severe, we find further injury of the lung taking place; the walls of the air-cells suffer in their nutrition, and some of them, already dilated to their utmost extent, give way; three or four, or more, become fused into one, and form large irregular cavities,

which are distributed among the healthy pulmonary tissue.

But, besides these changes in the lungs, the repetition of the asthmatic paroxysms leads with equal certainty to morbid changes in the heart. From the obstruction that is offered to the circulation through the lungs an undue amount of work is thrown on the right ventricle, which of course becomes more or less hypertrophied; at the same time the meshes of the pulmonary capillaries become enlarged, and no doubt experience some change in their vital properties whereby the circulation in them is retarded. This affords an additional means of obstruction through the lungs, and therefore an additional cause for hypertrophy of the right side of the heart. But as the backward pressure of the blood on the heart becomes increased, the right ventricle becomes not only hypertrophied, but dilated, and the dilatation extends in a retrograde course to the auricle, and thence to the large veins, so that in the advanced stages of this disease it is not uncommon to find venous regurgitation, and more or less congestion, throughout the whole venous system. In the early stages, however, none of these conditions will exist. You may have the most exquisite asthmatic dyspnoea without its leaving any perceptible deviation from the healthy standard either in the heart or lungs, or at most no more than may perfectly recover itself when the paroxysm passes off. This is more likely to occur in children, because their tissues have a greater power of recovery from their greater activity of nutrition.

Such cases as these would alone be sufficient to prove that asthma is a disease essentially independent of any organic lesions of heart or lungs, though frequently accompanied by them, aggravated by them when they exist, and always inducing them if it is of sufficiently long continuance. And it is remarkable how soon these lesions may be thus induced, how short a continuance of asthma will be sufficient to give rise to evident signs of organic change in the heart and lungs. Our patient Shaw had had asthma not quite three months, and for the greater part of the time not severely, and yet we find him with a barrel-shaped, unnaturally resonant chest, dilated thoracic parietes, and the heart so displaced and dilated in its right cavities as to beat in the region of the scrobiculus cordis.

I look upon this last sign as one of the most characteristic symptoms of asthma, and I consider its presence in any case where I suspect asthma, as a clear confirmation of the correctness of those suspicions. In accordance with this view, in examining a patient whom I suspect to be asthmatic, one of my first steps is to apply my finger

to his scrobiculus cordis: if I find no beating of the heart there, my conclusion is a contingent negative; but if I find it beating there; and not in its natural position under the nipple, my conclusion is a certain affirmative.

If you trace up the disease to the point of its first appearance, you will generally find that the first attack came on either without any assignable cause, or after some indiscretion as to diet—or after some imprudent exposure to weather; the patient went to bed well, and in every respect in his usual health, and woke up asthmatic,—but once having made its appearance, it renders its victim ever after liable to its recurrence.

A remarkable circumstance is, that it is often inherited; the father or mother have had it, or it may have left over a generation;—the grandfather may have been asthmatic, and the intervening generation not so. Once that the asthma has fairly established itself in any individual, it may be brought on by any slight cause, even the most trivial disturbance will be sufficient to excite it,—catarrh, indigestion, irregular hours, mental excitement, violent exercise, change of temperature, change of place—any one of these may bring on an attack. Now, if you inquire narrowly, you will generally find at the root of the disease some fault in the primary assimilation: you will find that the patient has learned to avoid certain things; that he is not at liberty like other men; there are some things of which he dare not partake, or, if he does, it is done at the price of an attack. And you will likewise find, on looking into the patient's secretions, that they are altered, that the urine is lithic, phosphatic (most frequently the former), or presents some deviation from the healthy standard. Very often, too, you will find the attack ushered in by a peculiar condition of the urine, either such as I have just mentioned, or urine resembling nervous hysterical urine, abundant, clear, and pale, and of very low specific gravity.

Now all these points,—the periodical recurrences of the attacks, the perfect, or nearly perfect health in the interval, the absence of any organic change, the associated humoral disturbances,—all bear in the most interesting way on the pathology of this malady. They tend to establish a remarkable analogy between asthma, gout, and some other diseases. As in asthma, gout comes on quite suddenly—there is no warning: a man may go to bed quite or nearly well, and he will wake up early in the morning with a fit of the gout in his great toe. There is another disease, epilepsy, in which we have exactly the

same phenomenon: a patient, with or without warning, falls down foaming, livid, and convulsed; the paroxysm goes off, and leaves him in his ordinary good health, and he may go on for years and not have another. Again, we know that a fit of the gout leaves no organic lesion if it occurs once or twice; but if it is often repeated it leaves permanent injury in the joints that it attacks. We may observe the same with respect to epilepsy. If a patient has suffered only one or two attacks, you will find no change in his brain; but if he has had several, you will. The disease evidently consists essentially of something *attracted* to the brain, and not *existing* in it. The same, too, of asthma, the organic changes are all secondary, and a few attacks leave no traces behind them.

All this leads us to suppose that the paroxysm of asthma has something in common with the paroxysm of gout and the paroxysm of epilepsy. Of the two I prefer to take the analogy to gout, because we have more definite and coherent ideas about gout, and we are more acquainted with its exact pathology. The theory at present most in favour with regard to gout is that it is a disease of assimilation, and this defective or vitiated assimilation gives rise to some *materies morbi*. When this matter is eliminated from the system, the attack passes off; when it accumulates, the attack comes on. In asthma defective assimilative power is a frequent coincident. Gout, too, and rheumatism, and all humoral diseases, resemble asthma in being inherited.

When the *materies morbi* of asthma has been generated its effect is to irritate the nervous system, not generally, but certain parts of it, those parts being the nerves concerned in the function of respiration—viz., the pneumogastric, and the nerves that supply the expiratory muscles, either at their peripheral extremities, or at their central termination in the medulla oblongata and spinal cord; extreme difficulty of breathing is the result, and, as a consequence of this, ultimate disease of the lungs.

Thus the occurrence of asthma in paroxysms would be accounted for much in the same way you would explain the occurrence of gout in paroxysms; and in the intervals between the attacks, the patient, being of asthmatic constitution, so to speak, is easily thrown into the paroxysm of asthma by causes which would but slightly influence other men,—as cold, impure air, mechanical irritation of the respiratory passages,—just as gouty men may readily be thrown into the gouty paroxysm by causes comparatively trivial.

This seems to me to be the most reasonable exposition of the pathology of asthma.

Now we often hear physicians of great and deserved repute speaking of *spasmodic* asthma. I need not tell you that the bronchi possess a muscular coat, consisting of circular fibres of the unstriated for it has been long proved, not only by microscopical observation, but by the most satisfactory experiments. It is in these circular muscular fibres of the bronchi that many pathologists localize the spasm, to which they ascribe all the phenomena of asthma.

The first link in the chain of effects of the immediate exciting cause of asthma would be, according to them, spasm of the bronchial tubes, then dyspnoea. Undoubtedly a state of spasm of the bronchial tubes would produce a great deal of dyspnoea; but what I want to point out to you is, that this state of spasm of the bronchial tubes ought rather to be regarded as one of the accompaniments, one of the phenomena of asthma, than as its cause. The feeling of breathlessness, or, in other words, a peculiar state of certain nerves and of a certain nervous centre, the centre of respiration, is the first link in the chain of asthmatic phenomena. The spasm of the bronchi follows sooner or later upon this, and often it follows so quickly upon it as to appear to come simultaneously with it: does it ever precede it? I doubt this.

Undoubtedly you may have severe asthma without severe spasm of the bronchial tubes. I remember a well-marked instance of this in a gentleman whom I attended for chronic disease, cancer, as I thought, of the liver. For nearly a week before his death he suffered from the most frightfully distressing asthma, which nothing could control, and which lasted without interruption till he died. I examined his chest repeatedly at all parts, and could hear nothing but the most perfect, loud, and puerile breathing, which is quite inconsistent with a state of spasm.

Again; section of the vagi nerves of animals produces phenomena exactly like those of asthma. Whatever be the cause of the dyspnoea in these cases, it is clear it cannot be bronchial spasm, as the muscles of the bronchi would be paralysed after section of their nerves.

There is one thing that I have observed, which has an important bearing on this subject; that in that particular form of spasmodic disease which I have no doubt in your future practice you will have many opportunities of witnessing, laryngismus stridulus, the crowing inspiration of children, there is a ronchus all over the chest, simultaneous with the convulsive attack.

The moment the convulsion comes on, when the eyes become fixed and the child begins to inspire with difficulty, if you put your ear to the chest you will hear a ronchus pervading the whole lung. The moment the recovery takes place, the ronchus ceases. And it is very interesting to notice that you may observe the very same thing in the ordinary convulsions of children, in which the larynx is not prominently engaged; and I suppose no one will pretend to locate these diseases anywhere but in the nervous system.

Hence the conclusion that I draw is this, that the spasm is the accompaniment, and not the cause of the difficult breathing that accompanies or follows upon the nervous changes, just as it does in laryngismus and in the ordinary convulsions of infants.

So much for the pathology of asthma; as to the treatment of this disease our attention must be directed

1. To obtain relief to the paroxysms, and
2. To improve the patient's constitution in the intervals of the paroxysm.

First, in the asthmatic paroxysm you must inquire into the immediate exciting cause, and endeavour to remove it. With this view an emetic, by emptying the stomach, will often be found useful; or if the quality, not the quantity of the contents of the alimentary canal is the source of the evil, an alkali will be found beneficial. Stimulants are sometimes of essential service, especially when the long-continued circulation of imperfectly arterIALIZED blood has deadened the sensibilities, and so far lowered the powers as to render the expectoration of mucus difficult: one of the best stimulants is sulphuric ether, or perhaps the chloric is even better, from its taste and smell being so agreeable. Ether, in combination with opium, will often be found of great service. But perhaps the *sedatives* constitute the class of remedies that has met most favour in the treatment of asthma, and especially the different members of the order *Solanaceae*,—as *Hyoscyamus*, *Belladonna*, *Tobacco*; but before and above all, both in the frequency of its employment and its real value, *Stramonium*. This medicine may be given in various forms,—as an extract of the seeds, in doses of from gr. $\frac{1}{4}$ to gr. $\frac{1}{2}$, or in the form of a tincture, in from ten minims to half-drachm doses; but more frequently it is smoked, and for this purpose the whole plant, leaves and stems, are used; it is dried and cut into small pieces, and smoked in a pipe, or the leaves alone are dried, and rolled up in the form of a cigar. I here show you some of these cigars, which have been put into my hands by Mr. Sa-

vory, of Bond Street; but I must tell you that these are not, strictly speaking, *Stramonium* cigars, being made, not of the *Datura Stramonium*, but of the kindred species, the *Datura Tatula*, which is said to be even more efficacious than the more generally used species.

Lastly, we find a valuable remedy for the asthmatic paroxysm in chloroform: we know how in other cases it influences all those functions which are ministered to by nervous influence, and in its efficacy in asthma we have an interesting physiological experiment, and an important lesson with regard to the true pathology of the disease. But in the administration of chloroform I would give you this two-fold caution:—first, to give it gradually and cautiously, and not in a full dose; not to produce insensibility, especially if there be anything like blueness of the surface, because, though remedial to the asthma, it will tend to increase those very consequences which are most to be feared from the circulation of venous blood. Secondly, to impress upon your patient that he must never give it to himself, nor without the presence of a medical man. This case was related in the papers the other day: A person who was in the habit of curing his attacks of asthma by inhaling chloroform, when administering it to himself one day, and when in a state of half subjection to its influence, to produce the full effect placed his handkerchief on the table, and buried his mouth in it; his insensibility became deeper and deeper, till at last he was too far gone to be able to raise his head. He therefore continued inspiring it, his coma became more and more profound, and a short time after he was found in that position, quite dead.

Secondly, the treatment in the intervals between the paroxysms should be directed to improve the digestive powers of the patient and the tone of his nervous system; the diet should be very carefully regulated, both as to nature and quantity; and this is of all things the most important to occupy the attention of the medical attendant. The alimentary canal and the secretions generally should be kept in a healthy condition; exercise, the hours of rest, and in fact every thing that bears on the general health, should be systematically and rigidly superintended; and I am sure that treatment of this sort will always be found of substantial advantage. Another thing having the same object is cold or tepid affusion, either by sponging or by shower-bath: but cold, while very serviceable to those who can bear it, is often too much for many people, in whom no reaction takes place, the coldness continues, and drowsiness is produced. Such symptoms should always be looked

upon as contra-indications to the use of cold affusion.

Now before I conclude, I will say a word on the relation of emphysema and asthma. Are these two diseases related to one another as cause and effect? and, if so, which is the cause, and which the consequence? To determine this let us see what emphysema is. It is a state in which the lungs are rendered more capacious, in which the relation of the ultimate elements of the lungs are in some measure deranged, and the pulmonary capillaries altered. Now, is this state of increased capacity a state capable of producing such a dyspnoea as we see in asthma? I cannot think that it is, although this increased capacity is attended with other conditions tending to diminish the efficient aeration of the blood, and, therefore, to the production of dyspnoea. But it will not explain the periodicity of the attack: emphysema is constant, asthma paroxysmal. On the other hand, asthma is clearly an efficient cause of emphysema; the asthmatic condition is just such as to produce those physical changes that constitute emphysema; and I think that all patients who have emphysema have had either asthma or bronchitis. It is confirmatory of this view, that emphysema comes on gradually, and that it bears proportion to the length of duration and severity of the asthma; if a person has had few attacks of asthma he has no emphysema, if he has had many, he has; the asthma precedes, the emphysema follows.

This then is the conclusion I come to,—that asthma is primarily humoral; that it is caused by a poison or a morbid matter acting on that portion of the nervous system which ministers to the function of respiration; that it leads to dilatation of the lungs and the walls of the chest, to emphysema, and ultimately to dilatation of the heart; that the habit may pass off, the morbid matter being no longer created, the patient ceasing to be asthmatic, just as a person ceases to be gouty or epileptic; and that, ceasing to be asthmatic, the patient may remain, or may not remain, emphysematous, according to the severity and duration of his previous attacks.

THE CHOLERA IN JAMAICA.

By intelligence from Kingston to the 8th ult., cholera continued to rage with great violence all over the island: the deaths in Kingston alone, from October 10 to November 7, were 1,000. Business was all but suspended. The weather had been highly favourable for the suppression of the disease.

Original Communications.

CASES, WITH REMARKS, ILLUSTRATING THE ASSOCIATION OF CHOREA WITH RHEUMATISM AND DISEASE OF THE HEART.

By WILLIAM SENHOUSE KIRKES, M.D.

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(Read before the Abernethian Society, March 7th, 1850).

For some years past attention has been frequently directed to the occurrence of various nervous phenomena, especially of chorea, in the course of acute rheumatism. The subject has engaged the notice of those best qualified for its investigation, and several ingenious hypotheses have been advanced in explanation of this singular association in disease. To the complete exposition of the views of those who have especially studied this point in pathology, given by Dr. Burrows in his Treatise on Disorders of the Cerebral Circulation, it will be needless, even were it possible, to attempt making any addition: indeed, the subject is so thoroughly discussed there, and in the works therein mentioned, that, but for the obscurity in which the association between the rheumatism and the nervous phenomena is still confessedly involved, I should not feel justified in again drawing attention to it. The very variety of the explanations which have originated from equally competent sources, bears ample testimony to this obscurity on the subject; and on this account it may be hoped that any additional facts which seem calculated to throw light on any part of it can scarcely be deemed superfluous.

The cases and remarks which I now offer have reference to only one form of the nervous disorders that may arise in the progress of acute rheumatism, and this because it is the form most apt to ensue, and because I am more familiar with it than with any of the other affections of the nervous system associated with this disease.

CASE I.—Mary Anne Burke, aged 20.

admitted into Hope Ward, under Dr. Hue, Dec. 3, 1849. Dark-haired, pale, anæmic-looking; manner strange, with an anxious alarmed expression. There were frequent spasmodic movements of the muscles of the face, especially about the nose, these movements being usually accompanied by a short inspiratory moan. These, with an occasional jerk of her head, were the only choreic movements observed. Although involuntary, yet the movements seemed to be under some degree of control; for, by an effort, she could restrain them for several seconds, though after such pause they returned with increased force. She was very taciturn, owing apparently to difficulty in uttering words rather than to disinclination to speak: her answers, when obtained, were rational and correct. She said that she was in pain all over, especially in the ankles, knees, and hips. The left ankle and foot were slightly swollen and tender, but not red.

I may remark here that this slight swelling and tenderness, which shortly disappeared, was the only positive sign, in addition to the general complaint of pains in the joints, that acute rheumatism existed: it was, however, quite sufficient to denote the fact. There was a dry black secretion about the lips; the tongue was pale, thickly furred white, with a dry brown streak down the centre, spreading out at the apex. The pulse was 84, but singularly irregular,—usually three beats would occur in ordinary succession, then there would be a complete intermission, then three more ordinary beats, followed by another intermission. She passed her evacuations in bed, but was conscious of doing so.

She was a dressmaker, always delicate, and subject to severe headaches, especially since a violent blow on the head six years before. She never had rheumatism or chorea; but her mother was rheumatic, and a sister had been in the hospital with rheumatic fever. Nine days before her admission she began to be ill, chiefly with palpitation and a feeling as if "something was amiss with her heart." Three days subsequently she came to the hospital to have a tooth extracted; it was then noticed that her throat was inflamed. At this time her catamenia, which had hitherto been regular, were due, but they did not appear. The next day her throat was

worse, with increase of palpitation and pain in the chest. The twitchings of the head and face commenced two days before admission. For several nights she had had no sleep, and latterly had been delirious, and with difficulty kept in bed.

On auscultation there was found to be increase of the heart's impulse, which was perceived over a greater space than natural. The first sound was loud and ringing, with a short blowing murmur at the apex; the second sound indistinct. There was increased and extended cardiac dulness on percussion. This auscultation was made a few hours after her admission into the hospital; but, when first admitted, certain sounds were heard which led my friend Mr. Wood, who then examined her, to suspect the existence of pericarditis. These sounds, however, had quite disappeared on the subsequent auscultation.

There was something in the general appearance of the patient which suggested the possibility of many of her symptoms being the result of hysteria, and she was accordingly treated at first with the *Haust. Ammoniaci Fœtidus*. The day following, however, the irregular movements had extended to the shoulders, and the next day both arms were affected, the general and auscultatory signs being otherwise unchanged, except that the pulse had become regular. During the night of December 5th, which was her third night in the hospital, she was noisy and delirious, but when visited the following morning was found calm and rational, though with a haggard anxious aspect. The spasmodic movements of the arms were increased in violence, and the right lower extremity was now affected. The lips were dry; tongue red, glazed, and clean, but inclining to dry; skin cool; pulse 120, small, soft, regular: the bowels, which had been relaxed, were quiet: she complained of thirst, headache, and a feeling of weariness and exhaustion; the heart's impulse and action were much increased, and the first sound was prolonged, muffled, and accompanied with a murmur.

Her hair was ordered to be cut short behind, and a blister applied to the nape of the neck, on the supposition that there was some mischief in the head.

The two following nights she had a little rest, and the involuntary movements of the limbs did not increase in

violence, but her peculiar anxious haggard look remained. On the night of the 8th (five days after admission) she was again restless and delirious, and on the following day the chorea had greatly increased, the arms and head being continually tossed about, and the shoulders and chest writhing and twisting incessantly. She got a little sleep in the morning, but was no better on awaking. The jactitation increased towards night. Chloroform was then administered, and quieted her for a time; but, as the effects of the reagent passed by, the movements returned with the same violence as before. On the 10th I made the following note:—With exception of an occasional doze, she was restless and tossing about all night, frequently uttering short spasmodic screams; is now in a state of constant restlessness and jactitation, throwing her arms about, and tossing her head from side to side, the legs being nearly still. She tries to speak, but cannot, only uttering unintelligible sounds: she also frequently attempts to cough, but fails in the effort, apparently from inability to combine the requisite muscular movements. Face pale and haggard; lips covered with sordes; tongue brown and dry; skin cool, moist: pulse small, feeble, about 120 in the minute, her restlessness preventing its being accurately counted. The like cause also interfered with a proper auscultation of the heart. Her head was again shaved, and a blister applied to the vertex. She also took chloroform again with temporary cessation of the movements, followed by a little sleep. She appeared to like the remedy, and occasionally asked for it, as well as she could. In the evening and early part of the night she had a little sleep, but after two o'clock the following morning the choreic movements became more violent, involving the lower limbs, and increased to such a degree that it became necessary to tie her in bed to prevent her being thrown out. Between nine and ten o'clock she was lifted out of bed to have her bed made and her blister dressed. On being returned to her bed, she took eagerly about half a pint of beef-tea, then lay quiet, and in a quarter of an hour was dead.

Five hours after death the brain and heart were examined. In the former, nothing unusual was noticed beyond a peculiarly soft condition of the cerebel-

lum. In the latter was found the probable source of all the mischief. The two surfaces of the pericardium were universally and rather firmly adherent to one another through the medium of a thin layer of soft, reddish, rather dry granular lymph. The heart was rather large: its muscular tissue presented extensive fatty degeneration, large buff-coloured blotches being abundantly scattered through the walls of both ventricles, while the interior of these cavities exhibited in an extreme degree the peculiar streaky, freckled aspect, so characteristic of fatty disease of the heart. Moreover, there was abundant evidence of recent endocarditis, the tricuspid, mitral, and aortic valves being more or less thickly set near their free borders with clusters of minute, soft, reddish, loosely adherent granules.

The objections of friends prevented the spinal cord and other organs from being examined. The liver, however, was noticed to be extremely fatty.

Here we have a case in which a delicate girl, inheriting a tendency to rheumatism, and having a sister formerly affected with that disease, becomes out of health just at a period when the catamenia are expected, but do not appear, suffers a slight attack of articular rheumatism, with a severe inflammation of the investing and lining membranes of the heart, and then has violent chorea, which terminates in death, dissection disclosing, moreover, extreme fatty degeneration of the muscular tissue of the heart. Abundant matter for general comment is here offered, but into this we must not now enter.

CASE II.—Ann Holt, aged 13, a thin, light-haired child, admitted under Dr. Roupell, October 18, 1849, with rheumatic inflammation of various joints, of five days' standing, and pain in the chest of one day's duration. The heart's action was accelerated, and there was a prolonged soft systolic murmur at the apex, faintly heard also at the base, where the second sound was clear. So far the mischief seemed to be only endocardial. She was treated by leeches to the region of the heart, and small doses of calomel and opium every four hours. The next day (the 19th) the first sound at the apex was confused, while at the base a peculiar creaking sound was audible with the systole, the second sound still remaining clear. On the

20th, the joints being still very bad, the first sound continued rough and crackling at the base, while a somewhat similar sound was occasionally audible with the diastole. Little doubt now remained of pericarditis being superadded to the endocarditis, and this little was removed by the pericardial friction becoming more distinctly marked during the next three days, and then it disappeared, leaving an endocardial systolic murmur both at apex and base. The rheumatism now rapidly subsided, and the child got so much better, that I took no further note of her until the 29th, five days after the disappearance of the pericardial sounds. On this day I learnt that the preceding evening she was observed to be very strange and foolish in manner, and that subsequently twitchings ensued in the face and arm: she passed a restless night, and in the morning she had frequent spasmodic movements of the muscles of the face, with involuntary knitting of the brows, while the left hand and arm were constantly in movement. Her thin, wan, feeble aspect, was even more striking than on any previous occasion, and was now combined with an expression of alarm at her strange condition. On auscultation the systolic murmur remained loud, both at the apex and base, while over the aortic valves was heard, for the first time, a remarkably harsh and prolonged diastolic murmur. Her feeble condition, with a small pulse of 130, precluded any active measures, and she was treated simply with Dover's powder to tranquilize her nervous system.

Without going into the daily particulars of this case, I will just remark that the chorea did not materially increase, and scarcely lasted three weeks. During this period there was no return of the pericardial rubbing sound, but the endocardial murmurs underwent strange varieties, which, however instructive in themselves, may be at present passed over, as not bearing materially on the question before us. The child left the hospital shortly after the subsidence of the chorea, but carried with her all the signs of seriously damaged mitral and aortic valves, with an enlarging heart, and probably some remaining mischief in the pericardium. In less than three months she was re-admitted in a dying state, with symptoms all pointing to the damaged heart and secondarily damaged lungs. At first she rallied, and for

some days gave promise of amendment, but then she worsened and died, a copious pleuritic effusion appearing to be the immediate agent in her death. The examination of the body after death revealed recent inflammation of the right pleura, with pneumonia passing on to suppuration. The heart was considerably enlarged, while the effects of the former inflammation were manifest on the mitral and aortic valves, and on the interior of the left auricle. The condition of the pericardium was examined with peculiar interest, on account of the pericardial friction-sound which was noticed about four months before her death. The only evidence of the previous inflammation was in the existence of numerous vascular tufts of new tissue about the base, and of several rough vascular patches of false membrane on the anterior surface of the heart. I have alluded to this case before, in support of the opinion I entertain, that inflammation of the pericardium attended by effusion of lymph does not necessarily terminate, as is commonly supposed, in the formation of permanent adhesions.*

CASE III.—The next case I wish to mention is that of a girl named Eliza Blanning, aged 13, who was admitted January 31, 1850, under Dr. Hue, with acute rheumatism and serious cardiac complication. This girl had been in the hospital two years previously with a somewhat similar attack, and then fell under the notice of my friend Dr. Ormerod, from whom I learn that she was admitted simply for chorea, that subsequently pericarditis ensued, and then, last of all, a distinct attack of articular rheumatism came on. The ordinary sequence of events was therefore, in this case, completely reversed; and the fact is one of importance in relation to the probable cause of the association of chorea, and other nervous diseases, with rheumatism. Another fact bearing on the same subject is, that this girl had suffered at least two, if not three previous attacks of chorea, unassociated with rheumatism, or other obvious cause—the first when she was nine years old, the second shortly afterwards. I need not dwell on any further particulars of the case during her present visit to the hospital: suffice it that, after being placed in ex-

treme jeopardy by the cardiac affection, she was discharged convalescent, though with a seriously and permanently damaged heart. During this attack there was no return of the chorea.

In each of the three cases I have just narrated the rheumatism and chorea were distinctly associated with pericarditis; but I will now mention other cases in which there was reason to believe that the pericardium was quite unaffected, while in at least one case it was probable that there was no affection of any part of the heart.

CASE IV.—In November, 1849, William Harvey, aged 20, a tall, dark-haired, nervous man, was admitted, under Dr. Hue, with a second attack of articular rheumatism, unaccompanied by cardiac affection. Ten years previously, when he was therefore 14 years of age, he had his first—a very severe attack. In the course of it chorea ensued, and lasted several months, at the end of which time he left the hospital well: he had no reason to suspect that his heart was affected during this attack, having no symptoms referable to that organ at the time, and not having been subsequently troubled with palpitation. On his second admission to the hospital the attack of rheumatism was severe, though less so than the former attack. I carefully examined his heart from time to time during the progress of the disease, but never could satisfy myself that there was anything morbid in its sounds or action beyond increased impulse, which might be explained by the general vascular excitement attendant on an acute febrile disorder. It is certainly possible that, in the first attack of rheumatism, there might have been inflammation of the pericardium, resulting in partial adhesion of the pericardial surfaces, and that, in this second attack, pericarditis may have again ensued, and its existence been undetected, owing to absence of the pathognomonic friction-sound in consequence of such adhesion. Yet this can only be supposition; and, in the absence of stronger evidence of pericardial inflammation, it is, I think, fair to infer that the pericardium was unaffected in this second attack. After being in the hospital for about five weeks, and when the rheumatism had almost subsided, chorea again came on, and gradually increased until it had attained such a degree of violence

that the man had to be kept strapped down for a week, to prevent his being tossed out of bed. During the chorea his heart was again several times closely examined, but nothing morbid was detected; and, when he was so far recovered as to be discharged from the hospital, the sounds were quite those of health. The nervous affection in this case seemed therefore to be unassociated with any affection of the heart.*

The following case may be mentioned as somewhat parallel to the last, though it is open to the objection that there was no auscultatory proof of the pericardium or the heart being formerly unaffected.

CASE V.—Elizabeth Lark, aged 17, a healthy-looking girl, was admitted February 1st, 1849, under Dr. Burrows, for a rather slight attack of chorea, of a fortnight's duration. The catamenia were regular, the heart's sounds perfectly healthy. She had had four or five similar attacks; the last occurred when she was 14 years of age, and after continuing for two or three weeks it was followed by a severe attack of articular rheumatism, which confined her to bed for fourteen weeks, the chorea continuing nearly the whole time, and greatly aggravating her sufferings, by the constant movements of the inflamed joints. She had at that time no pain about the chest, and was not subsequently troubled with palpitation.

As before stated, however, these negative circumstances cannot be held as certain evidence that there was at that time no affection of the heart: such affection might have existed, and subsequently quite disappeared, supposing the pericardium to have been inflamed in that attack. We must, however, in such case, conclude, either that no pericardial adhesion had resulted, or, what is not probable, that an adherent pericardium existed, without manifesting any general or physical sign of such a condition.

It may be observed, that in this case, as in Case III. the chorea preceded the rheumatism; also that there had been several previous attacks of chorea, unassociated with rheumatism: the import of these facts will subsequently appear.

* In two months afterwards, however, this man was re-admitted with another attack of rheumatism, followed again by chorea, and accompanied this time with a rough systolic murmur at the apex.

CASE VI.—Maria Potter, a healthy-looking girl, 16 years of age, was admitted under Dr. Burrows with rather a severe attack of chorea, from which complaint stated she had scarcely been free since she was six years old. When fifteen she was in St. Bartholomew's Hospital with acute rheumatism; she then had a slight attack of chorea; the catamenia first appeared at that time. She had been troubled with palpitation since the rheumatism. When in the hospital the second time, the chorea was not associated with rheumatism: there was, however, a soft systolic murmur at the apex, which seemed to indicate that in the previous rheumatism she had suffered from endocarditis. There was, however, no evidence of previous pericarditis; and the remarks on this point applied to the last case will apply here also.

CASE VII.—Eliza Fanchild, aged 17, a small, puny, undeveloped girl, who had never menstruated, was admitted under Dr. Roupell, with a severe and first attack of chorea, which could not be traced to any exciting cause. There was no rheumatic history; and I could not clearly ascertain that she had been particularly exposed to fright, which is so often the alleged, and probably the real, exciting cause of chorea. After about six weeks of incessant choreic movements, terminating at last in complete jactitation, requiring her to be restrained in bed, the poor girl died, quite worn out.

After a careful post-mortem examination of the body, including the brain and spinal cord, I could find nothing at all bearing on the case, except that the free edge of the mitral valve was roughened by minute granulations.

Apart from the general interest which this case possesses as an example of fatal chorea, in which examination after death failed in disclosing any serious organic lesion, the case is of pathological value, from the presence of the deposits of fibrine on the mitral valve: the bearing of this point on the subject under consideration will appear in a subsequent part of this communication.

[To be continued.]

AN ESSAY ON UNHEALTHY INFLAMMATIONS.

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[Continued from p. 796.]

ERYSIPELAS (continued).

Question of treatment continued—Heat and redness no criterion of power—Opinions of those writers who regard it as an asthenic disease—Illustrations of the views and arguments upon which they are founded.

"PARACELSUS declared to his audience, that if God would not impart the secrets of physic, it was not only allowable, but even justifiable, to consult the devil." It is much to be desired that practitioners in medicine, in our own time, would leave this latter alternative to the disciples of Mesmer and of Hahnemann, and to impostors of this stamp; and that, if resolved to persist in bleeding their patients *by the bucket*, they would be content to register their heroic deeds on the tablet of their own consciences, in place of recording them in print, to the irreparable injury of the rising generation in the profession—not to speak of the additional labour thereby indirectly thrown upon the hands of the already hard-worked registrar of mortality.

The mistaken and mischievous principle of seizing upon a particular *quality* in disease—as the "rubor," the "calor," or the "tumor"—and setting that up as the all-absorbing feature in its pathology, was well remarked upon two hundred years ago by Sydenham. In treating of gout, our illustrious countryman says—"He has the best and truest claim to the name of physician who is in possession of the medicine that shall destroy the *species* of the disease—not he who merely substitutes one primary or secondary quality for another. This he can do without extinguishing the species at all—i. e. a gouty patient may be cooled or heated, as the case may be, and his gout continue unconquered. This method of merely introducing different qualities can no more effect the direct destruction of specific diseases, than a sword can quench a flame. What can be done by cold, or heat, or wet, or dry, or by any of the secondary qualities

that depend upon them, against a disease whose essence consists in none of them?*" The far-sighted sagacity discernible in this single remark bespeaks this wonderful man as a couple of generations in advance of the pigmies in learning of his time, and is so far deserving of note in our own day, that we are only just beginning to recognise these "qualities" in disease as consequences of a cause in operation elsewhere, and not as the primary elements of the same. But the great body of the profession is by no means prepared, as yet, to recognise the truth, that some of these qualities under notice—the "rubor, tumor, et dolor cum calore," more especially—are not the direct expressions or representatives of power in the circulation of the part where they occur. Hence, I conceive, the practical evils arising out of these misconceptions, and which, I think, are forcibly illustrated in the practice of Dupuytren, of Dr. Duncan, jun., and of Mr. Lawrence, as addressed to the heat and redness of erysipelas.†

As respects the increased heat of inflamed parts alone, it is becoming more probable every day, as Mr. Paget has lately suggested, that this phenomenon is rather significant of some combustion or destruction "of wasted tissues, or of surplus food," than of the "increased formation of organic substances." It is certain that heat is often developed in proportion to the decline of the dynamic powers of life. This was long ago alluded to by De Haen:—"Omnium maximus calor inventus est hominis, quo tempore cum morte luctatur, exspirat, jamque aliquantum vivere desinit; ita quidem, ut deprehenderimus, in ægro, cujus calor per totum acutum morbum supra 103. gradum non ascenderat, sub mortem, perque bina a

morte minuta, notari gradum caloris 106." And he gives us, amongst others, this striking example—"Mulier exulcerato mammae cancro misera, colore livida, omnino emaciata, pulsum parvum habens, tota die sedens, iterato quater spatio mensis experimento, constanter inventa est gradus tres quatuorve majoris, quam sano in homine est, caloris notare."*

Mr. Paget, in his recent lectures on *Inflammation*, has pointedly touched upon this question. "It may be said," observes Mr. Paget, "that the signs of inflammation are signs of increased action; but these are fallacious if, again, by increased action be meant any increased exercise of vital force. The redness and the swelling of an inflamed part declare the presence of more blood, but this blood moves slowly; and it is a quick renewal of blood, rather than a large quantity at any time in a part, that is significant of active life. An abundance of blood, with slow movement of it, is in no case characteristic of activity in a part; it more often implies the contrary, as in the erectile tissues and the cancellous tissue of bones. The sign of heat in the inflamed part is equally fallacious. The source of the locally-increased heat cannot, I believe, be satisfactorily explained. This phenomenon of inflammation is involved in the same difficulty as are all those that concern the local variations of temperature in the body,—difficulties which the doctrines of Liebig, however good for the general production of heat, are quite unable to explain. But, from the fact that the general supply of heat in our bodies is derived from oxidation or combustion of wasted tissues, or of surplus food, we may assume that in local augmentation of heat the source is rather from some similar destruction of organic substances than from increased formation of them. This can, indeed, be only assumed; but, if there be little evidence for it, there is as little for any assumption that the increased heat of an inflamed part is an indication of an increased formative action."‡

I have felt the more satisfaction with the embodiment of these views of Mr. Paget's, that they express so completely

* Preface to third edition: from the Latin of Dr. Greenhill, vol. i. p. 21. The italics are my own.

† The question should have been appended to the critique upon Mr. Lawrence's monograph in my last paper, but may not be wholly out of place in the present. For what object or end was Mr. Lawrence's paper published at all? Who were the committee or arbitrators legislating upon its merits? Surely, it is to be hoped that he was not the presiding judge himself, as in the late adjudication of the Swiney Prize! If not, the selection of a most pernicious as well as mediocre production for publication in the Society's Transactions, bespeaks, in my opinion, either a venal and corrupt state in the administration of that learned body at the time, or a lamentable want of intellect on the part of the "Committee of Papers."

* Rationes Medendi Antonii De Haen, caput "De Sanguine et Calore humano," tom. iii. p. 227-8. Vide also vol. ii. caput, "De suppartando Calore Corporis humani."

‡ MEDICAL GAZETTE for July 19, 1856, vol. xiv. p. 90.

those which I have myself long held* in connection with the interpretation to be put upon these physical qualities of redness, heat, &c., in the inflammations, but, for a clear exposition of which I had in vain looked through the literature of medicine; and, they are the more important, that the attention which these lectures are at present receiving at the hands of the profession may become the means of restraining the rising generation of surgeons from blundering on in the footsteps of their predecessors, even where great names may seem to invite a continued onslaught upon the *rubor et calor* with lancet, leeches, and Epsom salts.

Having afforded the reader an opportunity of forming an impartial opinion of the merits of those writings in the literature of medicine which are the advocates of a depletory and lowering plan of treatment for this disease, I crave his patient attention to the arguments which I now propose to bring forward in support of a directly opposite practice; and I flatter myself that he will find no fault with the authorities I shall cite.

"Sanguinis equidem missio," says Heister, "nec non alvi purgatio in erysipellate non æque ut in phlegmone necessariæ videntur. Quicquid enim in erysipellate mali corruptive humoris subest, id ipsum, quis in summâ tantum cute hæret, per sudorem lenem quam commodissimè expellitur." However, Heister makes this qualification:—"Interim, ubi vel nimis vehementes pulsus arteriarum et calor est, vel nimia quoque sanguinis eopia, haud omnino aliena sanguinis per venas detractio censenda est."†

"The erysipelatous inflammation," says Hunter, "often arises spontaneously, or in consequence of a low or debilitating fever. It often arises from accident, but then it is commonly a secondary inflammation, although not always; for, the first shall have gone off, and when suppuration was to take place it shall have come kindly on, but afterwards the erysipelatous shall take place."

* Vide a series of papers, by the writer, on "Unhealthy Inflammations," in the *Lancet* for 1849, *passim*. *Es. gr.*—"It is the crying sin of the present day to associate heat and redness, under whatever circumstances occurring, with a necessarily sthenic condition of the system."—*Lancet*, Nov. 24, 1849.

† *Institutiones chirurgicæ*, vol. i. p. 233.

"Whatever the inflammation may be" (meaning, probably, whether idiopathic or symptomatic), "it is certainly attended with nearly the same kind of constitutional affection. The fever in both appears to be the same—viz. accompanied with debility, languor, &c."

Hunter, in the foregoing paragraph, has been explicit enough in his opinion of the asthenic character of the disease; but we find him even more decided in these views elsewhere:—

"So far as it may be necessary to take notice of the different inflammations, they may be comprehended in *five* divisions." (I have already† individualized these, but should have added that this division is meant to be exclusive of the *specific* inflammations—such as gout, syphilis, &c.) "All except the first," (the "adhesive") "have a kind of *affinity to each other*, although I think the oedematous has the least affinity to the three last; and may vary so as to make it difficult to say to what species the varieties belong."

"All" (except the first)—"that is, the oedematous, erysipelatous, carbuncular, and the gangrenous—have a kind of *affinity to each other*."

Now, of the oedematous Mr. Hunter remarks—"The difference between this inflammation and the adhesive arises, I conceive, from the principle of inflammation acting upon a *dropical disposition, which is always attended with weakness*; whereas a greater degree of strength would have produced the adhesive inflammation under the same cause or irritation."§

Of the carbuncular he says—"This inflammation attacks more beyond the middle age than at it, and very few under it. It is most common in those who have lived well. It appears to have some affinity to the boil; but the boil differs in this respect—that it has more of the *true*" (healthy?) "inflammation, and therefore spreads less, and is more peculiar to the young than the old, which may be the reason why it partakes more of the true inflammation."¶ Had Mr. Hunter lived in our own day, he would, doubtless, have expressed himself far more decidedly upon

* Hunter's Works, vol. iii. p. 314-15.

† *MEDICAL GAZETTE*, p. 707, Oct. 25, 1850.

‡ Hunter, vol. iii. p. 316.

§ *Ibid.* loc. cit. p. 314.

¶ *Ibid.* loc. cit. p. 317.

the unmistakably *low* character of the carbuncular inflammation.

That Mr. Hunter considered his last form of inflammation—viz. “that which leads immediately to mortification”—as no exception, *in amount of power*, to the preceding, is evident from his own account of the matter:—“Inflammation often produces mortification or death in the part inflamed. This commonly takes place in old people that are become very much debilitated, and chiefly in the lower extremities. I suspect it to be somewhat similar to the carbuncle—viz. principally in those who have lived well, although not so much confined to them as the carbuncle: however, it takes place in the young where *great debility has been produced from disease, especially those diseases that have debility as a principle*—such as what are commonly called putrid fevers; but the situation of these is not so determined, and, in such, inflammation hardly takes place without an immediate exciting cause, as the application of blisters, &c. Death in a part sometimes takes place almost immediately without inflammation.”*

“Such inflammations,” adds our author (alluding to the four here particularized), “have little of the adhesive tuffaction in them, are not clear or transparent, but rather of a dusky red. As the colour of the inflamed parts shows something of its nature, it is to be observed that it is different in all these inflammations from that of the true adhesive; and, as we have reason to believe that the circulation is quicker in the adhesive inflammation than is natural, and that the colour arises from this cause, *we may suppose that the motion of the blood in these is languid, and that it assumes the venal appearance even in the arteries.*”†

It is with peculiar pleasure that I have been able to press Mr. Hunter into my service, in support of the existence of such forms of inflammation as are immediately dependent upon *asthenic* conditions, as this is the best commentary I could offer upon Mr. Lawrence's negation of the compatibility of such with debility.

I refer with scarcely less satisfaction, in the next place, to an authority of great weight with the profession in this

country, as he has thrown himself so unreservedly into that view of the subject in hand which I believe to represent the true nature of the pathology of erysipelas:—“I must acknowledge that a patient study of nature hath induced me to suspect that the *relation between erysipelas and inflammation is extremely remote*. It appears to me proper and justifiable to consider erysipelas as a genus, the specific characters of which are as widely differing from inflammation as those of inflammation are dissimilar from the phenomena proper to fever. This opinion will receive farther illustration if we advert to the mode of treatment that is appropriated to each; the very different manner in which the two diseases terminate; and also to this remarkable fact—that the two diseases are not reciprocally “converted into each other” (p. 174-5). That hardness of the pulse, which is a distinguishing character of inflammation, is not present in erysipelas. The general state of the system verges rather to debility, or depression of strength, than to increased strength or vigour.” And then Mr. Pearson adds the following proposition:—“In a part that is affected with an erysipelas there is a morbid irritability of the nerves; arterial contraction is performed with an increased velocity, but with diminished vigour” (the same conditions of arterial contraction, I may remark, which characterise the system under loss of blood); “and the parietes of the blood-vessels give less resistance than natural to the *vis à tergo*.” (p. 182-3.) It is no little detraction from these very sensible and satisfactory views in relation to the pathology of erysipelas, that Mr. Pearson, under the head of Treatment, directs his readers “to diminish increased vascular action in the acute erysipelas by general and topical blood-letting.” But he adds—“General bleeding is not recommended in this place as a cure for erysipelas in the same sense in which it may be said to remove an inflammation: it is advised with the intention of obviating the effects produced in the system by so severe a stimulus as acute erysipelas!” And then—“The exhibition of emetics and severe purgatives have sometimes been succeeded by very fatal consequences.” In another place he remarks, under the head of “Symptomatic Erysipelas:”—“I have seen the most dangerous symptoms

* Ibid. loc. cit. p. 31a.

† Ibidem.

‡ Principles of Surgery, by John Pearson.

immediately supervene to the loss of a very small quantity of blood." (p. 200.)

Turn we now to another* master-builder in the science of disease:—"There are many constitutions which have a tendency to specific diseases that, when injured by fever or any constitutional complaint, readily produce the specific inflammation in such parts of the body as have the greatest susceptibility for any specific action; or, if such parts are affected by any local violence, the parts affected will not go through the *healthy* adhesive inflammation, nor will they enter into the *healthy* suppurative inflammation, but will fall into the specific inflammation peculiar to the habit." Such is the case with an *erysipelatosus* habit." Dr. Mason Good proceeds, in the next place, to associate this "erysipelatosus habit" with an essentially feeble state of sanguification: thus—"In almost every instance there is evidently a diminished vascular action; and hence we meet with the disease far more frequently in persons of delicate habits, women, children, and those who have long resided in warm climates. In one instance it has occurred to me in a strong hearty man, of plethoric form and sanguineous temperament, well known to the world as a public character; but in this case the diet had, from the patient's boyhood, been exclusively that of vegetables." (p. 368.)

Dr. Mason Good would appear to have considered this case as so much the exception to a general rule as to have been under the necessity of offering a special explanation of its occurrence. He presently after remarks—"It has occasionally happened that, instead of diminished vascular action, there has been such a degree of *entony* and caumatic fever as to call for free venesection from the first."

With such views of the essential nature of this inflammation, it will be needless to say more than that Dr. Good condemns depletion altogether in the treatment of the same, save in the rare *exceptional* cases which occasionally present themselves to notice. "Venesection was formerly recommended as a part of the ordinary plan, and has been so of late by a few writers; yet this is to act without discrimination, and to mistake the exception for the general

rule." "I can conceive very few ordinary cases in which the lancet has a chance of being serviceable; while the application of leeches always exasperates the efflorescences." "The bark given largely, as long since warmly and judiciously recommended by Bromfield and Colly, has rarely failed of success."*

In his "Lectures on the Theory and Practice of Surgery," Mr. Abernethy has raised his voice, succinctly but earnestly, in favour of the same views of the nature of erysipelas:—"Erysipelas," he says, "when it comes on spontaneously, or without an evident exciting cause, is indicative of a disordered state of health." "When erysipelas supervenes upon some local irritation, it equally indicates a disordered state of health." And then, "deceiving upon Desault's treatment of the disorder with tartarised antimony, he adds:—"As the disease is connected with weakness, even this medicine should be used with caution." (p. 68-9.)

Dr. Butler's work on "Irritative Fever" is well known to practical surgeons. That physician has recorded a series of cases of erysipelas (for they were instances of that disease under a different signification) which cannot fail to carry great weight with it in relation to the propriety of bleeding under such condition of the system. In summing up the evidence afforded by the materials of his work, Dr. Butler makes the following remarkable statement, which I think particularly worthy of attention in this place:—

* Mason Good, loc. cit., p. 369. The present is not an inappropriate place for introducing the late Mr. Samuel Cooper as an observer tinged with a very decided bias in favour of the views and practice of Mr. Lawrence in this inflammation,—though not prepared, perhaps, to go to the full extent of that surgeon's precepts. In the notes to his edition of Dr. Mason Good's work, both under the head of Erythema and Erysipelas, Mr. Cooper registers his dissent from that doctrine which regards the disease before us as a modification of common inflammation. "From the foregoing observations," says he, in sequel to Dr. Good's chapter on the subject, "it appears that the author was much under the influence of the doctrine that the fever called by him erysipelas, and all kinds of erysipelatosus, or, as he terms it, erythematic inflammation, are essentially connected with diminished vascular action and debility." "After what has been explained under the head of Erythema, in a previous section of this volume, it is unnecessary to insist upon the fact that the local affection is always of an inflammatory nature,—that, anatomically viewed, it requires antiphlogistic treatment." "But should free and even repeated venesection be omitted when the patient is young, strong, or plethoric." (Vol. ii. p. 104.)

* Study of Medicine, by Mason Good, M.D., vol. ii.

"Experience has shown that, out of twenty patients recorded in this work (thirteen of whom were artificers of the dock-yard) who had been blooded whilst labouring under irritative fever, nineteen died." (p. 249.)

Dr. Butter well remarks:—"A person who considers every unhealthy commotion of the mind or body as inflammatory, and employs his lancet accordingly, must be sometimes disappointed in affording the relief which he expected and intended, especially when the mischief arises from an excitement of the nervous system." (p. 242.)

Dr. Fordyce opposed a depletory line of practice in erysipelas with much earnestness:—"Bleeding, and other evacuations (he says), I have always found hurtful. Peruvian bark is the most powerful remedy: it should be exhibited in substance, and in as great a quantity as the stomach will bear."*

Dr. Wells, always an authority on this subject, concurred entirely in the practice laid down by Fordyce for erysipelas.

Mr. Bromfield informs us of the generally fatal tendency of the antiphlogistic treatment during its epidemic continuance for two years in London, while cordials had an opposite influence upon its course.

Heberden, the truthful delineator of Nature, after remarking that erysipelas is "much more common in those who have begun to find their health a little impaired," observes that "this distemper seems to partake of the nature of those which are called malignant more than of the inflammatory; by which I mean that in general it does not require nor bear much evacuation. I have seen very dangerous symptoms follow not only bleeding, but even a gentle purge, though given after the patient had begun to recover."†

Willan describes four forms of erysipelas—E. phlegmonodes, E. cedematosus, E. gangrenosus, and E. erraticum. "This practice (blood-letting) must evidently be improper in the three forms of erysipelas last described; and even in the Erysipelas phlegmonodes it does not always appear necessary. When the blood drawn is sisy, practitioners are often induced to bleed a second

time; but we generally find, in London, that repeated blood-letting aggravates the symptoms, and protracts the disease."‡

Sir Astley Cooper's† views of the speciality of this form of inflammation are well known in this country. "There is something peculiar," he says, "in this inflammation; and, as it is much disposed to produce gangrene, I will here introduce this subject to your attention." And, again, he presently repeats—"It is very common for erysipelatous inflammation to terminate in gangrene." In another place he remarks—"Whatever renders the body irritable‡ predisposes to erysipelas." After recommending the treatment to be conducted with Quinine, preceded by a little calomel at the outset, Sir Astley makes the following statement:—"My colleague Dr. Marcet, now deceased, but late a physician of Guy's, endeavoured to ascertain whether the antiphlogistic or tonic mode of treatment was best for this disease; therefore he put two persons in adjoining beds, having erysipelas, to one of whom, after purging him, were given tonics and a generous diet,—to the other, saline medicines and low diet; blood likewise was abstracted from the latter: they both recovered,—the former rapidly, while the latter remained in a debilitated state for a very considerable period."§

[To be continued.]

EMPLOYMENT OF HYDRO-FERRO-CYANURET OF POTASSIUM AND UREA AS A SUBSTITUTE FOR QUININE.

M. LEMAITRE, interne at the Hôpital la Charité, has made experiments with the above salt in five cases; in two it was successfully used, in three unsuccessfully. —*Journal de Chimie Médicale*, 1850. x

* On Cutaneous Diseases, vol. i. p. 517. Willan's erudite chapter on Erysipelas is an admirable exposition of even the present state of our knowledge in relation to the disease.

† Lectures on the Principles and Practice of Surgery, &c., vol. i. p. 224, et seq.—Sir Astley's account of the disease is very meagre, and much below the requirements of the present day.

‡ Under the head of "Irritable Inflammation" Sir Astley had previously observed—"In this disorder the blood-vessels are much less affected than the nerves."—Lectures on Surgery, vol. i. p. 53.

§ P. 247-s.—Mr. Tyrrell has appended to the text an interesting account of a formidable case of the disease, extending from the right foot to the occiput, which was treated daily (and successfully in the end) with "beef-steaks, three pints of porter, four ounces of sherry, and quinine."

* Medico-Chirurg. Trans. vol. i. p. 293.

† Commentaries on the History and Cure of Diseases, 4th ed. p. 141.

CONSIDERATIONS ON THE CLIMATE OF ITALY,

WITH REFERENCE TO THE TREATMENT OF CHRONIC DISEASE.

By JAMES EDWARD POLLOCK, M.D.,
Licentiate of the College of Physicians, and Medical Resident at Rome from 1842 to 1849.

NO. II.

THE general characters of the Italian climate, of which we propose giving only a sketch, are, for our purpose, to be considered comparatively to those of England.

The shortness of the winter—the absence of high winds—the mildness of the spring—are the three characters which, much more than high temperature, form the peculiarities of Italy between October and May. We shall notice its summer again. Those who expect in that country a uniform high temperature during the period indicated, will, on experience, be much disappointed: hundreds are so annually. Yet high temperature is not the ingredient of most importance in a residence for the consumptive. Evenness of temperature, and shelter from or the absence of high winds, are what prove to be the two sources of comfort to the invalid.

For medical purposes, the climate of Italy may be divided into the stimulant, or bracing, and the sedative; and without entering into details, for which we refer the reader to the many published tables of temperature, &c., to which these remarks can only be considered as supplemental, the former, or stimulant, will be found to embrace Nice, Florence, and Naples—the latter, Rome and Pisa: a sketch of each will suffice.

The stimulant air is felt to be so on every inspiration: it is dry and bracing, without being keen, unless when high winds prevail, which is not unfrequently the case; but its ordinary characters of purity, dryness, as well as the elasticity which it imparts to the muscular powers, and (shall we say) to the tissues, in accordance with our ideas of the word "bracing," render the French epithet, *cif*, peculiarly applicable. Under such influences, the languid in habit are roused, appetite increases, and the digestive powers are strengthened; the

respiratory acts are performed with more vigour, and the elastic structures of the air tubes rendered more contractile; secretion is lessened from the mucous surfaces, and from their great type, the skin. Such sensations we have all felt on some day of blue skies, on the mountain-side, in England, or, it may be, many of us in the upper valleys of the Alps. The excess of this kind of climate cannot be too well remembered—high winds, drying every pore, a hot sun. The effects on the respiratory apparatus in the sanguineous invalid are, lessened secretion, leading to congestions and hæmoptysis; the cough rendered dry and hard; expectoration diminished in quantity, tenacious, difficult. The skin, paralysed, as it were, in its action, throws a double duty on the exhalant powers of the kidneys and lungs, and all the internal organs are congested. The extreme degree of these results is often wondered at by the English medical traveller in the form of acute inflammations running to a fatal termination in 30 or 40 hours.

The inhabitants of the parts of Italy and France in which this form of climate prevails have temperaments formed under its influence, and physical and moral dispositions in wonderful accordance with it. Provence, of which Nice may be considered as an extension, Genoa, Florence, and Naples, produce the sanguineous temperament as if from a hot-house. Irascible and fiery in passions, premature and sparkling in intellect, with too much rapidity for energy, and too little durability for sustained action, they are prompt to feel and hasty to do, and their powers found expended in a premature and unavoidable enthusiasm.* Such is their moral

* We need scarcely remind the reader how the events of the last two years, illumining and then darkening over the horizon of that land endowed with the "fatal gift of beauty," bear out the above remarks. Had Italy been less violent and enthusiastic in her newly-recovered strength, she might now have been free. Who shall say that the more phlegmatic, yet not less energetic northerners would have lost such splendid chances?—or who shall prophesy a bright future to a people endowed with such effervescent energies? In pursuance of this subject, contrast the uniform historical conduct of the north with the south of France. A line drawn from Lyons to Nantes would leave below it the field of the most fearful tragedies of the Revolution. It is true that the south has given birth to great leaders,—as Bearn to Henri IV. and Corsica to Napoleon, but the qualities best suited to the chief are those least available for enduring

picture; the physical corresponds closely. In no European countries are inflammations so rapid or so fatal. It is common to hear of pneumonia, peritonitis, or pleuritis, cutting off a previously healthy man in 40 hours; and diseases viewed by us as chronic become fearfully accelerated with them. Experience has proved that, although the cases are few, in no country is phthisis so rapidly fatal. The great majority of their consumptive cases are "galloping;" active hæmoptysis, rapid softening, hectic of an insupportable violence, and death often in six months or three months, form their usual history. How very many English patients have died at Nice, Montpellier, and other places along or near the northern Mediterranean coast, every traveller can attest. We have several times known such irritation "lighted up" (no inappropriate expression) in cases of quiescent tubercle which had been conveyed to Nice for change of air, that high fever, hæmoptysis, and rapid passage from the first to the second stage of phthisis, have carried off the patient in a few weeks from his arrival; or, if a timely departure has saved the immediate fatal result, an advance from a passive to an active stage, to oppose which is the object of all treatment in consumption, has already taken place. We shall not stop here to point out the practical deductions to be drawn from an observation of these facts, nor indicate the class of cases which are most or least likely to profit by exposure to the stimulant air, but desire to place side by side with the above an outline sketch of the sedative air, such as prevails at Rome or Pisa, and of which the former place affords undoubtedly the best specimen in Europe.

For about fifteen days out of twenty, a calm and mild, yet pure atmosphere pervades the city of Rome, and the whole of the immense plain which, for a distance varying from 12 to 40 miles, surrounds it. The air is (to use the often repeated expression of the invalid) "easily breathed." Its effects on the respiratory system are to allay irritation and to relax the muscular and elastic tissues, amongst which we must class the bronchial apparatus. Secretion is more easy, and becomes profuse in many cases. The mucous surface performing its exhalant office freely, those sensations of tightness, difficulty

of breathing and of expectorating, which in most instances may, perhaps, be attributed to congestions of the minute tubes, scarcely trouble the patient; and we are called on much less frequently than at home to minister to those accidental accompaniments of consumption, which try our ingenuity and tax our patience in so many instances of the disease occurring in our own climate. A lessened cough, freer secretion; and consequently less frequent occurrence of congestions, and the absence of pain, which is so often caused by these or slight pleuritic attacks, may, perhaps, be stated to be the most observable effects of the sedative air on the respiratory function itself. The prevalent wind is from the south-west,—the "Scirocco,"—bearing on its wings often, if not always, evidence of its passage across the sandy plains of Africa, and, nearer at hand, of the malarious district which lies to the south and west of the city. It is hot, and, perhaps, more frequently dry than moist, but occasionally so wet as to render streets, and even staircases, slippery and damp. Under the influence of this air appetite ceases, the tissues are relaxed, the spirit flags, and energy of a sustained character is felt to be a difficulty; a sense of lassitude and weight in the head is almost universal; and those familiar with the crowded receptions of the Roman season have often, on an evening when the gayest have met the gay, seen the influence of the state of the sky overcast even the votaries of pleasure within the walls of some stately palace.

The energies of the people whose home is under such a sky are of a low order. Enterprise has vanished from the city of the Cæsars; and often have we heard foreigners of keen observation, on viewing the ruins of the great that were, and witnessing the sleepiness which prevails for ten months out of twelve over all the activities of the present inhabitants, wonder how ancient Romans accomplished such vast undertakings as her walls, her tombs, her theatres, her very sewers, now evidence to have been done in the former Capital of the World. To this we could find an answer; were this the page for such a discussion, in the fact that the districts now malarious were then cultivated, and that the monuments of the capital were often the result of the labours of captives from every clime.

whilst the heads which directed the enterprises were but rarely indigenous to the Roman soil, and even when so, had a field of climate extending from the snows of Britain and Germany to the luxurious lands of the East, in which to exercise the intellect and train the body to hardihood by military operations. The modern Roman is phlegmatic, slow in action, but ungovernable when temporarily excited, after which he again becomes sullen. On inquiry we found that most of the greatest crimes were committed on momentary impulse, and that murders were by far more frequent during the dead heats of the Scirocco in summer than at any other time.

The morbid effects of such a climate are seen principally on the nervous system. Head affections, phrenitis, and apoplexy, are very common, and often rapidly fatal,—so much so, indeed, that a Commission was expressly appointed by government to investigate the causes of their frequency, who reported that it was due to the influence of malaria. Headache is prevalent to a degree which interferes with the comfort of almost every stranger who visits the city, as well as of those who reside in it. It is generally frontal, and its character is that of weight, or of a tight band causing compression. In many instances we have known it to cease suddenly on a change of wind occurring from south to north. It is not generally intermittent, and is not curable by antiperiodics.

The digestive powers become gradually weakened by a residence in Rome, and dyspepsia of a congestive form prevails to a remarkable extent. Appetite fails, the bowels become torpid, as if their muscular tone was impaired, and purgatives, which have previously been found sufficiently active, cease to have anything like the same effect. This state of things is so universal, that the writer could not recollect a single instance in which it did not occur in all those who spent any time in Rome. Derangement of the biliary function, the result, probably, of slight congestion of the liver, is attributed by many physicians to the influence of malaria; and some, whose long residence abroad entitles their opinion to great respect, are decidedly of opinion that such an effect is very frequent, even when no intermittent affection points to the marsh poison more distinctly. That the kidneys

should also suffer, probably in consequence of the disorder of the secondary assimilating processes, is not to be wondered at. High authorities in this country have already noticed the influence of residence in a malarious district in giving rise to oxalic acid, and probably to other abnormal products, in the urine; and the writer can corroborate all these statements from experience.

It follows from this view that we must regard the southern climate, and especially that portion of it most suitable to the numerous class of chest affections which derive benefit from sedative air, as prejudicial to the nutritive process, the stomach itself losing tone, as evidenced by the loss of appetite, and the secondary processes of digestion and assimilation suffering more or less. This is a very important consideration in the treatment of chronic disease. We have already dwelt on the fact which should never be forgotten, that on the integrity of the nutritive function depends most, if not all, of the reparative efforts of nature which it is our province to assist.

In pursuance of our object, we would now briefly consider some of the more prominent varieties or modifications of phthisis (a disease which constitutes nine-tenths of the cases exported), with a view to arriving at conclusions warranted by physiological considerations as to the advisability of removing them from England to Italy.

Without entering into subtle pathological discussions, it may be assumed that the result of accumulated careful observations leads us to believe that the deposit of tubercle in any organ is the result of a perverted or lowered state of nutrition giving rise to a new formation of low vitality, and that the effects of such deposit, as seen in the subsequent symptoms, arise from two causes—*first*, from a continuance of the same deprivation of the nutritive function which produced the morbid formation; and, *secondly*, from the irritation caused in the tissue of the organ, the seat of the deposit, by which inflammation of an unhealthy character is produced—an effect which varies in intensity according to the greater or less importance of the seat of the affection in the vital scale, as also from the capacity of the individual, from idiosyncrasy or temperament, to produce and sustain an irritative inflammation. We have thus

a disease of *debility* which could not have been produced had the nutritive function existed in integrity; but the results belong to the class of *irritation*, and occasionally exhibit varying degrees of inflammation. Let us consider for a moment the effects of the different temperaments on this state of things. An individual already depressed in vital powers to a degree which has given rise to morbid instead of healthy formations from the blood, undergoes a deposit of extraneous matter in the lung. Should his powers of circulation and muscular tone be low, the blood itself deficient in red particles, the lymphatic system liable to congestions or failures in the elaboration of healthy products, the reaction against the morbid deposit may be trifling, and he may sink, overcome as it were by the first invasion of tubercle, which is generally found extensively deposited throughout a large extent of both lungs, as well as in other organs. Such cases we have observed among the very young, and have witnessed the gradual obliteration of the lung, as evidenced by progressive dulness on percussion, &c., ending in a fatal result without either cough or expectoration having ever been present, a post-mortem examination revealing a dense mass of tubercle in its first stage. But to this class also belong the slower scrofulous cases where violent irritation is absent, frequent external attacks on the glandular system often alternating (as it were) with the progress of the affection in the lung, and even alleviating the symptoms of the latter. But if the sanguineous temperament prevail, indicating a proneness to inflammatory action, an irritative excitement is at once set up, the parts surrounding the deposit are injected with blood, and hæmoptysis of an active character is generally among the earliest symptoms of the lesion. The history of such a case will generally be found to be that of a rapid destruction of the tissue of the lung, the tubercle undergoing its changes with dreadful speed; while daily chills, followed by burning heats and profuse sweats, with a rapid pulse, rarely under 100, indicate the sympathy of the whole system with the morbid local actions. Hæmoptysis is frequent, the cough is hard, the breathing tight and difficult, the expectoration frothy, showing how much the bronchial membrane is irritated; while slight pleuritic attacks are more fre-

quent in this than in any other class of phthisical patients. Observation shows that such cases are by no means inconsistent with the scrofulous diathesis to which we generally attribute a power of producing only slow inflammatory action. On the contrary, should the sanguineous temperament be superadded to an hereditary tendency to scrofula, and such an individual be attacked with tubercle, this combination may be said to present the most rapid form of phthisis. The tendency to bilious derangement in persons possessing the sanguineous temperament is a serious consideration in balancing the chances for and against the prolongation of life. The supply of healthy blood furnished from the digestive process, if interfered with by frequent, even although slight, failures in the elaboration of healthy chyme from the food, soon becomes insufficient to support the enormous drains from the system by expectoration and sweats alone, and the emaciation is rapid. From our observations on the climates of France and Italy, it will be seen that the stimulant air of Provence, Nice, or Naples, would eminently disagree with such cases, from the tendency of the latter to produce overaction of the vascular system. The sedative air of Pisa or Rome is often productive of excellent effects on the circulation, the pulse becoming calmer, and on the respiratory organs, as irritation is decidedly lessened by its mildness, cough and various uneasy sensations being allayed. But this improvement is only palliative, as, if the primary curative indication be neglected, that of furnishing by an improved state of digestion and assimilation a more healthy blood by which the reparative process is furnished with vital material, we cannot hope to withstand effectually the ravages of the disease.

There can exist but little doubt on the minds of impartial observers that the very properties on which depend the sedative effects of certain climates on the respiratory functions, are directly prejudicial to the full and healthy exercise of the digestive, and, in connection with the sanguineous temperament, which we have found to be that chiefly open to such influences, we shall here notice this important subject a little more in detail.

The European localities which have climates possessing this so-called seda-

tive property are generally marshy, or in such neighbourhood of marshes (or that peculiar soil which produces malaria, and which by no means needs stagnant pools for its development), that diseases of an intermittent form are known to prevail extensively at some, or all seasons of the year. The city of Rome, and the country around it, are well known to be affected with such affections in the autumn; and during the period we passed there, no winter was free from cases of tertian and various other periodical affections. The malarious atmosphere thus prevails at all seasons, although it follows there the usual rule of attaining its greatest intensity in autumn. There is, it is true, no marsh within forty miles of Rome, at which distance the Pontine malaria may be said to lie; but the soil is of a porous volcanic structure, and the heavy rains of the winter and spring, sinking through the superficial stratum, are afterwards drawn upwards through a thick, wild, vegetation, and thus give rise to the poisonous exhalations to which we are accustomed to attribute intermittent affections. Pisa, which is placed at a few miles distance from the mouth of the Arno, has all the intervening land between it and the sea in a positively marshy state. The almost incredible fact, that the city which now stands at least three miles from the mouth of the river was formerly on the "embouchure" itself, is sufficiently attested by historical and existing local evidences of its naval importance as a port, and proves the alluvial nature of the soil, which the river has thrown up between its present site and the Mediterranean.

Without entering on the discussion as to the influence of malaria in checking and curing consumption directly by exercising a counter-influence on the system,—the two states being supposed to be, as it were, incompatible,—we would call attention to the fact that the Italian climate, and perhaps the others which prove most beneficial or palliative in phthisis, are *decidedly malarious*. It is also undoubtedly true, that the two diseases do not often coexist in those localities; no instance of the double affection has ever occurred under our notice during a residence of seven years, while we have seen many of the healthy English community attacked by some form of ague. The fact of hectic fever

being itself distinctly an intermittent affection, and so preoccupying the system, may tell in two ways,—in favour of theorists who hold the indemnity to be due to the incompatibility of the two affections, the one filling up the system to the exclusion of the other, and against them—since it is a well-established observation, that the occurrence of any form of intermittent renders the recipient of the poison more obnoxious to malarious influences than he was before. Leaving the theory to the ingenious, we are satisfied that malarious airs are generally felt by the phthisical to be light, easily breathed, and unirritating. It will be remembered that at Rome and Pisa the air from the marshy districts is *southerly* and in this point *per se*, which implies the absence of northerly and easterly winds, we can see a reason why chest affections with symptoms of irritation should derive advantages from a residence in these cities of the plain. There remains, however, a very important consideration,—that the malarious influence, while it is sedative to the respiratory, is prejudicial to the digestive functions. The failure of appetite, the constipation and resulting languor, and often recurring attacks of bilious derangement, are a *serious set-off* to the advantages which the chest affection derives; and we would call the very active attention of the profession to this point in considering the advisability of exporting patients of the bilious temperament to Italy for any lengthened residence.

[To be continued.]

UNIVERSITY COLLEGE, LONDON.

At the session of the Council on Saturday last the Fellows' Clinical Medals for the best observations on cases at the hospital were awarded as follows:—The Gold Medal for the Winter Term 1849-50, to Mr. Edward Jackson, of Sheffield; the Gold Medal for the Summer Term 1850, to Mr. Thomas George Fitz Gerald; and a Silver Medal for the same Term to Mr. Robert Bowman. The Andrews' Scholarships were fixed as follows:—For competition in October next, a Scholarship of £100, and two of £50 each; for competition in October, 1852, one of £70, and two of £45 each.

CASE OF
**RAMOLLISSEMENT OF THE CEREBELLUM, TOGETHER WITH
 THAT OF THE CEREBRUM;**

WITH FATTY DEGENERATION OF THE
 HEART, AND TUBERCULAR ABSCESSSES
 IN BOTH LUNGS.

BY JOHN DAY BROWN, M.D.
 Strood, Kent.

(Communicated by F. J. BROWN, M.D.)

THIS case is reported because of the small number of instances in which softening of the cerebellum has been observed.

E. K——, æt. 39, married, and the mother of several children, suffered, during the last five years of her life, with occasional attacks of giddiness, pain in the head, and sickness, which were accompanied by numbness in one of the lower extremities. She was bled from the arm, or cupped at the nape of the neck, whenever these attacks occurred. During the months of December, 1849, and of January, 1850, she laboured under symptoms of chronic inflammation of the brain, which closely resembled those observed in idiopathic fever attended by an unusual degree of brain affection. She had scarcely recovered from a miscarriage when these symptoms appeared: her power of memory became impaired; paralysis of the left lower extremity ensued, with weakness of the right leg; and rigidity of the muscles of the lower limbs was occasionally, not permanently present. Emaciation progressed, and, at the period of her death, was considerable. She had an attack of bronchitis on the 7th June, from which she recovered.

On the 19th she was affected by paroxysms of breathlessness, which recurred five or six times during three consecutive days. There was now a rapid loss of strength, and a still further impairment of memory, and the patient died in a paroxysm of breathlessness on the 29th June, 1850, at 4 P.M.

Throughout the day and the preceding night she fancied that she saw deceased persons. Her consciousness never at any time failed her, although

she had a dull, vacant aspect of countenance. She stated, in answer to inquiries, that there was a sensation of "squashing" in the back part of her head; and that, whenever she was in an upright posture, her feelings were as if she must pitch forward.

An examination of the body was made 42 hours after death. The skull was dense, and greatly thickened, particularly anteriorly and laterally. There were about three ounces of serum effused beneath the membranes; the ventricles contained but little fluid; the vessels of the pia mater were dilated; no disease was found in the arteries at the base of the brain; the cerebral convolutions were healthy and moderately firm; there was white softening of the corpus callosum, so that its superior surface tore into the form of an ulcer by the stretch occasioned to it by slicing the convolutions. The fornix and its crura were almost destroyed, and the peduncles of the pineal gland were completely removed by the process of ramollissement; the crura cerebri were in a state of softening in the vicinity of the pons varolii; the superior vermiciform process, and the upper surface of the hemispheres of the cerebellum, were in a state of softening, being ragged and diffuent. The substance of the medulla oblongata was sound.

The heart was affected to an extreme degree by fatty degeneration: it presented the appearance of a gelatinous sac, with a few red fibres in the left ventricular parietes. Coagula were found at the apex of the left ventricle, but none in the ventricle on the right side of the heart.

There were old pleuritic adhesions; the lungs were emphysematous anteriorly, and on section presented numerous tubercular abscesses, some of them of considerable size: these cavities contained thin pus, and had no communication with bronchi.

The liver was enlarged and fatty.

The spleen was actually liquid within its capsule, so great was the softening.

The kidneys were greatly softened, and flakes of cortical substance were peeled off with the capsule.

The intestines and mesenteric glands presented no diseased appearance.

MEDICAL GAZETTE.

FRIDAY, DECEMBER 13, 1850.

THE recently published Report of the Commissioners of Lunacy contains some facts of interest in a medical point of view.* In addition to a statistical summary of the number of lunatics, male and female, confined in the various hospitals, asylums, and licensed houses throughout the country, there are tables showing the circumstances connected with the outbreak of, and mortality caused by, the malignant cholera in those establishments. We shall at present direct our attention to the general statistics of the insane. It appears from this document, that the total number of lunatics falling under the cognizance of the Commissioners, exclusive of single patients under the care of medical gentlemen and others, amounts to 15,079, thus distributed —

Private patients	3,774
Pauper patients	11,305

Total	15,079
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Arranged according to sex, the numbers stand thus :—

	Males.	Females.
Private	1,906	1,868
Pauper	5,168	6,137
	7,074	8,005

It will be perceived that the female are about one eighth more numerous than the male lunatics; and this preponderance is chiefly manifested in the Pauper class.

It appears that about one half of these lunatics are confined in asylums, the other half being distributed in hospitals and licensed houses :—

Asylums	7,140
Hospitals	1,208
Metropolitan Licensed Houses .	2,945
Provincial Licensed Houses .	3,786

* Copy of the Report of the Commissioners of Lunacy, to the Lord Chancellor. Printed by order of the House of Commons: 1850.

It would thus appear that a very large proportion of lunatics, amounting to nearly one-fifth of the whole, are congregated within the walls of the Metropolitan Licensed Houses, and that this number falls only a little below that assigned to the whole of the Licensed houses throughout the Provinces.

As we are in the last year of the decennial census, it is impossible to calculate with accuracy the proportion which the number of lunatics bears to the entire population of England and Wales. Judging, however, from the progressive increase of population, it may be fairly assumed, from the numbers given in the return, that insanity is not on the increase, as some have asserted, but that the cases now bear a smaller proportion to the population than they did at the last census. Its great prevalence among pauper females, as shown by this Report, is worthy of note, as an index of the distress and misery which afflict this class of the population.

The remarks appended to the Return tend to show that the Commissioners have exerted themselves to enforce the provisions of the law for the protection of these unfortunate beings, and they have not hesitated to expose delinquencies, where they have been brought to light. They say :—

“We have now the satisfaction of reporting that the various Institutions for the insane throughout the country are in an improved state, and that the conduct of the Superintendents, Officers, and Attendants, in reference to the treatment of patients and the management of Lunatic Establishments, is for the most part humane and judicious.

In several instances, however, the visiting Commissioners and Justices found it necessary to animadvert upon the excessive use of mechanical restraint; on the neglect of cleanliness; on inadequate ventilation; on want of sufficient attendants; on improper or deficient diet; on the dirty condition or scanty supply of bedding and clothing.

on irregularities in the medical books; and on other defects. And in about seventy instances these defects were deemed of sufficient importance to require the special interference of this Board, by whose direction letters were addressed to the parties inculpated, the effect of which has been that the defects thus noticed have been wholly or partially remedied."

In some few instances the defects of management were so glaring that the Commissioners have felt it necessary to enter into some details respecting them. Three licensed houses were especially noticed, in which it appears the Visiting Commissioners discovered that it had been the practice to place clean sheets on certain beds during the day, and to remove them at night, thus giving a fictitious appearance of comfort to the patients' beds. In one of these establishments the neglect of the patients was so gross that the Commissioners recommended the withdrawal of the license. In reference to metropolitan asylums and hospitals, the Commissioners observe:—

"In our last Annual Report we took leave to call attention to the burial-ground adjoining one of the airing-yards of St. Luke's Hospital, and expressed a hope that the Bishop of London would eventually interfere in the matter. We have now the satisfaction of stating that his lordship did interfere in the most prompt and decided manner; and that, in consequence thereof, this objectionable burying-ground (which contains only 1,000 square yards, and in which 200 bodies, on an average, were annually deposited) was closed in the month of August last."

Guy's is, we believe, the only London hospital for the general reception of patients to which lunatic wards are attached; and, with respect to these, the following observations occur in the report:—

"In the Annual Report, dated 30th June, 1848, we had occasion to notice the defective condition of the wards appropriated for the reception of insane

females in Guy's Hospital. Special reference was at that time made to the want of baths, the offensive atmosphere in the rooms, the insecure state of the window-fastenings, and also to the fact that three female patients were found by the Commissioners, at the time of their visit, in a state of perfect nudity, lying in the loose straw which constituted their bedding.

"It is satisfactory to report that, in all the particulars referred to, considerable amendment has taken place. Suitable baths have been fitted up, and bedding of a superior description has been lately provided. We have also to observe that a material improvement in the condition of this ward, and the treatment of the patients, is manifest. The amount of mechanical restraint to which they were formerly subjected is now greatly diminished, and efforts are made to increase their comfort, and to induce them to engage in suitable occupations.

"We are informed that an additional day-room is about to be appropriated to the use of the patients. This addition will afford a better means of separating and associating the inmates, and will tend materially to improve their condition and promote tranquillity.

"Notwithstanding these various additions and improvements, it is our duty to add that the ward appropriated to the insane in Guy's Hospital must, in our opinion, *owing to its situation*, continue to be very defective in several important requirements, and fail to afford the inmates many of the advantages which are calculated to alleviate disorders of the mind."

It appears further, from the Report, that the number of female lunatics now confined in these wards amounts to 22. From the remarks made, it is to be inferred that the Commissioners object to the locality of this Hospital-asylum, and they throw out a suggestion that its removal to another spot would be attended with benefit to the lunatics. There may be some reason in this suggestion; but the terms of the will of the founder, confirmed as they have been by an act of Incorporation, do not permit such a change, except by the intervention of the Legislature.

The newly-established *Asylum for Idiots* is thus favourably spoken of:—

"In continuation of our last Report relative to the provision made for the care and training of idiots as a separate class of insane, we have the satisfaction of stating that the comforts and advantages of the benevolent institution, Park House, Highgate, and the benefits conferred thereby upon the unfortunate class for whom it is designed, have been justly appreciated by the friends of the patients (who are more properly termed pupils) and by the public. This Board have received from two of their number who recently visited the Asylum a detailed report, showing that considerable progress has been made in carrying out the objects and maturing the plans of the institution. It appeared, at the same time, to the Visiting Commissioners that the system admitted of further development and improvement, and they made several practical suggestions with that view. A copy of their Report has been transmitted, by order of this Board, to the Committee of the Asylum, who, the Commissioners feel assured, will give every attention to their suggestions.

"It only remains to state that the accommodation afforded by the present premises at Highgate having proved inadequate to meet the increased and increasing numbers of applications for admission, it has been determined to extend the buildings: in the meantime, advantage has been taken of a very eligible offer of suitable premises at Essex Hall, near Colchester, to form a branch establishment in connection with that at Highgate—a temporary arrangement which received our concurrence; and we have felt justified in considering and treating the two establishments as one institution."

It is a remarkable fact that the cases of *suicide* have not exceeded eight during the last year, and the Commissioners justly observe:—

"Considering that the total number of lunatics in asylums in the country is not less than 15,000, and that the ancient system of mechanical restraint has in many institutions been altogether abandoned, and in most others exceedingly diminished, we cannot but consider that the number of deaths by suicide is

smaller than might have been anticipated, and that the fact is highly creditable to the superintendents, medical officers, and assistants, of the various establishments for the insane, to whose vigilance and care this result must be mainly attributed. Of the suicides referred to, six—viz., three males and three females—were by strangulation, one male by cutting his throat, and a female by drowning. In the last-mentioned case, which occurred in the metropolitan district, the lady referred to escaped from her attendant while walking with her in the country, secreted, and afterwards drowned herself."

In conclusion, the Commissioners advert to the unprotected state of the property of lunatics of the middle classes:—

"The important subject of the protection and administration of the property and income of lunatics under certificates has continued to occupy much of our attention, and we have had frequent occasion to regret the very defective state of the existing law, more especially as respects the property of patients, tradesmen, and others, of small means, or whose mental malady is likely to be only temporary. We earnestly hope that this subject will receive your Lordship's early consideration."

Anything more unjust than the law of England in this respect, it would be impossible to conceive. The cost of a commission in its cheapest form in an undisputed case is not less than three hundred pounds; and where the property is of sufficient magnitude, the costs are easily made, by process of law, to expand in a direct proportion to the estate of the lunatic. Thus the expenses of commissions among the higher classes are rarely under one thousand, and sometimes reach to five thousand pounds! The charge made by the Court of Chancery on the relatives of a tradesman for placing his property under their safe custody, and preventing waste, as we are informed, is at the lowest amount *three hundred pounds**, although

* In Elmer's "*Practice in Lunacy*" the cost of finding whether a man be lunatic or not is set

his lunacy and incompetency may be so palpable that a common jury of twelve of his neighbours would settle the question in five minutes. The result of this infamous system of extortion is, that, comparatively speaking, few commissions are taken out. The relatives prefer incurring any risk rather than seek the so-called *protection* of the Court of Chancery. In fact, it is only where property belonging to the lunatic is worth the risk, trouble, anxiety, and expense, which inevitably attend Chancery proceedings in lunacy, that an inquisition is sought for. In the meantime, dishonest persons may refuse to pay debts to a lunatic's estate. The dividends on investments in Government Funds or other securities, rents of estates and house property, and funds at bankers, are withheld; for no one can give a legal receipt except under an order of the Court of Chancery. This renders an inquiry necessary, with the payment of extortionate fees, for the support of an antiquated system of injustice.

The working of such a system is clearly shown by this Report. If we leave out the pauper class of lunatics altogether, and set down the numbers confined during the past year at 3774, there were only 238 cases in which inquisitions were held—i. e. among *eleven* persons above the pauper class there is only *one* who has been found lunatic by inquisition! In many of these cases a charge of three hundred pounds, or even one half of this sum, for answering a very simple question, is doubtless so disproportionate to the means of the lunatics that it cannot be incurred. We do not see why a simply constituted Court, with one competent Commissioner, should not be allowed to decide these cases cheaply and speedily.

down at £123. 2s. 6d.; but this must be the hypothetical cost. The period which the Court of Chancery is supposed to take in order to decide this question is *six months*; but this must be the hypothetical period.

The relations of a poor lunatic have as great a right to protection as those of a wealthy patient. The creation of a money-test deprives them entirely of this right, and often throws upon them, in addition to vexatious delays, an unnecessarily heavy charge for the support of the lunatic.

Reviews.

Remarks on Insanity: its Nature and Treatment. By HENRY MONRO, M.B., Oxon, Fellow of the Royal College of Physicians. In Two Parts. Part I. Pamphlet. 8vo. pp. 75. London: Churchill. 1850.

THE object of these "Remarks" by Dr. Monro is to prove that insanity is a disease of deficient nervous tone consequent on loss of vitality. The argument is introduced by observations on the classification of the insane. All cases of insanity are ranged by the author under three divisions:—"1. Acute stages of active insanity; 2. Chronic stages of active insanity (both of these stages manifesting varieties of form)—namely, in some, general delusions, in others partial; in some, great excitement, in others great depression; in some, intermitting paroxysms, in others continued; in some, intellectual derangement, in others only moral; and 3. The imbecile stage, or the state of wreck after the storm has passed." This classification the author thinks best adapted to treatment.

Dr. Monro treats at considerable length of the corporeal nature of insanity, and in this part of his work shows its dependence upon a state of depression of the nervous centres, establishing his position from a consideration of the physical condition of the insane. In the second chapter the author proceeds to the question, whether there is anything in the mental phenomena of the insane which could not be accounted for on the results of loss of nervous tone. This inquiry is pursued by a consideration of the most common mental phenomena of the insane, by a comparison of those mental conditions which are very similar to insanity, and by the consideration of

the acknowledged causes of those conditions, as well as of the mode of access of those causes. This portion of Dr. Monro's pamphlet is one of the best expositions of the subject that we have met with, being written in a peculiarly clear style, and presenting a picture faithfully drawn from nature. The conditions approaching to that of insanity are sleep, dreaming, somnambulism, waking trance, voluntary abstractions, transporting passion, the mental state of the infant, the mental state of old age, delirium caused by external agents, delirium occasioned by poisonous matter of internal origin, and, lastly, delirium caused by mechanical pressure.

This part of the work closes with the proofs of the loss of vitality and the causes of this nervous deficiency.

This is an extremely well-written essay, and well deserves the perusal of professional men, whether especially engaged in the treatment of the insane, or occupied in general practice. It is not undeserving the attention also of educated non-professional persons, on account of its intrinsic literary merits. We hope that it will not be long before the second follows this first instalment of so valuable a practical treatise.

Observations on the Treatment of Phthisis Pulmonalis. Pamphlet, 8vo. pp. 23.

Lectures on Clinical Medicine. Parts I. and II. Pamphlet, 8vo. pp. 92. By JOHN HUGHES BENNETT, M.D., F.R.S.E., Professor of the Institutes of Medicine, and of Clinical Medicine, in the University of Edinburgh. Edinburgh: Sutherland. London: Simpkin and Co. 1850.

THESE essays have already appeared in one of our contemporaries: they have received very considerable additions before their publication in a separate form, and will be followed by others of a similar character.

The first essay consists of an excellent practical exposition of the value of judicious hygienic treatment in arresting the course, or preventing the development of phthisis. This pamphlet has two faithfully-executed coloured engravings of specimens of morbid anatomy of phthisis.

The second contains clinical lectures on cases illustrative of the processes of simple, cancerous, and tubercular ex-

cessions, with their pathology and treatment; on pneumonia, cancerous exudation into the liver, cancerous ulceration of the oesophagus, simple stricture of the pylorus, profuse hæmatemesis, aneurism of the aorta, and plithisis.

The third comprises Lectures on the Classification and Diagnosis of Cutaneous Diseases.

Each subject is fully illustrated by cases and woodcuts. These publications being already known to many, and emanating from a pathologist of established reputation, require little more than this announcement of their publication in a separate form.

The Nature and Cure of Consumption and Indigestion, Scrofula, and Nervous Affections. By G. CALVERT HOLLAND, M.D., late Physician to the Sheffield General Infirmary, Bachelier es-letres of the University of Paris, &c. &c. 8vo. pp. 208. London: Orr and Co. 1850.

Cases illustrative of the Cure of Consumption and Indigestion. By G. CALVERT HOLLAND, M.D., &c. &c. Small 8vo. pp. 104. London: Orr and Co. 1850.

We were rather surprised on perusing the first of these two works to find that, according to its author, the medical profession is deplorably ignorant of the influence of the nervous system on the functions of digestion and nutrition, while Dr. Holland alone appears to have discovered that this system has a "widely-pervading influence" on the phenomena of life. He tells his readers that there are few disorders that cannot be brought within the comprehension of the general reader: the comprehensions, however, of the members of the profession to which Dr. Holland belongs are of a different order, and cannot so readily apprehend the phenomena of disease! Such, at least, we gather to be Dr. Holland's opinion. We suppose, however, that we must except the mind of Dr. Quin, the homœopath, to whom Dr. Holland dedicates his book with great laudation. The object of the first of these publications is obviously beyond the pale of the profession, as it is addressed to the public. Let the public, it judges: we shall

tion upon it.
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eye. It sets forth the virtues of tar-water and cod-liver oil in consumption, and of a combination of hydropathy with medical treatment and dietetic rules in the case of indigestion. *Valent quantum valere potest!*

On Bengal Dysentery and its Statistics; with a Notice of the use of large Ene-mata in that Disease, and of Quinine in Remittent Fever. By JOHN MACPHERSON, M.D. Pamphlet, 8vo. pp. 63. Calcutta. 1850.

FROM 1830 to 1850 there were admitted into the General Hospital, in Calcutta, 2,044 cases of dysentery, of which 457 or 22·3 per cent., died. The average mortality from the disease was, at Bombay, 18·3 per cent., and at Madras, 30 per cent. among civilians, and only 5·3 for the military. Very full tables are given by the author, which justify his conclusions.

The structural changes produced by the disease are also presented in tabular statements. On a comparison of these statistics of *acute* and *chronic* dysentery, it appears that the liver is more frequently disordered in the latter form of disease, in which also it is more generally small and indurated, while in the acute form it is enlarged and soft. The spleen is more frequently enlarged in the chronic disease.

Our brethren in India are among our most industrious cultivators of pathology. The obligations under which they have placed the medical profession by their labours, is further increased by Dr. Macpherson's pamphlet.

The Accommodation of the Eye to Distances. By WILLIAM CLAY WALLACE, M.D. Pamphlet. 8vo. pp. 36. New York, and London: Wiley. 1850.

THE author minutely describes many structures in the eye of man and the lower animals by which the adjustment to distance is effected. We extract, verbatim, the author's description of one apparatus by which this is mainly performed, and to the discovery of the use of which he lays claim:—

"The spherical lens is suspended by a ligament which is formed by the membranes of the vitreous humor, and is sometimes covered with pigmentum nigrum. Beneath the crystalline, there is a muscle (the campanula Halleri) which, though it

varies in shape and size, is very conspicuous in the halibut, the dolphin, and the striped bass. In the halibut the muscle is hatchet-shaped, and proceeds diagonally from the lower pole of the crystalline body to be attached slightly to the uvea, but firmly to the anterior and lateral portions of the membranes of the vitreous humor. When the muscle, which is supplied by a large branch of the third pair of nerves, contracts, the crystalline approaches the cornea; and when it is relaxed, the crystalline is drawn back by the elasticity of the membranes of the vitreous humor, some of which passing through the retina at a line that, in animals furnished with spherical lenses, divides the lower portion of the tunic, are firmly fixed to the choroid.

"The campanula Halleri has by some been described as the ganglion of the crystalline; by others, as a cartilaginous body: but, so far as I can ascertain, its muscular character, its passage through the loop at the back of the iris, its attachments and use, were unknown until 1834, when I pointed them out." (pp. 9-11.)

We may observe that this monograph contains also minute descriptions of other parts in the structure of the eye. We regard it as a very useful contribution to anatomical science.

Proceedings of Societies.

MEDICAL SOCIETY OF LONDON.

Nov. 30, 1850.

DR. J. R. BENNETT, PRESIDENT.

Inhaling Apparatus.

DR. SNOW exhibited and described the method of using some apparatus for the administration of various volatile medicines by inhalation, and in reply to questions as to the dose and temperature to be used, stated that he had come to the conclusion that about half the quantity of material placed in the instrument became absorbed by the patient, whilst the remaining part was expired, and consequently lost; and that such drugs as opium and stramonium required a temperature between 360° and 410°, but that hydrocyanic acid, chlorine, and the like, could be given without artificially heating them.

Cauliflower Excrescence of the Os Uteri.

DR. CORMACK read a paper upon cauliflower excrescence of the os uteri, illustrating the subject by a case from his own practice.

A patient, æt. 30, mother of five children, whose general health was good, had an attack of menorrhagia in September 1847, and was treated with advantage by tannic acid internally. She was pregnant at the time, and there had been, during the first month of her pregnancy, a slight discharge.

All went on well till the 28th of November, when the author was summoned to attend her, she believing herself to be parturient. There had been but little pain, although a great discharge of watery fluid had taken place, which was gradually followed by alarming hæmorrhage. The author, anticipating a placenta prævia, made a digital examination, and discovered a tumor of considerable size affixed to the anterior lip of the uterus, the os itself admitting only the tip of the forefinger. The author injected a strong solution of sulphate of alum, plugged the vagina with compresses soaked with the same, and administered an opium enema by the rectum. By these means the bleeding was arrested, and the patient obtained some repose. At 6 A.M. of the following day the pains became sharp and regular: the tumor had diminished much in bulk; it had shrunk almost to a roughness, and was not tender. After eleven hours, no change having taken place in the meantime beyond the drain of dirty, offensive fluid, the labour pains became more frequent, but not very effectual. At 8 P.M. there was sudden discharge of blood. The author punctured the membranes and exhibited ergot. In an hour a healthy living child was brought forth; a little brandy and tea was given to the patient, and she was left in charge of her attendants with a tolerably good pulse.

On recovery from her confinement, the tumor still remaining of its original size, the author, with the concurrence of Dr. Henry Bennet, proposed removal by the knife, determining to take away also at the same time the portion of the uterus to which it was attached. To this the patient did not consent. Mr. Fergusson's opinion on the matter was taken, and, in accordance with that opinion, no operation was then resorted to.

Matters continuing to become more unpropitious in spite of the employment of local and constitutional astringents, the vagina being excoriated, on the 6th of February, 1848, Mr. Fergusson excised the diseased parts with temporary relief. In two months time fresh excrescences had sprouted forth, the bleedings reappeared, and the accompanying pain required enormous doses of morphia to allay it. On the 17th of July the sufferer died during an attack of hæmorrhage.

The post-mortem examination showed

the vagina to be ulcerated, the uterus to be enlarged and cartilaginous, and having its internal surface covered with mammillations about the size of a pea, whilst no disease existed in the urethra, bladder, or ovaries. The author expressed his opinion that the operation should have been performed at a much earlier date, but that, from the slight pain accompanying the disease at its commencement, the patient would not submit. He drew attention to the styptic powers of gallic acid when given internally, and stated that in weighing the question as to whether the disease could be successfully removed by the knife or ligature, he kept in view the results of Dr. Simpson's experience, which was of a nature favourable to putting in force operation.

Dr. Andrew Anderson's theory as to the nature of cauliflower excrescence the author considered as countenancing surgical interference.

Dr. MURPHY said two points of interest were contained in the paper just read. First, the absence of any inconvenience during pregnancy, the disease appearing to be in abeyance; and, secondly, the mode of treatment: his opinion was averse to operation. In one case of his own the patient died three weeks after the operation; in a second, three months after the operation; whilst a third was carried off by peritonitis. He also remarked upon the result of the pressure upon the tumor.

Dr. HENRY BENNET made some observations in respect of the nature of cauliflower excrescences. He considered it as proved that there were two kinds—one which was vesicular, and another which was distinctly malignant, and had been described by Dr. Clarke under the name of cauliflower excrescence. The former might be successfully removed, whilst the latter was almost sure to return. In his own practice he only met with one case of the vesicular form, which he ligatured without permanent benefit. He had seen three cases of the malignant polypoid variety, one of which he attended with Dr. Murphy. The tumor was removed by ligature: however, the issue was fatal. Nevertheless, he should not hesitate to perform the operation of extirpation, as there were cases on record which were said to have had a happy termination. He stated as fact that cancerous disease of the uterus had never come under his notice in its first stage of development, and that he had never seen a clear case of the disease end otherwise than fatally.

Mr. STILLMAN related the particulars of a case of fatal hæmorrhage from a cauliflower excrescence in a woman, mother of one child. The hæmorrhage took place

suddenly without previous warning, and caused death in a very short time.

Mr. I. B. BROWN considered operation justifiable if there were any chance of prolonging or rendering more comfortable the patient's existence. He begged to call attention to the diminution caused in the tumor by the pressure of the plug introduced into the vagina.

Mr. HUBB expressed a strong opinion against the propriety of operating. He thought the members had been discussing two diseases under the same name. There were clearly two forms of cauliflower excrescence—one malignant, and the other not so.

Mr. P. MARSHALL said he had seen many cases of the disease: he had always found them unmanageable. He related a case of malignant disease which remained in abeyance during pregnancy, but which, after the patient's confinement, speedily terminated her existence. He differed from Dr. Bennett in respect of the recognition of malignant disease of uterus in its early stages. He had more than once traced malignant disease of uterus from its commencement.

December 7.

Dr. SNOW exhibited

An Enlarged Heart,

which measured 15 inches in circumference, $5\frac{1}{2}$ from base to apex, and weighed, after being carefully emptied and washed, one pound four ounces. There was but slight valvular disease. The man from whom it was taken died at the age of 52: was a porter of intemperate habits. The lungs were emphysematous. The kidneys were in a state of granular degeneration. Anasarca and albuminuria had existed for two years.

Mr. HENRY SMITH brought before the Society a specimen of

False Passage in the Urethra, leading to the Bladder just above the Natural Orifice,

and made some observations respecting the complication this state of matters would make in the performance of Mr. Syme's operation for the relief of stricture.

Mr. CANTON spoke of the great value of Avery's urethral speculum in such cases, as with that instrument the surgeon could distinguish the aperture of the stricture into which he was about to pass the sound.

Mr. CHIPPENDALE and Mr. BORLASE CHILDS spoke with great confidence of *their own skill* in passing the catheter, and therefore considered operation unadvisable.

Mr. COX read a paper upon

Laryngismus Stridulus, or Crowing Inspiration of Infants.

The author gave a concise *resumé* of the views held by the most esteemed writers upon the subject. From his own experience, he was led to infer that the disease was more frequent in females than males; that children of strumous habit were peculiarly liable to it; that artificial feeding, by affording only insufficient nourishment, was a predisposing cause; that enlarged thymus was only a very rare cause of the disease.

He considered the analogy between laryngismus stridulus and epilepsy to be very strong.

1st. The symptoms were in some measure similar.

2d. That patients subjects of this disease in infancy sometimes became affected with epilepsy in after-life. He had seen instances of this.

3d. That epileptic seizures were often preceded by laryngeal spasm.

The author considered a retardation of the pulse as premonitory of an attack: he had noticed the pulse fall from 115 to 80 or 85.

In the treatment of the disease he had found chloroform very useful in removing the suffocative spasm: he had never met with any inconvenience from its use: he administered the vapour by placing 20 or 30 drops upon a piece of lint.

General tonic treatment he found the best preventive against the recurrence of attacks, especially in scrofulous children.

Hydrocyanic acid had proved in his hands most efficacious in curing the disease after it had become centric, when an irritability of the spinal cord had been set up.

The author brought forward twelve cases.

In one there was congestion and inflammation of the meninges of the cord, from the foramen magnum to the 5th cervical vertebra, arising from a fall across the back some time previously: in another, aneurism of the arch of the aorta involved the recurrent laryngeal; the child was four years old. In five cases artificial feeding probably had been the predisposing cause.

Dr. SNOW suggested, from the dangerous nature of chloroform when in the hands of the inexperienced, that it should be diluted with alcohol, and left with the friends of the patient, in order that it might be given on the instant of the attack.

Dr. GOLDING BIRD had found colchicum very useful in the treatment of the disease, especially in children of a plethoric habit: he would be afraid to employ chloroform.

Dr. Willehire, Mr. Hind, Dr. Crisp, Mr. Dendy, Dr. F. Winslow, spoke upon different points in the author's paper.

HARVEIAN SOCIETY.

Fatal Case of Carcinoma of the Ileum, from Thickening of its Lower Portion at its junction with the Cæcum, caused by Cephalomatous Deposit underneath the Mucous Membrane, with a large Tumor of the same character imbedded in the Folds of the Omentum, which surrounded and compressed the Caput Coli. By DR. POWELL.

E. M., æt. 60, married, of nervo-lymphatic bilious temperament, formerly lived at Acton, stoutly made, was subject to torpid bowels from her youth, especially since her confinement some twenty years past, which required the daily use of aperient medicine. This became aggravated about six weeks previous to her death, but she was not confined to bed for more than a week previously, from which time she had no evacuation from the bowels. Had been treated by calomel and croton oil, followed by cathartic mixture and injections of turpentine and olive oil, but without effect, all medicine and aliment being rejected, and soon attended with feculent discharge from the stomach. Seen by me three days previous to her death. I found her much reduced, with constant vomiting; red, dry, chapped tongue; pulse frequent, small, compressible; countenance pinched; urgent desire to pass urine; not much pain on pressure over the abdomen, which was tympanitic, except over the right iliac region, which was dull on percussion; no abdominal tumor to be detected. Introduced catheter, after the use of a hip-bath, which gave much ease, but without bringing away any urine. Ordered fomentations with Ol. Terebinth. Introduced up the rectum œsophagus tube nearly a foot, and injected two or three pints of warm water, till the sense of pressure became inconvenient, but without bringing away any feculent motion.

Prescribed small doses of calomel and croton oil, with effervescent mixture, every second hour, also mercurial and camphor frictions every sixth hour. Next day found the patient more exhausted, with constant vomiting, very restless; pain over abdomen more decided, with increased distension; no action from the bowels; passed a little urine; pulse weaker and less frequent; patient quite collected, though very restless. Ordered to continue the mercurial frictions, and the powders in smaller doses, with half a grain of opium, and the effervescent mixture; beef-tea and a little brandy and water as often as tolerated; beef-tea injections, with olive oil, every four hours, with occasional fomentations to the abdo-

men. Third day; moribund, but conscious; rejected all medicine and aliment, cold water alone grateful; pulse almost imperceptible. Patient took one more powder, with a quarter of a grain of Acet. Morph., which quieted the stomach and relieved abdominal pain: died in the night (66 hours after I first attended her) perfectly conscious.

Post-mortem examination 36 hours after death. — Epiploon descending over the cæcum, containing a dense tumor, some six ounces in weight, in its interstices, surrounding and compressing the bowel. When incised the tumor was found to be cephalomatous. The lower ileum, some inches from the ilio-cæcal valve, was much thickened, and contained solid fecula. Mucous membrane dark, coats infiltrated with soft cancer. The cæcum distended with solid fecula. The folds of intestine injected at their free edges. The peritoneum injected with encephaloid deposit underneath; some slight serous effusion.

REMARKS.—How futile are all therapeutics with a *curative* view, how obscure and unsuccessful any diagnostic or *operative* procedure, the preceding case well illustrates. The fatal occlusion of the bowel was *directly* owing to the effects of cold, inducing febrile disturbance and intestinal congestion. The death was more remotely the result of malignant deposit of slow growth. The lower portion of the ileum and cæcum will probably be found the seat of obstruction in most cases of ileus productive of mechanical arrest, where no immediate cause is in operation. Preventive means are the chief safeguard; violent measures useless and objectionable, as aggravating both the disease and the patient's suffering. Surgical operation is rarely admissible, and past experience by no means recommends its further trial.

Carcinoma of the Right Lung.

I was called to visit a woman, aged 74 years, on the 26th of November, 1850. She had been an old servant, of middle sanguine temperament, and highly abstemious habits, some members of her family having died of phthisis pulmonalis. She had had an attack of acute rheumatism some two or three years previously, being subject to so-called asthmatic attacks, and to conjunctival inflammation. About three months before my visit I was informed that she ejected from the mouth a considerable quantity of florid blood; and, a day or two subsequently, took cold from exposure to rain, which brought on this fatal illness. I was told that the medical gentleman in attendance was of opinion that no disease of the lungs existed, but that her

heart was the seat of her ailment. From further queries I learned that she at first complained (after exposure to cold, &c.) of severe pain in the right side, which subsided after a little time; and that, after five or six weeks, her medical man discontinued attendance (resuming it prior to her death), partly as she was averse to ordinary medicines (more homoeopathy!), and because small benefit seemed to be derived from the means used, which consisted of little else than castor oil, and mustard poultices to the chest, with some pectoral mixtures, &c. I found the patient dozing, and was told that she had been and continued to be delirious for some days previously, the delirium being of a low, wandering character. She was roused up, and answered questions, though imperfectly: her posture, which was half-raised in bed, she was obliged to retain, as being subject to paroxysms of orthopnea; her countenance was somewhat sullen; lips of a bright vermilion colour; pupils contracted; head cool, and little action in the temporal arteries. The feet and ankles were slightly cedematous; skin hot, and very dry; pulse about 100, small, compressible, and regular; tongue slightly furred, and readily protruded. There was difficulty of deglutition, and thirst; no pain on pressure over the abdomen, nor was the respiration abdominal; the bowels not being acted upon for some days, and the urine being high-coloured and rather scanty. On exploring the chest (the patient being with some difficulty placed in a suitable position, and otherwise unfavourably circumstanced for examination) the respiratory movements did not present much dissimilarity on either side; being, however, slightly diminished on the right: no dilatation was present on this side, either of the intercostal spaces or parietes generally, which, however, seemed to remain in a partially fixed state, neither contracted nor dilated. On percussing the chest, complete dullness, both from light and heavier percussion, was found to exist, extending from the clavicle to the 6th rib, and from the sternum to the scapula, closely following the inferior boundary of the right lung. The left side of the chest contracted remarkably in clearness, commencing accurately from the left half of the sternum, with the right side. The stethoscope detected little or no breath-sound over the right half of the chest, save a slight whiffling rale near the mid-sternum, and between the scapulae, bronchial respiration and bronchophony being absent, as well as vocal vibration. The respiratory murmur was puerile and slightly rough over the left lung, and the cardiac sounds were natural in situation, extent, and character. There was but little cough, and

slight expectoration, which, I was informed, was never sanguinolent, but opaque and viscid. Such was the previous history and present condition of the patient, collected as well as circumstances permitted. Now there were no less than six distinct abnormal states of the chest which would give the most marked physical aberration here present—viz., dullness on percussion. First—did it arise from pleuritic effusion? The accession of the attack, with pain in the side, and febrile symptoms, without much cough or expectoration, and the absence of bronchial respiration, vocal resonance, or vibration, seemed to favour this conclusion; but there was no intercostal or parietal dilatation from effusion, which most probably there would have been, from the continuance of the attack for three months, unchecked by constitutional treatment. Again; the thoracic parietes performed, though imperfectly, the normal movements; and, although the patient had had acute rheumatism, no organic disease of the heart remained to account for those congestive and effusive sequelae usually resulting from cardiac disease. Moreover, no displacement of the heart took place. Secondly: was the dullness consistent with the existence of hepatization of the lung from pneumonia? Although some previous and present points in the case—such as the probable supervision of pneumonia on hæmoptysis, &c.—might support this supposition, yet its protracted continuance, with undiminished dullness, nearly total absence of all sound derived from the respiration or voice, as well as the very general diffusion of the dullness over the entire right half of the chest, induced me to negative this conclusion.

Thirdly, was this state consistent with cirrhosis of the lung? a pathological condition so well described by Dr. Corrigan, of Dublin. This was negatived by the heart or lung not being dragged over to the affected side of the chest, accompanied with characteristic signs, and by the case being attended with acute symptoms, and still no pectoriloquy or breath sound present.

Fourthly, did the dull sound arise from malignant deposit in the lung, which might have given rise to the hæmoptysis, and congestion of the pulmonary or pleural tissues? But how account, under this supposition, for the great extent of the morbid sound on percussion, and for the absence of bronchial respiration, voice, or thrill? and the aspect of the patient did not strike me as that of carcinoma.

Moreover, fifthly, the patient's age rendered the existence of tubercle to this extent improbable.

Sixthly and lastly, was the dullness on percussion the result of pulmonary hæmorrhage?

hage or apoplexy? This, on the whole, seemed the most likely interpretation of the case. There was, however, no sanguineous expectoration throughout; and could effusion or consolidation of the pulmonary tissue possibly occupy the entire lung? I concluded, finally, that slight pleural effusion existed, as a consequence of preceding inflammation, attended with pulmonary congestion, succeeding to malignant deposit in the lung.

This view taken of the case was interesting more in relation with the diagnosis than as to the treatment, as it was evident that the patient's state was hopeless, and now insusceptible of remedy. A few doses of Hyd. c. Creta, with Pulv. Doveri, followed by Castor oil; a blister on the right side; a supporting diet, with a mixture of Carb. Ammoniac, Tr. Scilla, Tr. Camph. Comp., in Camphor Julep, constituting the treatment prescribed. The patient died two days subsequently, in a fit of orthopnea.

The necropsy (thirty-six hours after death) cleared up much of the obscurity of the case.

On raising the parietes, the right lung was found adherent to the internal aspect of the sternum; it presented a whitish mottled appearance, and felt quite solidified from top to bottom. A vertical section disclosed a nearly uniform and extensive layer or deposit, resembling soft cartilage, and white marble in colour, the growth nearly replacing the pulmonary tissue, and appearing, as Dr. Symonds observes, "to have monopolised all the nutriment sent to the normal tissue, which has in consequence disappeared before it, and of which it has taken the mould."²

A section of the pulmonary tissue, not having any carcinomatous deposit, sunk in water, and seemed much congested, and of a dark colour; the smaller bronchial tubes were obliterated. The left lung was free from deposit, but was slightly congested, though a portion floated in water. Slight effusion existed in the right pleura. The heart was quite healthy, and of normal dimensions; as was the liver, save being a little deeper in colour than usual. No other parts were examined.

We could now account for many of the obscure circumstances attending this case. The perfect and extensive dulness on percussion was produced by the general carcinomatous deposition in the right lung, conjoined with its adhesion to the sternal pleura. Neither bronchial respiration nor bronchophony was heard (no more than in pleuritic effusion, from the non-transmission of vibrations through a fluid medium), in

consequence of the obliteration of the bronchial tubes. But as there was little effusion, no intercostal bulging or distension of the thoracic parietes existed, and the lung continued, though imperfectly, to follow the respiratory movements, the febrile symptoms were accounted for by the congested state of the non-cancerous portions of the right and left lung, and by the slight pleuritic implication. So that a conjunction of the characteristic signs and symptoms of the several affections passed in review, was accounted for in a pretty satisfactory manner by the result of post-mortem inspection, which proved the partial existence of three out of the six states above referred to.

The case affords an instructive instance of the complicated form in which disease may present itself; and forcibly indicates the necessity of our opinion as to the nature of any particular case coming under observation, being based on a comprehensive review and consideration of the past and present state of the patient.

ACADEMY OF MEDICINE, PARIS.

Nov. 20, 1859.

Scarification of the Neck of the Uterus in Leucorrhœa.

DR. MAYER, of Belfort, transmitted an essay on this subject, and on the use of a new scarificator, in symptomatic congestion of the uterus.

The author concluded:—1. That there is a form of leucorrhœa dependent only on uterine congestion. 2. That this form of leucorrhœa requires local depletion. 3. That for this depletion leeches are insufficient. 4. That they may be superseded by scarification. 5. That an instrument which he has contrived for the purpose merits the attention of practitioners.

On the Employment of Impermeable Vaseline in Inflammations of the Skin.

M. ROBERT LATOUR addressed a letter to the Academy, in which he complained of the little notice that had been taken of a communication submitted by him on the above-named subject, in the month of March last. After enumerating the diseases in which the treatment he advises has been found useful, he concludes with the following propositions:—1. That animal heat is dependent on the capillary circulation. 2. Inflammation is an "hydraulic animal fact" subordinate to calorification. 3. To attack inflammation the animal heat must be attacked in its essential elements.

4. The experiments of M. Fourcault have proved that the immediate action of the air on the skin is essential to calorification.

5. The first indication in the treatment of inflammation of the skin is to check calorification by preventing the access of air.

Aneurismal Varix cured by Ligature.

M. ROUX communicated a case of aneurismal varix operated upon successfully. The disease was seated on the left arm, the whole venous system of which it embraced: it dated six years. M. Roux placed a ligature on the brachial artery, both above and below its relations with the tumor. The result had been successful.

M. Roux added the following remarks, deduced from five cases:—Aneurismal varix may be either spontaneous or traumatic: its most frequent cause is puncture of the artery in venesection: it is more frequent on the left than on the right arm.

SURGICAL SOCIETY OF PARIS.

Nov. 27, 1850.

Considerations on Calculi of the Prostate.

M. CHASSAIGNAC made some observations on a case of vesico-prostatic calculus mentioned by M. Vidal at a preceding meeting. The following is a summary of M. Chassaig-nac's remarks:—

In patients presenting calculi in the prostatic portion of the urethra the prostate exhibits a state of atrophy. The calculi are maintained very firmly in the pouch which encloses them. The lateral operation is to be preferred to any other for their extraction, while lithotomy is excluded. In performing this operation, the atrophied state of the prostate must be borne in mind. The calculi must be removed with a scoop, and not with forceps.

M. Vidal divided these calculi into vesical and prostatic, according to their origin. M. Vidal did not concur in M. Chassaig-nac's recommendation to have recourse always to the lateral operation, neither did he disapprove of the use of forceps. A short discussion followed, embracing a repetition of the preceding points.

Medico-legal Considerations on Gun-shot Wounds.

M. RIGAL (of Gaillao) submitted the following observations:—

The questions raised by the case of suicide related to the Society on the 20th inst. by M. Deguise, induced the author to think that a notice of several cases which had occurred in his own practice might be acceptable to the Society. M. Rigal then proceeded to their relation:—

1. Le Sieur Causeé, *propriétaire*, about forty-five years of age, residing at Bernae, had been for several weeks the subject of melancholy; afterwards he became possessed with jealousy and groundless suspicions of thieves. One evening, after having been in search of an imaginary thief, he returned home and seated himself at the fire-side between his wife and their nurse and infant child. While his wife was in the act of stooping forward to prepare his beverage, he placed the muzzle of his double-barrelled fowling-piece to her body and discharged the two barrels in rapid succession, laying her dead at his feet. The murder thus perpetrated, Causeé delivered himself up to justice.

On the following day M. Rigal and Dr. Thomas arrived with the magistrates to make an examination of the body. On the posterior aspect of the right side of the chest, near the inferior angle of the scapula, was a round opening, measuring four or five centimetres (=1.574 to 1.968 Eng. inches) in diameter. Around this opening the skin was burnt, dry, and parchment-like. One of the bullets had pierced the right lung and bruised the heart, and passed out below the left breast: the other charge had fractured the vertebrae and divided the spinal cord. The different course of this charge might be explained by the revolution of the barrels. However that might be, it is clear that two successive shots fired from a fowling-piece at a short distance from the body might produce a single, perfectly round orifice. The burning and desiccation of the skin might have influenced the form of the loss of substance.

The woman's chemise presented *three holes*, of which two were confounded in part of their circumference, while the third was quite distinct and at a short distance from the others. The flannel under-garment presents only two apertures. Thus, while the projectiles had caused only one aperture in the part of the body which they had entered, the garments having been applied close to the body, had retained the traces of two projectiles separated by an interval, the folding and shifting of the garment serving to account for the difference.

M. Rigal observed that it was a point of great interest to determine how far a gun-shot wound is modified by the application of the muzzle of the gun to a resisting point —e.g. the thoracic parietes. Considerable difference of opinion exists on this subject, some persons considering that the gun must necessarily be burst. Experiments to determine the question are attended with too great danger to be made. M. Rigal considered himself, therefore, fortunate to be able to adduce an actual case in the absence of experiment.

2. M. X—, aged thirty-eight years, of a cultivated mind, finding himself in the second stage of phthisis, determined to destroy himself, and for that purpose he placed the muzzle of a pocket-pistol close to his chest, holding it in his left hand over the region of the heart: the ball, however, made no wound, but passed off from the body into the apartment. A second similar attempt was attended with the like results. X— reloaded his pistols, and holding the muzzle of one to his forehead, discharged it, and was astonished to find that he had not effected his intentions. He sought the reason of what he called his misfortunes, and found, as he supposed, that his failure was owing to want of air: he therefore made a crucial incision on his forehead with a penknife—attended, however, with the same ill success. "His precaution had been useless"—"the incision had not been large enough," he said. Accordingly he made fresh and longer incisions! but five successive attempts were not more successful. This unfortunate man then opened the veins of his arms by means of his penknife, and retired to his bed. In the morning he was found bathed in his own blood, but still living. M. Rigal was summoned to him; he was restored from his present condition, but fell a victim to the progress of phthisical disease. He had himself confided the preceding facts to M. Rigal, who found the five bullets in the apartment flattened by having come in contact with hard bodies. The forms of the eschars and wounds on his body bore witness to the truth of his statements. From a consideration of this case M. Rigal draws the following conclusions:—

1. When fire-arms are discharged, under the circumstances above indicated, the air confined in the tube produces a movement of recoil. 2. This recoil will be in proportion to the charge, and to the force and degree of exactness with which the muzzle is placed in contact with the surface of the body. 3. It does not necessarily follow that a wound shall be produced, and if it be, it will not always be found exactly at the spot on which the fire-arm was applied. 4. The gun will not explode if well made, and have an ordinary charge.

M. Rigal referred to cases in which balls have traversed the circuit of the cranium, beneath the integuments, without entering its cavity; also, to those cases in which the fire-arm being held close under the chin, the face is horribly mutilated.

M. Rigal terminated his observations by submitting the following questions:—

If a person be attacked in the dark and have fire-arms discharged in his face, is it possible for him to recognise his assailant by the light of the flash?

This medico-legal problem was suggested by the following case:—A man living in a forest was standing at his door, smoking his pipe, one dark autumn evening; he distinctly heard the click of a flint lock which flashed in the pan, without igniting the charge; he turned towards the direction of the noise: immediately the forest resounded with the sound of a gun, and he exclaimed, "Oh, Gabriel, you have killed me!" He subsequently affirmed that by the flash of light he was enabled to recognise the well-known dress and features of a neighbour. The possibility of the recognition was by the authorities referred to the opinion of M. Rigal, and Dr. Crouzet. The experiments they performed in reference to this point lead them to the conclusion that such recognition is impossible.

MM. LARREY, FORGET, and DREUZE, made some few remarks on M. Rigal's cases and observations, in the course of which M. Forget related a case of attempted suicide by gunshot wound.

ACADEMY OF SCIENCES, PARIS.

Nov. 25, 1850.

Action of the Pancreatic Secretion on Oils.

M. LASSAIGNE forwarded a note, entitled Contributions to the History of the Pancreatic Secretion, in which he adds several facts to those already made known by MM. Bernard and Barreswill,—viz., that the pancreatic juice possesses the remarkable property of forming an emulsion with animal and vegetable fats, and of transforming them into fatty acids and glycerine, at a temperature of $+ 38^{\circ}$ Cent. ($= 83^{\circ}$ Fah.)

M. BERNARD had transmitted to M. Lassaigue a small quantity of pancreatic juice which he had obtained from a dog of moderate size.

The following propositions include the result of M. Lassaigue's experiment on this fluid:—

The action of the pancreatic juice on oils is exercised when at a temperature of $+ 12^{\circ}$ to $+ 15^{\circ}$ Cent. ($= 54^{\circ}$ to 57° Fah.) in the course of a few hours. The acidification takes place equally in various gases,—e. g., oxygen, hydrogen, nitrogen, and carbonic oxide. This change appears to be dependent on what Berzelius named the catalytic force. The pancreatic juice will preserve its alkalinity for several days. Sugar and gum dissolved in this fluid remain unchanged.

New Researches on Disinfecting Agents, and on Substances used for the Preservation of Animal Matters.

1851 transmitted the summary

of his researches on these subjects, from which it appears that volatile substances which are composed of carbon and hydrogen, either simply, or essentially, constitute a class of agents having the special property of arresting the action of moist oxygen. In this category are placed sulphuric ether, chloroform, naphtha, oil of *houille*, crude or distilled oil of *schiste*, acetic ether, benzine, naphthaline, oil of spirit of wood, essence of caoutchouc, essential oil of potatoes, essential oil of bitter almonds, and hydriodic ether.

Animal matters placed in vessels of either of these do not undergo putrefaction. Portions of meat exposed in closed vessels to a mixture of air with the vapour of either, retain their freshness for several months.

M. Robin, following the analogy of the chemical composition of those substances, has discovered a second class of compounds possessing the same properties; these are the binary combinations of carbon, and the metalloids, except hydrogen,—*e. g.*, sulphuret of carbon, protochloride of carbon, nitrate of carbon, &c.

Chloroform and protochloride of carbon preserve also the colour of animal substance, besides preventing their putrefaction, but in this respect are not equal to hydrocyanic acid; while with reference to price and the rapidity of its action, the oil of houlle surpasses all other of these agents.

M. Robin considered that the latter might be employed for embalming, for the preservation of anatomical specimens, for the tanning of leather, for the destruction of insects that attack specimens of natural history, and for the preservation of grain, &c. &c.

Hospital and Infirmary Reports.

GUY'S HOSPITAL.

Extensive Lacerated Wound of the Fore-arm from the bursting of a Cannon.

WILLIAM GRANT, *et. 34*, was admitted into No. 15, Cornelius Ward, August 11th, 1849. He is a man of middle stature, has lived and drank rather freely: states that half an hour previous to admission he was engaged firing a cannon to celebrate a festival: had succeeded in doing so with safety several times, until at last he forced a plug into the mouth of the piece, and then fired it, retiring as usual to a distance of five yards, where he stood with the rod in his hand: the result was an instantaneous bursting of the cannon into twenty

or thirty pieces, one portion of which, weighing about 1½ lb., struck him with great violence on the outer side of his left forearm, causing him to stagger back, but without knocking him down. The coat-sleeve and shirt were torn in shreds, and blood soon began to flow freely: it was bound up tightly, he became very faint, and was brought to the hospital in a cart. On examination there was a large lacerated wound on the dorsal aspect of his left forearm, occupying its middle third, and lower part of the upper third. The extensor muscles were very much lacerated and hanging in loose shreds, with a compound comminuted fracture of the ulna, allowing the finger to be passed freely through the wound, between the radius and ulna, through the interosseous ligament down upon the flexor muscles. Mr. Hilton removed three loose pieces of bone, as also the fractured ends, by means of the chain-saw. The radius was dislocated forwards and upwards, its head resting on the humerus above the external condyle: there was no evidence of fracture of this bone, and Mr. Hilton succeeded in reducing it without much difficulty: the loose portions of ragged muscle were removed by the scalpel, and there was no important hæmorrhage.

The anterior part of the forearm having sustained but little apparent injury, the wrist-joint being sound, the chief nerves and vessels uninjured, the patient himself in the prime of life, and in good health, Mr. Hilton thought it would be proper to attempt to save the limb, notwithstanding that there seemed some probability of the elbow-joint being seriously injured, as evidenced by the necessary laceration of the coronary and interosseous ligaments allowing so extensive a displacement of the radius: accordingly, as the surrounding parts were swollen and contused, the arm was raised on a pillow, a piece of moist lint was applied with slight pressure, by means of compress, to prevent hæmorrhage, and he was ordered two grains of opium immediately.

12th.—He took the opium, but nevertheless had passed a very restless night; at present feels more comfortable; is free from actual pain, but complains of numbness along the back of his arm and hand, extending as far as the first phalanges, including the distribution of part of the radial nerve. The extremities of the fingers, however, retained their sensibility. His tongue is moist, but slightly furred; pulse 82, and rather full; skin moist, and of natural temperature. As he had been accustomed to drink freely, and gave evidence of considerable nervous agitation and unsteadiness in his tongue and arms, he

was ordered two pints of porter. In the evening Mr. Hilton removed the temporary compress; the arm was rather more swollen than yesterday, but the wound as yet looked favourably. Warm water dressing; opium gr. ij. nocte maneque.

13th.—He has passed a much better night, having slept comfortably. There is now slight evidence of constitutional disturbance. Pulse increased to 108, and rather full; tongue furred, but moist, and feels thirsty: the swelling around the wound is still considerable, especially towards the elbow: the lacerated ends of the muscles show a slight tendency to slough: temperature of the limb natural; bowels are confined. Ordered Hydrargyri Chloridi, gr. ij.; Opii, gr. j. statim; to continue the porter, and repeat the opium at bedtime.

14th.—He has passed a good night, and is quite free from pain or uneasiness, the numbness and swelling being both less, and the discharge has assumed a more fetid character: the chief part of the surface of the wound is gangrenous, but a few healthy-looking granulations appear in different parts: pulse 120, and full; bowels moved twice.—Pt.

15th.—He is progressing favourably: the marginal skin, as also the surface of the wound in most parts, having separated, leaving a healthy granulating surface: the head of the radius is now rather displaced, and cannot easily be retained in its proper position: his pulse is reduced to 85, and less full.—P.

16th.—He is doing well: the swelling around is subsiding, and he can extend the fingers slightly. As the granulations looked rather weak, Mr. Hilton thought he would bear more support, and ordered the porter to be increased to three pints per diem, to have middle diet, and continue the opium.

17th.—He has passed a more restless night: there is still a little sloughing from the deepest part of the wound; the discharge has increased in quantity, and is of a dirty-looking, purulent character. Pulse rose to 120; tongue coated with white fur, but moist.—Pt.

18th.—The remaining slough has separated, leaving a healthy granulating surface over every part, whilst the discharge is less offensive and more purulent; the swelling around has now nearly subsided. Tongue quite clean and moist; pulse 90, and natural.—P.

20th.—He is gradually improving, and appears very cheerful; sleeps well; appetite returning; he chiefly complains of lumbar pains, seemingly of a rheumatic character. — Ordered Opii, gr. j. omni nocte.

22d.—There is now a good, healthy-

looking, purulent discharge from the wound, which is gradually filling up; and he is progressing altogether as favourably as can be wished for, only suffering from lumbar pains, which are, on the whole, rather less. Being tired of keeping his arm on a pillow, a splint was applied underneath, and he was allowed to get up.

31st.—The wound is nearly filled up, and cicatrization has commenced in some parts.—P.

Sept. 5th.—His general health seems much improved, and the wound is gradually lessening in size: the head of the radius is still rather displaced, but he can bend the elbow to a certain extent without much difficulty; he soon after left the hospital, and continued to attend as out-patient.

He presented himself again, after an interval of three months, and stated that three small pieces of bone came away before the wound closed over. He can now bend the elbow tolerably well. In pronation of the hand, the fractured end of the upper portion of the ulna projects, but is immediately restored in supination; the radius, although displaced, allows of some rotation; he can flex the finger sufficiently to retain anything of moderate size in his hand; his general health is much improved, and he has gained flesh.

August 8th.—Nearly twelve months since the accident. He has now a very moveable and useful arm; can place his hand on his head, or behind his back—in fact, almost wherever he pleases: he can bend the elbow to a right angle, and can extend it nearly as well as on the opposite side: the symmetry of the limb is in some measure lost. After passing the hand over the olecranon, it dips into a hollow, where the ulna is deficient from two to three inches. Owing to well-adjusted pressure from a pad, which he has constantly worn up to the present time, the fractured ends of the bone, at first very prominent, are now lying in a much more favourable position close to the radius; the lower portion of the ulna is still moveable, and not ankylosed to the radius; the cicatrix is much smaller than could have been expected, and sensation is quite perfect over every part of the arm; he can extend the fingers perfectly, and can flex them sufficiently to grasp the hand with moderate force; the head of the radius is now lying in front of the external condyle, where it can be distinctly felt, and is moveable to some extent: he still wears the pad, which consists of two parts, one encircling the arm above the elbow, connected by a leather strap to the other and most important, which was fastened round the forearm, below the elbow, where it is sufficiently padded so as to make constant pressure on

the projecting fractured ends of the ulna, whilst a space was left between the two portions of the apparatus, so as not to interfere with the movements of the elbow-joint. This had the desired effect of keeping the ends of the bone in a more favourable position, whilst at the same time the patient found it a great support to the arm.

Amongst the many severe cases which present themselves to the notice of the practical surgeon, there are none which require deeper consideration than those of injuries to the extremities, involving, as they frequently do, a question so difficult to determine, whether a limb should be amputated, or an attempt be made to save it; and indeed, when one considers how great a variety of accidents occur from a similar cause, and the somewhat rare occurrence of two cases presenting precisely similar features, it can readily be understood how difficult it would be to draw a line which would enable us to say pointedly, this limb should be amputated, or that should be saved; for, on seeing the state of the patient's arm on admission, and having noted the violent laceration of soft parts, the extent of the fractured bone, the contusion of the surrounding tissues, necessarily the result of a force sufficient to cause such an injury, and also having ascertained an extension of the mischief towards the elbow joint, as evidenced by the laceration of the cordary and interosseous ligaments, allowing such marked displacement of the radius, one's first impression certainly would be that amputation was the only resource, as a means of safety to the patient's life; and such, indeed, was the opinion of many who saw this case at the time: but, on after consideration, I think that amputation, under existing circumstances, would not have been justifiable; for, as Mr. Hilton observed at the time, the most important part had sustained but little injury; the flexor part of the forearm was comparatively uninjured; the chief nerves and vessels were safe, and the wrist-joint was sound; the patient's age was in his favour; his general health had been previously good, and his constitutional powers apparently unimpaired. The justice of these remarks was amply borne out by the progress and termination of the case, which was in reality more favourable than could have been anticipated, as no bad symptoms occurred from beginning to end.

In reference to treatment, it will be observed that stimulants and support were indicated from the commencement. The patient had been accustomed to drink freely, and from the nervous agitation which existed on the day after the accident,

it did not seem improbable that delirium tremens would have been the consequence, if the accustomed stimulus was denied. Such an occurrence was also no doubt prevented by the large doses of opium exhibited. One question yet remains to be considered. Was the limb a useful one after all? This may be best answered by his condition at the present time.

Nov. 27th, 1850.—He has now resumed his work as a cooper for one month, having left off the pad (which he has worn for the last fifteen months) about a fortnight since. The injured arm when compared with the sound one is one inch shorter, and its movements are considerably improved; whilst the large gap left between the ends of the bone, where the muscle was destroyed, is now very nearly filled up by soft tissues. The head of the radius can easily be felt in front of the external condyle, where it is moveable to some extent, and allows flexion of the joint to a right angle, whilst extension is nearly perfect. The flexor muscles have recovered from their former stiffness. He can grasp with sufficient force to hold the handle of his knife in his left hand when at work, and can raise a moderate weight without much difficulty. The fractured ends of the ulna are now in close contact with the radius; though not ankylosed to it; and the only inconvenience he complains of is a slight aching pain over the end of the bone after working some hours, or when the arm is suffered to hang down.

Gun-shot Wound in the Neck.

William Everitt, *et.* 18, was admitted into No. 2, Cornelius Ward, under Mr. Hilton's care, July 10th, 1850. He was in a faint and somewhat exhausted condition, though quite sensible, having been shot in the neck some few hours previously.

It appears that, in company with another man, he had broken into a gentleman's house at Camberwell, had removed some property, and was standing in a stooping posture outside a window, his head and neck being parallel with, though somewhat above the ledge, when the gentleman fired at him from a short distance with a pistol. He immediately fell, but soon recovered himself, and with his companion scaled the garden wall, and made good his escape into the Kent Road, leaving about four ounces of blood on the spot where he was shot. He soon after met with a policeman, said he had been shot by a gamekeeper, and was immediately brought to the hospital.

On examination there was a circular wound on the left side of the neck, situated below the mastoid process, about one inch below and behind the lobe of the ear near to the posterior border of the sterno-mas-

toid muscle, a portion of which the bullet in all probability had perforated. On introducing a probe, it passed in a direction downwards and forwards towards the lower part of the larynx (where a mark was made with nitrate of silver), but no evidence of the bullet, or indeed any foreign substance, could be detected. His neck and chest



were carefully examined with a similar result: the external opening was quite circular, about a quarter of an inch in diameter, its edges being inverted, minutely lacerated, and rather livid, with slight swelling around. His chief complaint was of pain in his left arm, which he attributed to having fallen upon it, but no evidence of external injury could be detected: he also complains of an uncomfortable sensation, almost amounting to pain, on swallowing: respiration, however, is perfectly easy and natural: pulse 84. Ordered Pil. Colocynth. c. Cal. gr. x. statim; Olei Ricini, ʒij. post horas quatuor.

2 P.M.—There has been no hæmorrhage since admission: he now complains of numbness and tingling along the outer and back part of his arm, which suggested that the bullet might probably be lodged somewhere near the brachial plexus: bowels have been opened. As he was the subject of scabies, ordered Ung. Sulphuris, infricand. A bullet from the same mould as that with which he was shot measured five-sixteenths an inch in diameter.

11th.—He has passed a good night: there is rather more swelling around the wound, with tenderness on pressure: he still complains of pain in his arm, and lies most comfortably on the right side. Pulse 116, and rather full. As yet there is no indication to the position of the bullet. Hyd. c. Creta et Pulv. Doveri, ss. gr. v. ter die; Julep. Ammoniac Acetatis, ʒij. ter die. In the evening he seemed more feverish, and complained of numbness over and above the clavicle.

12th.—He is less feverish to-day and has passed a better night, having less pain in the arm. There is now a slight discharge from the wound, whilst the inverted edges are less marked; but the surrounding swelling is if anything increased: he lies chiefly on the affected side, and feels easiest in that position. Pulse decreased to 100, and not so full. Linseed-meal poultice, with Ext. Conium.

13th.—The wound is at present discharging freely, and he is much less feverish, but still complains of pain down the arm and numbness over the acromion: tongue furred, but moist; pulse decreased, to 88; bowels confined. Pil. Col. c. Cal. gr. x. statim: Ol. Ricini, ʒij. post horas quatuor.

17th.—He seems daily improving: the numbness in the shoulder is much diminished, whilst the discharge from the wound is of a more healthy character: tongue cleaner. Sodæ Sesquicarb. gr. x.; Ex. Decoct. Cinchona, ʒj., t. d.

21st.—The numbness has quite left the shoulder, whilst the pain in the arm is very slight. He now complains of pain in his neck along the margin of the trapezius; nothing abnormal, however, can be felt there, whilst the external wound is nearly healed. Bowels confined.—Pil. Col. c. Cal. gr. xv. statim.

25th.—He seems much improved. Appetite returned. Quite free from pain.

30th.—The wound is healed. He feels as well as ever. To be presented.

Perhaps an explanation of the reason why so little constitutional disturbance existed in this case may in some measure be afforded by remembering that the patient's neck was uncovered over that part where the bullet entered, and thus prevented the possibility of any portion of clothing or other extraneous substance being carried into the wound. The direction of a gun-shot wound would depend in some measure on the posture of a person at the time of the shot, also on the speed of the ball, as well as the thickness and resistance of the part it struck. In the present instance the man was in a stooping posture, his neck being parallel with the ledge of a window, and, from the direction in which the shot was fired, one would

have expected the ball had lodged somewhere in the back part of the neck. Such, however, did not appear to be the case, for no evidence of it could be discovered externally; whilst the direction in which the probe passed was quite the reverse; and, rather remarkable, it appeared to pass at first through the posterior portion of the sterno-mastoid muscle, thence under it, downwards and forwards, towards the lower part of the larynx. At that spot where the extremity of the probe could be felt under the skin Mr. Hilton made a mark with the nitrate of silver, so that, in case of suppuration taking place, and not discharging through the wound, it might afford some indication as to the best situation in which an opening should be made. Knowing how a ball may be turned by any slight resistance not directly opposed to it, it has been suggested by several authors that, if the resistance should be greater than the momentum, and offered by an elastic body, the recoil of the ball may be so exact as to return through the same opening by which it entered; and, indeed, a similar opinion was hazarded by a gentleman who saw this case at the time. The conjecture, however, does not seem at all probable, as the character of the wound was decidedly against it, its edges being inverted and minutely lacerated; whereas it seems reasonable to suppose that the small opening made by the swift ball on entering would not have allowed the same to return (its impetus of course being considerably diminished) without everting the edges of the wound, so that the only conclusion one can arrive at is, that the ball still remains buried in the tissues of the body. The period of time during which a foreign body may remain without giving rise to any notable inconvenience is also worthy of notice. Mr. Hilton mentioned an instance which had come under his observation where a bullet had been lodged in the lumbar region for twenty-three years.

involved. It was evident that an obstruction existed somewhere to the return of the venous current from the lower half of the body, involving the portal circulation, but apparently involving much more.

Her own statement pointed to the anasarca of the lower limbs as anterior to the ascites, but in this she was mistaken; and the very considerable amount and long-continuance of the anasarca were unusual: indeed, the chief circumstances indicating the liver as the cause, were pain,—recent pain there,—and a yellow colour of the eyes, and both were calculated to mislead: some, unlike old cirrhosis, were like congestion from obstruction farther on in the cava itself. And it only became evident that ascites was probably the primary, as it certainly was the more important of the dropsical effusions, from the maintenance of a low horizontal position in bed.

The kidneys were never successfully brought into action; and this, I think, is constantly the case in similar instances, even when quite free from disease, simply in consequence of the pressure of the fluid. In corroboration of this view, I may cite the similar instance of ovarian dropsy, where I have observed albuminuria, produced simply by pressure, disappear when that pressure was relieved.

Of the milder purgatives she had plenty, and the want of success attending their employment did not encourage the use of the more violent cathartics in her weak and depressed state.

The question of giving vent to this fluid by tapping was once or twice referred to; but having been employed about the same time in another case, which proved fatal in a very few hours by peritonitis, Dr. Nairne congratulated himself that this course had not been followed.

O. R., *etat.* 15, a girl of quiet habits, was brought up from the country in an advanced stage of dropsy affecting the abdomen and lower extremities, but not extending at all to the upper part of the body; and admitted under Dr. Nairne, on 29th of August, 1850.

The history of the case was obscure: she had had a severe illness of some sort, supposed to have been "a fever," about eighteen or twenty months previously, and had not been thoroughly well since, but had been sufficiently strong to be engaged in farm-work until after the commencement of the present attack, which dated back about twelve months, having begun, so far as she knew, with swelling of the ankles, especially of an evening. The abdomen had begun to swell soon after; and since that had been observed, she had never been entirely free from oedema. She had been under medical treatment during the whole

ST. GEORGE'S HOSPITAL.

REPORTED BY Dr. BARCLAY,
Medical Registrar.

Cirrhosis of Liver and Ascites in a Girl of 15 years of age.

THIS case seems worthy of being placed on record, as well from the rarity of cirrhosis of the liver in so young a female, as from the obscurity in which its history was

period; she had not suffered from pain in the back, nor had had pain in the right side until lately; but a blister had been applied to the right hypochondriac region nearly a month ago. The catamenia had occurred last in June, had been seen twice at irregular intervals last year, and not since.

The abdomen is very tense, communicating to the hand a very distinct sense of fluctuation, and with no hardness. The bowels can be made to float in any direction by turning her on either side, the resonance on percussion always being found at the most elevated situation, indicating that the fluid is not contained in any cyst. The legs are very anasarctous. Pulse quick; tongue coated, but moist. Bowels confined; urine said to be very scanty.

She was ordered to be freely purged with jalap, colocynth, senna, &c., for two or three days.

Sept. 1st.—On testing the urine it is found perfectly free from albumen, but scanty, and of a very deep brown colour, not containing any excess of lithates, nor giving any pink sediment. The heart is also free from any abnormal sound. Ordered to have a draught containing a scruple of acetate of potash every six hours.

5th.—The dropsy has not at all decreased, and she complains of great pain at the top of the right thigh, where there is a small superficial ulcer, with a good deal of redness round, which she accounts for by stating that the skin had burst in that situation some short time previously. This also probably explains a remarkable difference which is now to be observed between the two legs in point of size, the right being larger and harder than the left; for she states positively that, before this occurrence, both legs were of exactly the same size.

The thigh was directed to be propped up with pillows, by which, through gravitation, the distension would be relieved; and local applications were directed by the surgeon to the ulcer. It soon ceased to trouble her.

7th.—A slight attack of epistaxis to-day, accompanied by a sudden discharge of blood from the mouth, which does not seem to have been brought up by coughing: neither was it attended with retching, but has probably, from its dark colour, come from the stomach. The urine to-day, too, has acquired a slight degree of opacity, and does not become clear on boiling with acid; under the microscope it exhibits the presence of numerous blood-globules. To have castor oil, and go on with her mixture.

11th.—Ordered to have a draught every six hours, containing one ounce of decoction of bark, half an ounce of spirit of

mindererus, and a scruple of acetate of potash every six hours.

13th.—To-day she is attacked with severe dyspnoea and pain in the chest, which she refers principally to the region of the heart. Its action is quick, excited, and somewhat oppressed, but no abnormal sound can be made out. Twelve leeches were directed to be applied, and distilled water to be substituted for decoction of bark in her mixture, with the addition of ten minims of tincture of digitalis, and to have house medicine to-morrow.

17th.—She has had no return of the severe dyspnoea, but had a slight attack one evening lately. She is more swelled, both in stomach and legs; the yellow colour has disappeared from the eyes. Ordered to have three grains of blue pill, and one of powdered squill, three times a day, while she leaves off the mixture.

18th.—To-day all the distress has returned. There is pain referred to the region of the heart, with excited and tumultuary action. The swelling of the abdomen and legs is such that she cannot sit up in bed, although the breathing is much interfered with by the upward pressure of the diaphragm. Six leeches were again directed to be applied to the præcordium, and she was ordered to have three grains of calomel and a quarter of a grain of opium three times a day.

27th.—Nothing has occurred worthy of note. To-day the gums have become affected by the calomel. Her general symptoms unrelieved. Her appetite fails, and she is afraid to eat, from the extreme state of distension, which causes the smallest quantity of food to produce great dyspnoea. Still she lies flat on her back, or only inclines to one side or other. Ordered to leave off the calomel, and have a senna draught.

30th.—She has complained, in addition, of pain in the loins. The urine has the same general characters as previously—viz. a certain degree of opacity, which is not removed by boiling with acid, but is not more distinctly albuminous. She is to resume the pill twice a day.

Oct. 7th.—The calomel was omitted after a few days' continuance, and a mixture has been ordered with nitre, nitric ether, and a few drops of laudanum; but she is now beginning manifestly to sink. The face is pinched and anxious, the pulse scarcely perceptible, and her sufferings much aggravated; she can scarcely swallow a little wine which has been ordered.

11th.—Died last evening. She has struggled hard for life. Her end was hourly expected on the evening of the 9th, but she did not cease to breathe till the afternoon of yesterday.

Post-mortem examination 21 hours after death.

General appearance.—Body well-made. Abdomen much distended. Lower extremities very oedematous.

Thorax.—Each pleural cavity contained about half a pint of clear serum. The lungs were much compressed, and slightly congested posteriorly, and there was a good deal of ecchymosis beneath the pleura costalis on either side. The heart healthy; ventricles contracted.

Abdomen.—The cavity of the peritoneum was greatly distended, and filled with clear serum. The liver was small, shrunken, pale-coloured, rounded at the edges, and slightly uneven on the surface, where the investing serous membrane was thickened in some places, and opaque. Its cut surfaces presented some portions of pale but healthy structure, especially at the upper part of the organ, but in the neighbourhood of the fissure, where the great vessels enter the liver, it presented a well-marked specimen of cirrhosis. The gall-bladder was small, and contained a little pale biliary fluid; the coats of the gall-bladder were thickened, and more opaque than usual. Gall-duets healthy. The spleen was small and firm. The kidneys congested, but otherwise healthy.

Medical Intelligence.**UNIVERSITY OF LONDON.**

M.B. SECOND EXAMINATION—1850.

PASS EXAMINATION.**First Division.**

Bristowe, J. Syer, St. Thomas's Hospital.
 Growse, Robert, Guy's Hospital.
 Hewitt, W. M. G., University College.
 Lewis, R. B., Leeds School of Medicine.
 Manley, Edmund, Royal Manchester School of Medicine.
 Salter, Henry Hyde, King's College.
 Shearman, C. J., University College.
 Thornton, William Henry, Royal Manchester School of Medicine.

Second Division.

Filliter, William, University College.
 Heale, J. N., St. Thomas's Hospital.
 Jones, James, Royal College of Surgeons of Ireland.
 Robertson, James, St. Thomas's Hospital.

EXAMINATION FOR HONOURS.**Physiology and Comparative Anatomy.**

Bristowe, J. Syer	} Equal.	} St. Thomas's Hospital.
(gold medal)		
Growse, Robert		
(gold medal)		Guy's Hospital.
Hewitt, W. M. G., University College.		
Shearman, C. J., University College.		

Surgery.

Bristowe, John Syer (scholarship and gold medal), St. Thomas's Hospital.
 Lewis, Robert Benson (gold medal), Leeds School of Medicine.
 Growse, Robert, Guy's Hospital.
 Shearman, C. J., University College.
 Hewitt, W. M. G., University College.

Medicine.

Growse, Robert (scholarship and gold medal), Guy's Hospital.
 Shearman, Charles James (gold medal), University College.
 Bristowe, J. Syer, St. Thomas's Hospital.
 Lewis, R. B., Leeds School of Medicine.
 Hewitt, W. M. G., University College.

Midwifery.

Hewitt, W. M. G., University College.

M.D. EXAMINATION—1850.**First Division.**

Baines, Matthew, King's College.
 Barron, Edward Enfield, Guy's Hospital.
 Birkett, George, Charing-Cross Hospital.
 Carlill, J. Burford, University College.
 Eadie, Peter, King's College.
 Edwards, W. T., University College.
 Elam, Charles, Leeds School of Medicine.
 Monckton, Stephen, King's College.
 Ransom, W. H., University College.
 Sankey, W. H. O., St. Barthol. Hospital.
 Wilks, Samuel, Guy's Hospital.*

Second Division.

Ayre, William, London Hospital.
 Heale, James Newton, St. Thomas's Hosp.

EXAMINATION FOR THE FELLOWSHIP OF THE ROYAL COLLEGE OF SURGEONS.

THE examination for the Fellowship took place on the 3rd and 5th inst. There were only four candidates for the honour.

In the first examination on *Anatomy* and *Physiology* the following questions were submitted:—

1. What glands pour their secretions into the mouth? Describe the situation and connections of the parotid.
2. Describe the elbow-joint, and the mechanism by which rotation and supination of the hand is (are?) performed.
3. Describe the position, course, and connection of the intestinum rectum in the male; and mention the peculiarities of its structure.
4. Describe the course of the common iliac artery, its relative position to the parts around on the right and left side, the point of its bifurcation, and course and relative position of the external iliac.

* To Wilks was awarded a gold medal for his commentary on a case in medicine.

5. Describe the distribution of the arteries supplying the abdominal portion of the organs of digestion.

6. Enumerate the parts occupying the upper opening of the thorax, and describe their relative situation.

In the second examination, which was on *Pathology and Surgery*, there were the following questions:—

1. Describe the healing process by adhesion, by granulation.

2. Describe the most frequent causes of suspended animation, their modes of action, and how death is to be averted.

3. Describe the evidences of the presence of a foreign body in the air-passages, the operation for its removal, and the difficulties and dangers which may attend it.

4. Describe the morbid changes which attend the formation and progress of a popliteal aneurism, the operation for its cure by ligatures, and by compression of the femoral artery. How do these two modes of treatment act in the cure of the disease, and what are their respective merits?

5. Describe the effects of rickets on the bones generally and specially: give the treatment of the disease.

6. Enumerate the diseases of the eye in which morbid action originates in the conjunctiva.

ROYAL COLLEGE OF SURGEONS.

GENTLEMEN admitted members on the 29th ult.:—Messrs. A. Hampeon—W. D. Lacy—A. M. Blomfield—G. A. Galland—E. Batly—J. Ringland—H. Lawrence—E. H. Paget.

GEOLOGICAL SOCIETY.

THE following papers will be read at the next evening meeting of the Geological Society, Dec. 18:—

1. On the Epiolitic Rocks of the Venetian Alps. By Prof. A. T. Catullo.

2. On the Mineral Springs of Vichy. By Sir R. I. Murchison, F.G.S., &c.

3. Report on a New Combustible Substance (Pungerite) discovered in Russia.

OBITUARY.

AT Ordnance Place, Chatham, on the 1st inst., Patrick Brodie, Esq., late Assistant-Surgeon 13th Light Infantry.

On the 10th inst., at 63, Hoxton Old Town, George Thos. Heath, M.D., æt. 67.

ADULTERATION OF MILK.

THE *Journal de Chimie Médicale*, Sept. 1850, states that two persons in Paris, carrying on the business of supplying milk to the public, have been sentenced to eight days' imprisonment, and a fine of fifty francs, for adulterating milk with dextrine and water.

Selections from Journals.

DISADVANTAGES OF CHLORIC ETHER AS AN ANÆSTHETIC.

THE chloric ether consists of one part of pure chloroform, with nine parts of alcohol. This, "it cannot be denied," says Dr. Hayward, "derives its power of producing insensibility from the chloroform it contains; and it is difficult to understand how the addition of alcohol can deprive it of its dangerous properties when it is well known that the mixture of this substance with sulphuric ether renders it in great measure unfit for inhalation."

"The advantages which it is said to possess are, that its odour is less pungent and disagreeable than that of sulphuric ether, and that it can be inhaled with little or no inconvenience. At the same time, it must be admitted that it is necessary to use as much chloric as sulphuric ether, and to continue the inhalation for as long a time to produce the desired effect.

"The disadvantages are, that when it comes in contact with the unprotected skin, it acts upon it in the same manner as chloroform. From this cause a patient suffered several months at the hospital, and I believe much more severely than if he had undergone the operation without the ether. I am confident, too, that it is more apt to produce vomiting, and a greater disturbance of the brain and nervous system, causing headache, restlessness and vigilance, which not unfrequently continue for many hours after its exhibition. Perhaps these last symptoms may be owing to the great amount of alcohol it contains.

"I cannot, I confess, divest myself of the belief that chloric ether is an unsafe anæsthetic agent, when I consider that it is simply chloroform diluted with alcohol. It is true, that as far as we know, no fatal effects have hitherto followed its inhalation; but it is also true, that it has yet been used to a very limited extent, and in all the cases in which it has been exhibited that have come to my knowledge, it has been managed with great caution and judgment. But I fear that if it be used with the same freedom that sulphuric is, we shall soon have to record some very different results. We cannot feel confident that it will always be confided to skilful hands only, nor by any means certain that death, when not looked for, may not follow its exhibition."—*The American Journal of the Medical Sciences*, July 1850.

CHEAP MEDICAL TEACHING IN THE UNITED STATES—FREE MEDICAL SCHOOLS.

DR. N. S. DAVIS, Professor of Physiology and Pathology in Rush Medical College, Chicago, has, in an address on "Free Medical Schools," insisted very much on "the inordinate expenses" attending medical education on the present system, and proposes to do away with fees to lecturers, and make medical instruction literally "cheap as dirt." The editor of the *Western Lancet* has so ably dissected this scheme, and exposed the miserable rottenness and corruption at its core, that, did our space permit, we would reprint the whole article. We freely endorse the sentiments of the writer on this subject, and expect the next proposition of Professor Davis will be, in addition to a gratis education, to give the students *oyster suppers on Wednesdays and Saturdays, and turkey and venison dinners on Tuesdays and Fridays*; for surely it will never do to let the poor fellows starve on coffee and "chicken-fixins." "Mens sana in corpore sano" used to be the motto in our school-boy days: to attend six lectures a day is mighty hard work, we think, and few minds will be able to labour that much profitably, unless the powers physical be sustained; and what's the use of all the learning in the world without health and strength, we should like to know? If any thing is to be given away, true benevolence would first see that the objects of her bounty lacked nothing in the way of "creature comforts," and these abundantly supplied; the intellectual wants might next be cared for. The Chicago system reverses this order, and offers the hungry, shivering student, a free lecture on the physiology of digestion, saying, "cat and be filled," or a ditto upon animal heat, as a substitute for woollen stockings and over-coat. The plan is new, but immoral; hence 'twill so necessarily be frowned down in this age of regenerate piety and almost universal charity, so universal that even medical schools are becoming tinctured with it, that we have small doubts as to the result.—Professor Smith, *i New York Medical Gazette*.

BOOK & PERIODICALS RECEIVED FOR REVIEW.

(The List will be given in our next No.)

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer 30.14
 " " Thermometer 42.6
 Self-registering do. Max. 54° Min. 29°
 " From 12 observations daily. " Sun.

RAIN, in inches, .07.—Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was 3° above the mean of the month.

BIRTHS & DEATHS IN THE METROPOLIS

During the Week ending Saturday, Dec. 7.

BIRTHS.		DEATHS.	
Males....	799	Males....	494
Females..	769	Females..	510
	1568		1004

CAUSES OF DEATH.

ALL CAUSES	1004
SPECIFIED CAUSES	998
1. Zymotic (or Epidemic, Endemic, Contagious) Diseases....	229
Sporadic Diseases, viz.—	
1. Dropsy, Cancer, &c.	39
2. Brain, Spinal Marrow, Nerves, and Senesce	110
4. Heart and Bloodvessels	44
5. Lungs and organs of Respiration	208
6. Stomach, Liver, &c.	63
7. Diseases of the Kidneys, &c.	10
8. Childbirth, Diseases of Uterus, &c.	5
9. Rheumatism, Diseases of Bones, Joints, &c.	8
10. Skin	2
11. Premature Birth	35
12. Old Age	47
13. Sudden Deaths	6
14. Violence, Privation, Cold, &c.	28

The following is a selection of the numbers of Deaths from the most important special causes:

Small-pox	27	Convulsions	32
Measles	23	Bronchitis	83
Scarlatina	35	Pneumonia	66
Whooping-cough	50	Phthisis	118
Diarrhoea	21	Lungs	8
Cholera	0	Teething	9
Typhus	43	Stomach	7
Dropsy	16	Liver	14
Hydrocephalus	18	Childbirth	3
Apoplexy	22	Uterus	2
Paralysis	30		

REMARKS.—The total number of deaths was 24 below the average mortality of the 49th week of ten previous years.

NOTICES TO CORRESPONDENTS.

Dr. F. J. Brown's case of Diabetes Mellitus next week.

Dr. Tilt's paper, in continuation, has been received.

Dr. Griffith.—In the following number, if possible.

Dr. Thomas Williams, Swansea.—We are obliged by the communication, and shall have great pleasure in giving it early insertion.

We regret the necessity of being obliged to postpone for another week Mr. Brown's (London Hospital) Case of Reduction *en masse*.

Dr. F. Winslow's correspondence shall be published next week. It reached us too late for the current number.

Dr. Pollock's third paper has come to hand.

Mr. Margetson.—Mr. Tomes's lectures are contained in the 39th and 40th volumes of the Journal, 1846-7. Application should be made to the Publishers.

RECEIVED.—Dr. T. K. Chambers.—The Scotsman.

CORRIGENDUM.—In the case of the Countess of Goerlitz, at page 248, col. 1, line 59, after (833'11 cubic feet Eng.) read "of oxygen equivalent to."

Lectures.

COURSE OF LECTURES
ON
DISEASES OF THE HEART.

*Delivered at St. Vincent's Hospital during the
Session 1849-50.*

By O'BRYEN BELLINGHAM, M.D.

Fellow of, and Member of the Court of Examiners
of, the Royal College of Surgeons in Ireland, and
one of the Medical Officers of the Hospital.

LECTURE XV.

GENERAL SIGNS OF HEART DISEASE.

*Indirect or Secondary Symptoms of Cardiac
Disease (concluded)—Congestion of the
Liver—Congestion of the Intestines—
Epigastric Pulsation the Result of Gastro-
Intestinal Congestion—Epigastric Pulsa-
tion in Nervous, Hysterical, and anæmic
Subjects—Congestion of the Brain—
Influence of Hypertrophy of the Left
Ventricle in giving rise to Apoplexy or
Paralysis.*

Congestion of the Liver.

THE connection which exists between a congested state of the liver and morbid conditions of the heart was alluded to by Corvisart, and has been noticed more or less by succeeding writers. Indeed, next to the lungs, the liver is more liable than any other organ to become congested and enlarged, as the result of cardiac disease; and there are several good anatomical reasons why this should be so. 1st. Owing to the structure of the liver; 2d. To the large amount of blood which circulates through it; 3d. To the blood which reaches it by the vena portæ having already passed through capillary vessels, and having consequently lost whatever influence the contraction of the left ventricle had upon it; 4th. To this blood having to pass through two sets of venous capillaries before it reaches the branches of the vena cava hepaticæ; and 5th. To the returning blood of the liver having to arrive at the right side of the heart against gravity.

Thus the substance of the liver, in general terms, may be said to consist of a close network of capillary vessels and ducts, arranged in lobules, and enclosing in their meshes nucleated cells; the tissue of the organ is soft and yielding, and it readily becomes increased in size when its vessels are distended. Again, the whole of the venous blood from the abdominal viscera (with the exception of that from the kidneys, bladder, and uterus) must pass

through the liver before it can reach the right side of the heart; which large amount of blood circulates through the capillaries of the liver, in addition to that which it receives by its own nutritious artery. The circulation of this venous fluid through the liver must necessarily be slow, in consequence not only of the absence of the influence of the contraction of the left ventricle, but from the large size of the capillaries, and the blood having, as has already been observed, to pass through two sets of venous capillaries.

Now if any impediment exists to the passage of the blood through the right chambers of the heart, owing to disease seated in the lungs, or in the left side of the heart, or if the blood ascending by the inferior cava cannot freely enter the right auricle, the circulation in the vena cava hepaticæ becomes retarded, which reacts through the intermediate vessels upon its capillaries. The capillaries of the vena cava hepaticæ have their origin in the centre of each of the lobules, and the circulation through them being impeded, they, as well as the intermediate vessels, become distended. This constitutes the slightest form of congestion of the liver, and has been termed by Mr. Kiernan "the first stage of hepatic venous congestion." A section of the liver, under such circumstances, presents a mottled appearance, the centre of each lobule having a deeper colour than the rest.

When the impediment to the return of the blood to the right side of the heart increases, or when the former condition has lasted for some time, the capillaries of the vena portæ, which occupy the circumference of the lobules, next suffer. The blood in both sets of capillaries being retarded, they become now distended, constituting a still more advanced stage of congestion of the liver; or, what Mr. Kiernan calls "the second stage of hepatic venous congestion." Here "a section of the liver presents likewise a mottled appearance, but now the pale portion will be in spots."

Both sets of vessels may be congested without the function of the liver being impaired, or without any considerable impediment to the secretion or escape of the bile. In many cases, however, when the congestion has lasted long, and the impediment to the return of the blood to the right side of the heart is considerable, the biliary plexus, and the minute ducts which ultimately form the excretory duct, suffer from the pressure; the bile is prevented from freely escaping, it accumulates in the lobules, and biliary is superadded to sanguineous congestion. A section of a portion of the liver will then present the ap-

pearance with which we are familiar as *nutmeg liver*.

Congestion of the liver may therefore be limited to the ramifications of the *venae cavae hepaticae*, or it may extend to the capillaries of the *venae portae*; or the two forms may be combined, as we have seen, with biliary congestion. The first has been termed by some "a passive form of congestion;" the second, an active form; but, when congestion of the liver depends upon cardiac disease, it is always of the passive form. The congestion of the liver may be partial, and occupy only a portion of the organ, or it may engage the entire viscus. This cannot occur without occasioning an increase in the size of the organ, which will be in proportion to the degree of the congestion; to the increased amount of blood contained in the distended vessels: to the length of time the causes which produced it have been in operation; and to the condition of the liver at the time. "In young subjects, and in persons in whom the liver is healthy, and its capsule thin, Dr. Budd* observes, the liver will necessarily enlarge much more for a given force of distension than in persons in opposite circumstances." "When the liver (he adds) has become unnaturally firm and tough, by the interstitial deposit of new fibrous tissue, an impediment to the free passage of blood from it towards the heart, unless it be long continued, will produce but little increase of its size; but it will exert the same, or even greater pressure on the other elements of its texture, and be as apt, therefore, or even more apt, to cause secondary biliary congestion."

"In habitual, and particularly in permanent mechanical hyperemia of the liver, the vessels of the liver, as well as the trunk of the *venae portae*, and the branches from which it arises, are found (Rokitansky† observes) dilated and varicose." Habitual hyperemia of the liver, he adds, is apt to be followed by hypertrophy, and as a consequence of an increased production of portal blood, and an exaggeration of its peculiar qualities, the nutmeg liver may result, which again may give rise to granular degeneration of the organ.

The size which the liver may attain under these circumstances is remarkable: it sometimes extends downwards to below the umbilicus; anteriorly it forms a swelling evident sometimes to the eye, but always perceptible to the hand, while its enlargement upwards causes it to encroach upon the right lung. It must be borne in mind, however, as observed by Dr. Clendinning,‡ that the liver may be enlarged,

and yet not extend below its normal limits. "A large liver, well sustained superiorly, or strongly pressed upwards by tympanitis, or ascites, might pass (he observes) for small or normal; and a small liver, pushed down by emphysema, or pleuritic effusion above, or by stays outside and around, might pass for abnormally large." The actual size which the congested liver attains may, in general, be pretty accurately determined by percussion of the hepatic region.

Enlargement of the liver, the result of congestion, may usually be distinguished from enlargement of the organ from other causes by the liver preserving its normal shape, which is more or less altered in diseased states of the organ, as well as by the suddenness with which the enlargement supervenes and subsides again under treatment. Indeed, the rapidity with which the liver enlarges in some of these cases, and the great increase in size which the organ attains, have sometimes excited an unnecessary degree of alarm in the mind of the practitioner.

When the congested state of the liver has caused considerable increase in size of the organ, the enlargement upwards, which is sometimes from two to three finger-breadths above its normal limits, and as high as the inferior angle of the scapula, or above it, will impede the descent of the diaphragm, and will cause the left side of the thorax to be encroached upon, by which, as M. Gendrin* observes, the dyspnoea under which the patient ordinarily labours will be much aggravated. In addition, the patient usually complains, more or less, of an uneasy sensation in the hepatic region, particularly of a sense of weight: he does not suffer from pain unless pressure is made upon the part, and but seldom even then.

When biliary is combined with sanguineous congestion, the secretion of bile may be interrupted, or its escape may be impeded, owing to the pressure exercised upon the biliary plexuses and interlobular ducts, or to the swelling from congestion of the mucous membrane lining the ducts themselves. The patient will suffer from various symptoms of gastric derangement; as thirst, loss of appetite, vomiting, costiveness, or diarrhoea, with colicky pains, followed by jaundice in a slight or more marked degree, and frequently with diminished secretion of urine. "The gastric derangement which ensues in such cases has a pernicious reaction," Dr. Furnivall* observes, "on the original disease, the gastric irritation adding to the cardiac irritation and aggravating

* Diseases of the Liver.

† Path. Anat. vol. ii.

‡ Croonian Lectures, MEDICAL GAZETTE.

* Leçons sur les Maladies du Cœur.

† Lancet, 1846.

the paroxysms of dyspnoea, as well as exciting them; while the retention of those matters which should be excreted from, and by the agency of the liver, deprives us of one of our principal means of relieving the load of labour which is imposed on the heart." Frequent attacks of, or the long continuance of congestion of the liver, are followed by permanent increase in size of the organ, by congestion of the mucous lining of the alimentary canal, and by hæmorrhoids, or, in aggravated cases, by hæmatemesis; and when the patient's constitution has become much enfeebled, ascites generally sets in.

Any form of cardiac disease which directly or indirectly occasions an impediment to the return of the blood ascending by the inferior cava may give rise to congestion of the liver: the forms of cardiac lesion with which it is most frequently associated are—1st. Considerable contraction of the mitral orifice: here congestion of the lungs always precedes the hepatic congestion. Next in frequency, it occurs in cases where the tricuspid orifice is enlarged, and free regurgitation occurs at each ventricular systole; here the lungs are not engaged, but the venous system generally is engorged. Lastly, congestion of the liver is frequently associated with dilatation of the ventricles, with or without thinning of their parietes; this is frequently associated with a state of the tricuspid orifice permitting free regurgitation: here the congestion is partly due to this cause, and partly to the impediment to the passage of the blood through the chambers of the heart, and partly to the pressure exercised upon the ascending vena cava, as it is about to enter the right auricle, by the enlarged organ.

Congestion of the liver is occasionally consecutive to lesions of other organs beside the heart. This is not the place, however, to delay upon it; it will be sufficient to observe, that in any diseased condition of the lungs in which the pulmonary circulation is impeded congestion may ensue. Among these, pulmonary emphysema is one of the most frequent. The latter condition itself, however, is no unfrequent accompaniment of diseased states of the heart, and, when congestion of the liver occurs, it may depend as much upon it as upon the emphysematous state of the lungs.

Congestion of the Intestines.

The portal vein, we know, receives the blood from the superior and inferior mesenteric veins, from the splenic and gastric veins; consequently the blood returned from the spleen and pancreas, from the small and large intestines, must pass through

the liver before it reaches the right side of the heart. It is easy, therefore, to understand that if congestion of the liver occurs it will react upon the *arrière* current of blood, and, if long continued, that congestion of these organs will ensue. Hence congestion of the intestines is necessarily a secondary lesion to congestion of the liver, and will not be liable to occur in the cases we are considering unless congestion of the liver has preceded it; and, it is to be regarded as purely the result of a mechanical obstacle to the return of the venous blood from these parts.

In the slightest form, where the free return of the venous blood from the gastro-intestinal mucous membrane is merely impeded, the venous trunks simply become congested. When the impediment is greater, or the causes have been longer in operation, along with this state of congestion of the veins, "an injection of the small vessels in streaks, stripes, patches, or points, with opacity of the injected parts, will," Dr. Copland* observes, "be found; and, in the highest degree, a partial effusion of blood into the sub-mucous cellular tissue, forming ecchymoses, or into the cavity of the part, colouring red the matters contained therein." "The mucous membrane lining the intestines, or even the entire membrane of the intestines, may present," Rokitsansky† remarks, "an uniform reddish-black colour, the tissue being saturated with blood, and no injection of blood-vessels being distinguishable: the larger vessels, and particularly the venous trunks, are distended even as far as the mesenterics, and overcharged with blood. In either case hemorrhage may take place into the cavity of the intestines."

The simple form of congestion, when temporary, is not accompanied by any prominent symptom. When the congestion is more marked, when it has persisted for some time, and when the impediment to the return of the venous blood is greater, the train of symptoms which are laid down as characterizing dyspepsia set in, and the patient may refer all his complaints to the abdominal complication, so that it is possible the diseased condition of the heart might be overlooked. "Congestion generally," as Dr. Williams* observes, "impairs the vital properties of internal organs; natural contractility and sensibility are lowered, but pain, spasm, and morbid sympathies are often excited, although in a manner much less distinct and constant than in inflammation or determination of blood.

When the gastro-intestinal mucous mem-

* Dict. of Pract. Medicine.

† Path. Anat. vol. ii.

* Elements of Medicine.

brane has been long or considerably congested, its functions are more or less impaired, and its secretions, as the result, may be either increased or diminished, or vitiated; and according as the gastric mucous membrane, or that lining the large and small intestines, is particularly engaged, the symptoms will vary somewhat. Thus, when it is limited to the stomach, we may have nausea, vomiting, loss of appetite, and epigastric pain; when the mucous membrane lining the intestines is much congested we shall have flatulent distension, irritation, pain, and constipation, or diarrhoea, according as the secretion is increased or diminished, or altered; when the lower portion of the large intestine is congested we may have hæmorrhoids in addition.

The retardation of the circulation in the minute veins and capillaries, and the congestion of the mucous membrane which is the result, may be relieved in part by the transudation of the serous or aqueous parts of the blood, giving rise to diarrhoea with watery stools; or, by the escape of blood itself upon the mucous surface, giving rise to hæmatemesis; frequently by more or less copious discharges of blood from the hæmorrhoidal vessels. The secretion of urine is almost always at the same time diminished, and ascites usually begins to show itself at this period—always, however, preceded by anasarca of the lower extremities.

Epigastric Pulsation.

When hepatic is combined with gastro-intestinal congestion, and the impediment to the return of the venous blood is considerable, this must react upon the arteries which supply these viscera, and, through the celiac axis, and superior and inferior mesenteric arteries, upon the abdominal aorta at its upper part; the pulsation of this vessel becomes more distinct and stronger, constituting one variety of epigastric pulsation, and resembling the form described by Dr. Faussett,* and which was referred by him to local or visceral congestion, or to sub-acute inflammation. This variety of epigastric pulsation is accompanied by "pain on pressure at the epigastric region, or towards the umbilicus, with considerable fulness at the part; the digestive organs are always deranged, the bowels torpid, and the feet cold. The pulsation is most severe towards the afternoon, or soon after dinner, and is accompanied by a sense of vital depression. On auscultation a loud whizzing sound is heard."

A pulsation of the abdominal aorta, or its immediate vessels, which is symptomatic

of inflammatory disease in the abdomen, has been described by Dr. Stokes.* It consists in "a throbbing generally commensurate with the disease; removed by treatment calculated to relieve enteric inflammation, and aggravated by everything which will increase this affection." "In other words, we may have (he observes) from enteritis or peritonitis a throbbing of the abdominal aorta, or its vessels, perfectly analogous to the morbid action of the radial artery in whitlow, or of the carotids or temporal arteries in cerebritis." The diseases in which this pulsation was observed were gastro-enteric fever, peritonitis, and fever after corrosive poisoning.

A pulsation in the epigastric region which is evident to the eye, and perceptible to the hand, and of which the patient himself is conscious, is not unfrequent, however, under other circumstances. In describing the normal position of the heart, we saw that this organ rested upon the cordiform tendon of the diaphragm: now if the sternum is short, and the heart's action strong, there is no more difficulty in understanding how the heart's impulse may be felt in the epigastrium than there is to account for the impulse communicated to the abdominal parietes in the act of coughing; while if the heart itself is enlarged, or if the volume of the lungs is increased, and the heart is pushed somewhat downwards, as happens in emphysema of the lungs, its pulsation will be still more likely to be felt in the epigastrium; as the latter condition of the lungs is in general associated with some enlargement of the right cavities of the heart, particularly if it has persisted long: the epigastric pulsation is usually well marked in such cases. This form of epigastric pulsation is felt high up in the epigastric region; it is an undulatory movement, though quite perceptible to the hand placed upon the part, and is evidently produced by the heart itself. It usually occasions less distress to the patient than some of the other forms, and we are seldom called upon to treat it alone.

Nervous and hysterical pulsation.—Another kind of epigastric pulsation which is observed in subjects in whom the heart and lungs are both perfectly healthy occurs in nervous and hysterical individuals, particularly females. It is nothing more than the pulsation of the abdominal aorta communicated to the parts resting on it, owing to the action of the heart being temporarily increased. The pulsation is most marked below the epigastrium, and the impulse communicated to the hand is a short, smart jerk, very unlike that of aneurism. On auscultation a short whiff is audible, par-

* Dublin Journal of Medicine, vol. ii.

* Ibid. vol. v.

ticularly when the stethoscope is pressed firmly upon the part. This form of epigastric pulsation has remissions, or complete intermissions, and is aggravated or brought on by mental excitement, deranged digestion, or intemperance. The pulsation is usually perceptible to the patient, and is sometimes a source of considerable distress to him.

Anæmic pulsation.—A pulsation in the epigastric region communicated by the abdominal aorta, which occasionally complicates cardiac disease, occurs in anæmic subjects, particularly where this state has been brought on by profuse discharges of blood. The pulsation is strong, abrupt, and bounding, and often accompanied by a short, whizzing murmur, leading sometimes to the suspicion of aneurism. The pulsation is evident to the eye, as well as perceptible to the hand, is most marked below the epigastrium, and is, in a great measure, limited to the line of the artery. In such cases a similar pulsation and a similar sound are usually audible in the large vessels which come off from the arch of the aorta.

Many years since Dr. Baillie* called attention to a form of epigastric pulsation of which he had met some examples, and which probably belongs to some one of the forms already noticed. He observed it in persons about the middle period of life, most frequently in the male; the pulsation, which was often visible to the eye, was stronger in some subjects than in others, varied in strength at different periods, and was usually most marked in the horizontal posture. He believed it to be connected with deranged digestion and an irritable constitution. It is apt, he observes, to cause a good deal of unnecessary anxiety in the patient: it does not depend upon any diseased condition of the artery, for he has known it to continue well marked during twenty-five years without the health suffering. In two instances in which he had the opportunity of making a post-mortem examination, the artery was found to be perfectly healthy.

Pulsation communicated to abdominal tumors.—Tumors, or diseased states of some of the abdominal viscera—as of the pylorus, mesentery, or omentum, or air or feces confined in the transverse colon—not unfrequently have an impulse communicated to them, where they rest upon the aorta below the diaphragm. The pulsation is both evident to the eye and perceptible to the touch, and has sometimes been mistaken for aneurism. It may always be distinguished by the nature of the impulse, which wants the heaving character of the

impulse of aneurism in this situation; by the absence of bruit, or by its different character; by the mobility of the tumor, by the previous history of the case, and by the absence of pain, and of the other signs of abdominal aneurism.

Congestion of the Brain.

That a connection exists between organic disease of the heart and cerebral disease was noticed so long ago as the time of Morgagni; many clinical observations have been since brought forward to prove it, and abundant evidence of the fact has been, within the last few years, adduced by Dr. Clendinning,* by Dr. Burrows,† and Dr. Law.‡

The circulation in the brain may be disturbed in more than one way, as the results of disease of the heart; thus—

1. The blood may be transmitted to the brain in increased amount, or with undue force, by an hypertrophied left ventricle.

2. There may be an impediment to the free return of the venous blood from the brain to the right side of the heart, owing to disease of the latter organ.

3. The blood may not undergo the necessary changes in the lungs, owing to disease of the valves or orifices of the heart, when a mixture of venous and arterial will be transmitted to the brain.

4. The blood may be transmitted to the brain in deficient quantity, as pointed out by Dr. Law, owing to obstructive disease of the orifices of the left side of the heart.

Lastly. Independent of disease of the heart, the blood itself may be thin, watery, and deficient in some of its constituents, as in anæmia; or it may be rich, and quickly formed, as in plethora.

Writers upon diseases of the heart have laid considerable stress upon the influence of hypertrophy of the left ventricle, in causing cerebral congestion, by its transmitting the blood in increased quantity, or with augmented force, to the brain. The arteries which supply the brain run a short course from the arch of the aorta, and they receive, almost immediately, the force of the impulse of the left ventricle; the circulation in the head and face is consequently active, indicated by the greater heat of these parts than of the extremities. Hence, we should expect, that when the left ventricle is hypertrophied and dilated, cerebral congestion would be likely to ensue; but in many instances hypertrophy of the left ventricle, with or without dilatation of its cavity, is itself a secondary lesion, and the result of some impediment to the passage of the blood through, or out

* Croonian Lectures, MEDICAL GAZETTE.

† Disorders of the Cerebral Circulation.

‡ Dublin Journal of Medicine, vol. xvii.

* Trans. of College of Physicians, vol. iv.

of the chambers of the heart. Hence, although the heart's action may be very strong, the pulse is often weak under such circumstances; showing that the strength of the current is not necessarily augmented, or that the amount of blood transmitted by the ventricle is not always increased when the left ventricle is hypertrophied, and its cavity dilated.

In considering congestion of the liver, we found that one effect of congestion was to cause an increase in the size of the congested organ; and we can readily understand that such a delicate organ as the brain, which is enclosed within solid bony walls, could not undergo even slight enlargement, without the cerebral substance and nerves suffering compression.

That injurious effects do not more frequently ensue, has been explained by Dr. Burrows, as follows:—1st. "Owing to the great development of the venous system within the cranium and spinal canal, which affords a ready exit for the redundant blood;" and 2dly, "Owing to the existence of the cerebro-spinal fluid." This fluid he has shown to play a most important part, as—"by its capability of oscillation between the ventricles of the brain and the spinal canal, it compensates for variations in the quantity, or tension of the circulating fluids of the brain. When by increased action of the heart more blood than common is thrown upon the cerebral vessels, the cerebro-spinal fluid subsides into the spinal canal, and injurious pressure upon the cerebral pulp is thus, within certain limits, abated; and on the other hand, if less blood than usual pass into the brain, the fluid rises into the cranium, and the equilibrium of tension is preserved."

Although cerebral congestion may be the result of hypertrophy of the left ventricle, this state is even more likely to ensue, if there is any impediment to the free return of venous blood from the brain. In a state of health this fluid readily returns from the head, being assisted by gravity, and any thing which interferes with it is quickly felt; thus, if the head remains for any length of time on a plane lower than the body, symptoms of cerebral plethora will ensue; as the internal jugular veins are not provided with valves, there is no provision to prevent the reflux of their contents towards the brain. Indeed, persons partly asphyxiated by submersion, or in a state of stupor from intoxication, have often been deprived of life by being carelessly removed in a position which allowed the head to hang downwards.

"Pathologists hitherto have," Dr. Law* observes, "almost entirely confined their

attention to the excess of the normal quantity of blood, or the congestion produced in organs by disease of the heart, but have overlooked the deficiency that must necessarily result to other organs, and consequent hurt to their nutrition and function." He was the first to call attention to the effects of a diminution of the supply of blood to the brain; and he has shown that "ramollissement of the brain occurs in connection with diseases of the heart, whose effect is either directly or indirectly to diminish the flow of blood to the head;" and that "this cerebral lesion may be connected with either disease of the aortic and mitral valves; particularly with considerable contraction of the latter orifice.

When cerebral congestion arises under any of the circumstances mentioned, the patient will suffer from headache, vertigo, tinnitus aurium, or ocular spectra; vague sensations about the head, of rushing, heat, and noise, &c., will be experienced, or heaviness of the head and drowsiness will be the most prominent symptom; the face may be flushed, or the cheeks and lips congested, or it may on the contrary be very pale. "In some instances," Dr. Furnival^o observes, "headache is early complained of, and is both severe and long continued. This is doubtless caused by cerebral venous congestion, and it and other cerebral symptoms have been sometimes so prominent as to mask and to draw attention from the original and originating disorder." When the venous blood is not entirely converted into arterial in the lungs, a mixture of venous and arterial blood is conveyed to the brain, evidenced by drowsiness and stupor, or by wandering of the mind, and a transitory and sometimes pleasing kind of delirium. The latter results are not, however, observed as often as might be expected; the majority of those who die of cardiac disease preserve their intellects perfectly to the close.

Apoplexy and paralysis are considered by several writers to be direct results of hypertrophy of the left ventricle. Thus Dr. Hope, in the *Cyclopædia of Medicine*, and subsequently in his work on *Diseases of the Heart*, remarks:—"Instances of apoplexy supervening upon hypertrophy have been so frequently noticed, that the relation of the two, as cause and effect, is one of the best-established doctrines of modern pathology. Eight or nine cases of suddenly fatal apoplexy, and numerous cases of palsy from hypertrophy, have within a few years fallen under our observation." "Whence we are led to the conclusion, with MM. Richerand and Ber-

* Dublin Journal of Medicine, vol. xvii.

* Lancet, 1846.

tin, that hypertrophy forms a stronger predisposition to apoplexy than the apopleptic condition itself."

It appears to me, however, that the effect of hypertrophy of the left ventricle upon the brain has been overrated; and although in many cases of apoplexy or paralysis, terminating fatally in individuals advanced in life, the ventricles are found to be hypertrophied, yet we usually find the coats of the arteries of the brain to be likewise diseased; in addition, the valves at the left side of the heart are also diseased, by which the return of the venous blood from the brain had been impeded. It is to this latter condition, in connection with a morbid state of the coats of the arteries of the brain, that the majority of the cases of this kind should be referred. Indeed, Corvisart, no mean authority, goes still further, and says, that he never saw organic disease of the heart directly occasion apoplexy and death. "I have," he says, "many times, in cases of this kind, seen all the vessels of the brain, and particularly the sinuses, gorged with blood, but I have never met with extravasation of blood either in the substance of the brain or in its cavities." "In several of the cases related elsewhere the death was sudden; I cannot, however, he adds, assert that I have ever seen a single case of apoplexy which was evidently the result of cardiac disease."

[Conclusion of the First Part of the Course.]

COLLODION APPLIED TO BURNS.

DR. LIMAN, of Berlin, states that he has found collodion a most excellent application to burns. He has applied it in many cases with the best results. He states that it allays the smarting, forms a protective covering, which excludes the action of the air, and is so exactly adapted to all parts that no other dressing is required. The first application is attended with some pain, but is soon followed by alleviation of the suffering, and the cure proceeds steadily without pain. Dr. Liman applied the collodion with a hair-pencil, covering the entire surface, and daily reapplying it to the fissures and uncovered parts. Dr. Liman relates one case in which it was applied in an extensive burn with immediate advantage, and ultimately a speedy cure, without remaining contractions of the integuments. — *Casper's Wochenschrift*, 1850.

. We can confirm Dr. Liman's recommendation, having found collodion equally serviceable with the author in burns, and more especially useful with children, on whom it is often difficult to retain ordinary dressings.

Original Communications.

CASES, WITH REMARKS, ILLUSTRATING THE ASSOCIATION OF CHOREA WITH RHEUMATISM AND DISEASE OF THE HEART.

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(Read before the Abernethian Society, March 7th, 1850.)

[Concluded from p. 1009.]

For the reasons stated at the commencement of this paper, I shall refrain from entering into any discussion of the several theories advanced in explanation of the occurrence of nervous phenomena in the course of acute rheumatism. The details, however, of some of the cases I have narrated suggest a few remarks on the principal alleged cause of the development of these nervous symptoms during rheumatism—viz. the existence of some inflammatory affection of the heart, but especially of the pericardium. Now, admitting, as I do most fully, the influence which acute inflammation of the heart or its membranes no doubt frequently exercises in inducing the most varied disorders of the nervous system, and believing that the cardiac complication in articular rheumatism has a large share in the production of chorea, or other nervous disorders which may arise in the course of the attack, yet there appear to be some circumstances favouring an opinion that such inflammatory affection of the heart, or at any rate of the pericardium, is not essential to the development of any of the nervous symptoms in question. The two principal circumstances in favour of such an opinion afforded by the cases I have narrated, and by others presently to be noticed, are first, that chorea occasionally arises in the course of articular rheumatism without the existence of any positive evidence that the heart or pericardium is inflamed; and second, that, as in Cases III. and V., chorea is sometimes developed first, the rheumatism, with or without cardiac complica-

tion being only subsequently super-added.

The two facts just mentioned,—namely, the occasional occurrence of chorea in articular rheumatism uncomplicated by any decided cardiac affection, and the occasional occurrence of chorea *previous* to the onset of rheumatism or of any affection of the heart, naturally suggests an inquiry whether there is not some more general cause for the association between the chorea and the rheumatism than that ascribed to an affection of the heart or pericardium. Apparently impressed with these, and other similar facts, irreconcilable on any previous explanations, Dr. Begbie has recently advanced a very ingenious theory on the subject.

He suggests that the same diathesis or morbid condition of the blood which gives rise to rheumatism, may give rise also to chorea; and he founds his opinion on a fact which I have myself also several times noticed, that out of the same family one member may be affected with chorea, another with rheumatism, while a third may be the subject of both these affections. In instances of this kind the tendency usually seems to be hereditary, one or other of the parents having generally manifested a liability to rheumatism. This view, that the same diathesis may lead to the occurrence either of chorea, of rheumatism, or of both, as the case may be, has many circumstances in its favour, while it explains difficulties which on other hypotheses are inexplicable, and reconciles many facts otherwise opposed to each other, especially the occurrence of chorea previous to the onset of the rheumatic attack. Moreover, it receives the support of Dr. Watson,* and also seems to accord with the observation of Dr. Todd, that "many of the patients who suffer from chorea are of a rheumatic diathesis."†

However inclined one may feel to agree in the general probability of such an explanation of the occurrence of chorea in rheumatism, it seems to me, nevertheless, that there are still several interesting facts to which too little attention has hitherto been paid in the investigation of this subject, especially as these facts appear to help to a better understanding of the associa-

tion in question. While examining this subject it occurred to me that much information might be gained by collecting a number of cases from various authentic sources, and arranging them in a tabular form, constructed so as to show at a glance all the important points in each case. I have therefore brought together 36 cases in which symptoms of chorea were observed in connection either with articular rheumatism alone, or with acute disease of the heart alone, or with rheumatism and cardiac affection combined. (See p. 1052).

On analysis of these 36 cases it will be seen that the chorea was more or less closely associated with rheumatism in 33, while in the remaining 3 there was recent disease of the heart without any affection of the joints. The latter number might have been considerably increased had I included in the table all cases of simple chorea in which an endocardial murmur was heard; but these were purposely omitted, as not seeming to bear on the subject in hand; for it is a question how far in such cases the murmur is dependent on actual organic disease of the heart. In each of the three cases I have given, however, there was distinct evidence of cardiac disease, recent pericarditis in two (1 and 18), and affection of the mitral valve in the third (25).

The next point of interest in the analysis which may here be noted refers to the total number of cases in which there was disease of the heart. Unfortunately this point cannot be determined with certainty; for in 5 of the cases (10, 30, 31, 32, 34) the condition of the heart is not stated, while in three others (4, 21, 24) there was no positive evidence either for or against any cardiac affection. Excluding these doubtful cases, however, we have still twenty-eight in which the state of the heart was noted; and of these we have strong evidence for believing that the heart or its membranes were affected in all but two (13, 23). This very large proportion shows the important share which the cardiac affection doubtless takes in the development of the nervous phenomena: yet the existence of even but two cases of chorea and rheumatism, unaccompanied with disease of the heart, prevents the conclusion that the cardiac complication is invariably an essential element in the production of the nervous symptoms. Besides, it must be remembered that at

* Practice of Physic, 3rd edit. Vol 1, p. 663.

† LONDON MEDICAL GAZETTE, 1846, p. 664.

least in three of the cases (21, 22, 35), the symptoms of chorea ensued before either the rheumatism or affection of the heart appeared. Hence, I think, we may conclude from this part of the analysis, that chorea may be associated with articular rheumatism alone, or with disease of the heart alone, that neither are essential to its occurrence, while it is from the combined influence of the two together that it is most likely to be developed.

On examining into the nature of the cardiac affection in the twenty-six cases in which the heart was said to be diseased, we find in

10 cases, effects of both endocarditis and pericarditis (2, 8, 9, 16, 17, 19, 20, 22, 27, 36).

11 cases, effects of endocarditis alone (5, 6?, 7, 12, 14, 15, 25, 26, 28, 29, 35).

5 cases, effects of pericarditis alone (1, 3, 11, 18, 33).

The endocardium, therefore, presented evidences of disease in no less than twenty-one out of these twenty-six cases, the pericardium only in fifteen. Therefore, if the mere weight of numbers might determine whether affections of the endocardium or of the pericardium were the most likely to be associated with symptoms of chorea, the verdict would be in favour of the former. And such verdict may, I think, be substantiated by several other not unimportant particulars; for, having found the endocardium affected in so large a proportion of these cases, we may be led to inquire whether there is any reason for believing that it might have been affected in the remaining five cases also. In one of these cases (18) the mitral valve was said to be "somewhat thickened;" in the remaining four the condition of the endocardium is not named, and therefore, perhaps, it ought to be inferred that it was sound; but when it is remembered how slight the effects of endocarditis frequently are, consisting only of a few minute beads of lymph along the borders of one or more valves, which might be easily overlooked after death, while their auscultatory signs during life would be masked by those of the pericarditis which existed in all the cases, and that in the fatal cases attention would probably be directed chiefly to the extensive disease presented by the pericardium, especially in cases 1

and 3, which occurred at periods (1821 and 1824) when the appearance of minute granules on the valves of the heart would have attracted much less attention than at the present time, and if seen, might have been deemed unworthy of note,—we may conclude that the non-existence of endocarditis is at least not proved in any of these cases. We might, indeed, go even further than this, and conclude, not unreasonably, that the endocardium was probably affected in one or more of them, remembering "that pericarditis is more frequently found in combination with endocarditis than alone."* It may, perhaps, be thought that, by parity of reasoning, the absence of pericarditis in those cases in which its existence is not stated ought to be considered as also unproved; but the likelihood of the disease being overlooked, either before or after death, is so much less in the case of pericarditis than of endocarditis that this mode of reasoning could have but little weight.

Since, therefore, we have tolerably strong proof that the endocardium was affected in twenty-one out of the twenty-six cases in which the condition of the heart was stated, and some reason for believing that it might be affected in some of the remaining five; while, on the other hand, we have proof of the pericardium being affected in only fifteen, and no positive reason for believing it to be affected in the remaining eleven,—we are, I think, justified in concluding that of the two diseases the affection of the endocardium is more closely associated with the development of nervous phenomena in acute rheumatism than is affection of the pericardium. Such conclusion naturally leads to the supposition that the cardiac murmurs, so frequently heard in cases of chorea unassociated with rheumatism, may also be dependent on some organic disease of the interior of the heart, "an insidious endocarditis affecting the mitral valves,"* and that such a morbid condition may have an important share in the production of the choreic phenomena. The mode in which such affection of the endocardium can bring about the nervous symptoms involves inquiries too lengthened for the present communication: yet it may be observed that there

* Latham, *Diseases of the Heart*, vol. i. p. 145.
* Dr. Todd, *MEDICAL GAZETTE*, 1842, p. 564.

No.	Sex.	Age.	Severity of Rheumatism, if present.	Nature of the Nervous Affection.	Date of commencement of Nervous Symptoms.	Previous Affection.	Affection of Heart.	Relation in point of time between commencement of Nervous and Cardiac Symptoms.	Results.	Remarks.	Authority and Reference.
1	F.	16	No rheumatism.	Delirium; chorea.	Chorea about 3d week; delirium earlier.	None noted.	Pericarditis (idiopathic).	Cardiac symptoms preceded the chorea.	Temporary recovery; subsequent death.	After death were found effects of pericarditis; lungs indurated; other viscera healthy; condition of endocardium not named.	Abercrombie, Trans. of the Med. Chir. Soc. of Edinburgh, vol. i. 1831, quoted by Dr. Burrows.
2	M.	17	Slight.	Chorea (very violent); delirium.	6th day after commencement of rheumatism, which had subsided.	Do.	Pericarditis; Endocarditis.	Unknown.	Death in 3 weeks.	Lymph in pericardium; vegetation on valves of left side of heart; brain healthy.	Dr. Bright, (Med.-Chir. Trans. vol. xiii.), quoted by Dr. Burrows.
3	F.	27	Severity not stated.	Delirium, with jactitation.	3d week of rheumatism.	Do.	Pericarditis.	Almost coincident.	Recovery.	Endocardium not named, but probably not affected.	Dr. Macleod, (on Rheumatism, &c.), quoted by Dr. Burrows.
4	F.	16	Moderate.	Chorea; delirium.	Not clear.	Do.	None detected.	—	Do.	Not clear that the heart was unaffected.	Dr. Burrows, (loc. cit. p. 196).
5	F.	14	Do.	Chorea.	3d week.	Chorea when 7 yrs old.	Systolic murmur at apex (endocarditis).	Both present on admission.	Do.	No previous rheumatism.	Dr. Ormerod, (MS. notes)*
6	F.	14	Do.	Chorea (slight).	3d week. (?)	None noted.	Double cardiac murmur.	Cardiac murmurs on admission; chorea 10 days afterwards.	Do.	Not clear whether pericarditis existed with the endocarditis. Sister to No. 19.	Do.

7	F.	14	Do.	Chorea (severe).	Do. (?)	Chorea. 18 months before.	Endocardial murmur.	No note of auscultation till 11 days after chorea.	Death (3 weeks after chorea).	Pericardium healthy; beads on aortic and mitral valves; brain and spinal cord congested. Never menstruated; anteversion of uterus.	Dr. Ormerod.
8	M.	10	Do.	Chorea.	See Remarks.	Chorea, unassociated with rheumatism, 4 years previously.	Recent endocarditis; recent and old pericarditis.	Both present on admission; (see "Remarks")	Do. (3 weeks after chorea).	A year before had rheumatism, with pain in heart and "inflammation of lungs," 4 weeks ago, pain in chest, followed in one week by chorea. Old pericardial adhesion; also soft lymph. Recent granules on tricuspid, mitral, and aortic valves; brain and spinal cord quite healthy.	Do.
9	M.	16	Severe.	Chorea (severe); delirium; convulsions.	10th day of rheumatism.	None noted.	Pericarditis; endocarditis.	Pericarditis on admission, 3 days before chorea began.	Death (7th day of chorea; 17th of rheumatism).	Recent lymph in pericardium; fibrinous deposit on mitral and aortic valves.	Do.
10	M.	14	Severity not stated.	Chorea.	On convalescence from rheumatism.	Chorea twice before.	Not noted.	—	Recovery.	At 8 years old had rheumatism; 4 months afterwards had chorea; in another year chorea again.	Dr. Todd, (Med. Gaz. N.S. vol. viii. p. 664).
11	F.	19	Moderate.	Do.	6th day of rheumatism.	Chorea 18 months previously.	Pericarditis.	Not noted.	Death.	Died after 4 days of chorea and 10 from commencement of attack; adherent pericardium.	Dr. Pritchard, (London Medical Repository, vol. xxi. p. 2).

* I have to thank my friend Dr. Ormerod for permission to analyse his valuable notes of this and the four following cases.

No.	Sex.	Age.	Severity of Rheumatism, if present.	Nature of the Nervous Affection.	Date of commencement of Nervous Symptoms.	Previous Nervous Affection.	Affection of Heart.	Relation in point of time between commencement of Nervous and Cardiac Symptoms.	Results.	Remarks.	Authority and Reference.
12	F.	15	Ordinary.	Chorea.	On decline of rheumatism.	Chorea when 12½ years old.	Endocarditis.	Endocardial murmur at 3d week of rheumatism; chorea later.	Recovery.	Sister had chorea; brother had rheumatism and pericarditis, without chorea.	Dr. Begbie, (Monthly Jour. of Med. 1847).
13	M.	13	Do.	Chorea (slight).	During or after rheumatism.	None noted.	None.	—	Do.	Brother to next case; had rheumatism before.	Do. (loc. cit. p. 746).
14	M.	14?	Do.	Chorea.	Do.	Do.	Endocarditis.	About coincident.	Death.	Died after chorea of several months' duration. Several previous attacks of rheumatism; heart healthy till last attack; diseased aortic valves after death.	Do.
15	M.	18	Severe.	Do.	On decline of rheumatism.	Do.	Do.	Not clear.	Recovery.	Slight but protracted chorea on convalescence from rheumatism.	Dr. Babington, (Guy's Hosp. Rep. vol. vi. 1841, p. 419).
16	F.	15	Ordinary.	Chorea (slight).	24th day of rheumatism.	Do.	Endocarditis; pericarditis.	Chorea subsequent to cardiac affections.	Do.	Chorea in left arm only, of short duration.	Do.
17	F.	16	Severe.	Chorea (moderate).	On decline of rheumatism. (?)	Hysterical.	Do.	Do.	Do.	Catamenia regular; began to menstruate at 13; the chorea began to subside on appearance of catamenia. Had rheumatism before.	Do.

	18	F.	15	None.	Chorea.	See Remarks.	None noted.	Pericarditis.	Uncertain.	Death.	Chorea for 6 weeks. Seemed to be getting well, when suddenly ensued convulsions, dyspnoea, and death in a few hours. Abundant old and recent lymph found in pericardium. Mitral valve somewhat thickened.	Do.
	19	F.	20	Very slight.	Chorea (severe).	7th day of illness.	Do.	Pericarditis; endocarditis; fatty discase.	Both present when first seen.	Do.	Sister to No. 6. Catamenia ceased at commencement of attack.	Dr. Kirkes, (Case 1, narrated) M. A. Burke.
	20	F.	13	Ordinary.	Chorea (slight).	16th day of rheumatism, which had almost subsided.	Do.	Pericarditis; endocarditis.	Chorea subsequent to cardiac affection.	Temporary recovery; subsequent death.	See particulars of case.	Do. Case 2—Ann Holt.
	21	F.	14	Severe.	Chorea.	Before the rheumatism.	Several attacks of chorea.	None. (?)	See "Remarks."	Recovery.	Three years later had chorea again, independent of rheumatism; at that time catamenia regular, and heart-sounds healthy.	Do. Case 5—Elizabeth Lark.
	22	F.	11	Ordinary.	Do.	Do.	Do.	Pericarditis; endocarditis.	Nervous affection preceded cardiac.	Do.	Two years afterwards had rheumatism and pericarditis without chorea.	Do. Case 3—Eliza Blenning.

No.	Sex.	Age.	Severity of Rheumatism, if present.	Nature of Nervous Affection.	Date of commencement of Nervous Symptoms.	Previous Nervous Affection.	Affection of Heart.	Relation in point of time between commencement of Nervous and Cardiac symptoms.	Results.	Remarks.	Authority and Reference.
23	M.	14	Severe.	Chorea.	During the rheumatism.	None noted.	None. (?)	See notes of case.	Recovery.	Ten years afterwards had another attack of rheumatism; on subsidence of which, severe chorea ensued, unaccompanied with signs of cardiac disease; a few months afterwards another attack, with endocarditis. First men-trusted during this attack; subject to chorea afterwards. Nothing detected after death but granules on mitral valve.	Dr. Kirkos. Case 4—William Harvey.
24	F.	15	Ordinary.	Do.	Do.	Repeated attacks of chorea.	Do.	—	Do.	First men-trusted during this attack; subject to chorea afterwards.	Do. Case 6—Maria Potter.
25	F.	17	None.	Do.	—	None.	Endocarditis.	—	Death.	Nothing detected after death but granules on mitral valve.	Do. Case 7—Ellas Fanchild.
26	M.	16	Ordinary.	Chorea (very severe).	On subsidence (3d week) of rheumatism.	None noted.	Do.	Uncertain.	Recovery.	His father, mother, and sister had shown evidences of insanity. It was so impossible to attack him during the long continuance of the violent chorea, that pericarditis might easily have existed and been overlooked.	Do. William Hobbs.
27	M.	10	Do.	Chorea.	On subsidence (5th week) of rheumatism.	Do.	Endocarditis; pericarditis.	Both existed when first seen, but appeared to have commenced almost coincidently.	Do.	—	Do. John Gilbert.

28	F.	17	Severity not stated.	Do.	In course of rheumatism.	Do.	Endocarditis.	—	Do.	"Bruit, at first audible below the mamma, disappeared under treatment."	Dr. Hughes, (Guy's Hosp. Rep. vol. iv.) Case 10.
29	F.	14	Do.	Do.	Not stated.	Do.	Do.	—	Relief.	"Bellows-murmur over aortic valves and below mamma."	Do. Case 17.
30	F.	13	Do.	Do.	In course of rheumatism.	Do.	—	—	Recovery.	Condition of heart not named.	Do. Case 33.
31	F.	19	Do.	Do.	2 (?) months after rheumatism. Not stated.	Do.	—	—	Do.	Do.	Do. Case 45.
32	M.	18	Do.	Do.	Not stated.	Do.	—	—	Do.	Do.	Do. Case 77.
33	F.	15	Do.	Do.	In course of rheumatism.	Do.	Pericarditis.	—	Do.	Endocardium not named.	Do. Case 98.
34	M.	18	Do.	Do.	Do.	Do.	—	—	Do.	"Heart permanently diseased."	Do. Case 99.
35	F.	15	Ordinary.	Do.	Before rheumatic or cardiac affection.	—	Endocarditis.	Chorea for 4 months; then rheumatism; then endocarditis.	Do.	Heart not named.	Do. Page 376.
36	F.	16	Do.	Do.	See "Remarks."	Do.	Pericarditis; endocarditis.	Almost coincident.	Death.	Rheumatism subsided first, then chorea, then the endocardial murmur. Only menstruated once, 5 months previously. A month before had left the hospital well from an attack of rheumatism, pericarditis, and chorea; returned with dyspnoea, cardiac pain, and general distress. In 17 days after readmission, pericardial friction, with chorea, which became most severe, and terminated fatally in 4 days. Solid lymph of various dates in pericardium; vegetations on aortic and mitral valves; firm coagula in each ventricle.	Do.

are reasons for believing that the efficient cause is in some way closely connected with that condition of the endocardium, or of the blood, or of both, which leads to the deposition of the fibrinous granules on the margins of the valves of the heart.*

Whatever may be the immediate exciting cause which calls the nervous phenomena into existence, there appears to be sufficient reason for believing that in most of the cases in which such phenomena arise there pre-exists a peculiar proneness to the development of nervous disorders, and that the rheumatic or cardiac affection occurring in persons possessed of such an evidently irritable nervous system, gives rise to symptoms which, in persons less predisposed to nervous affections, would probably not be developed. This opinion seems to suggest itself from a further analysis of the thirty-six cases I have tabulated, for such analysis demonstrates three important points:—1st, that the chorea is much more common (as has been often observed before) in females, in whom the nervous system is peculiarly prone to disorder, than in males, in whom this proneness does not exist; thus of the thirty-six cases two-thirds (twenty-four) occurred in females, one-third (twelve) in males. 2dly, that a large majority of the cases occurred at that period of life in which there naturally exists, especially in females, a peculiar tendency to nervous affections,—namely, the period of puberty, or of the first onset of the menstrual functions. Thus, on examining the ages of the several cases, we find that among the females the chorea occurred in one at 11 years old, in two at 13, in five at 14, in six at 15, in four at 16, in two at 17, in two at 19, in one at 20, and in one at 27. Out of the twenty-four females, therefore, no less than seventeen were between the ages of 13 and 17,—i. e., during the critical period

attending the development of the catamenial functions. A review of the male cases affords very similar results; for with the exception of one case, in a boy of 10, all the other cases occurred between the ages of 13 and 18.* 3dly, that in several of the cases there was distinct evidence of predisposition to nervous affections. Thus it is noted that no less than nine of the patients (5, 7, 8, 10, 11, 12, 21, 22, 24) had suffered from previous attacks of chorea, which, with one doubtful exception (10), were unassociated with rheumatism; and another case (17) occurred in a nervous hysterical girl. This number might probably have been greatly enlarged had attention been paid to this point in the history of the other cases; but unfortunately in nearly all of them there is no mention of the existence or non-existence of any previous nervous affection.

There are one or two other points exhibited by the cases, to which I may briefly allude before concluding this communication. On glancing at the fourth column, which relates to the severity of the rheumatic attack when present, it will be observed that of the thirty-three cases in which the chorea was more or less directly associated with rheumatism, the rheumatic attack is described as being

Severe in	cases.
Ordinary or moderate in 17	"
Slight in	2 "
Severity not stated in	9 "

It may be assumed that in the last nine cases, in which there is no record of its severity, the attack was either slight or ordinary. But even supposing these nine to have been severe, we still find that in more than half the number the rheumatic attack was moderate or of an ordinary kind, and that therefore the development of the nervous phenomena cannot be ascribed to the severity of the rheumatism. From the sixth column, again, we learn that in those cases in which the period of the commencement of the chorea is noted, the nervous symptoms usually

* As bearing on the comparative frequency with which the pericardium and endocardium are affected in chorea, I may also notice that the valves of the heart were affected in all six of the fatal cases of chorea tabulated by Dr. Hughes, in which the condition of this organ is named; while the pericardium was found inflamed in only three of them, and in no case without coincident affection of the valves. The affection of the valves in four of the cases consisted in the presence of granules of fibrine on the borders of one or more of the valves, while in the other two cases the mitral valve is described as being merely "opaque" and "thick."

* I need not go further into this part of the subject, since the same views in relation to chorea in general, though not to chorea in rheumatism, have been fully developed by Dr. Hughes in his valuable paper in Guy's Hospital Reports (vol. vi.), with which I did not become acquainted until after the substance of this communication was read before the Abernethian Society.

arose, not at the beginning or climax of the rheumatic attack, but on its subsidence. The ninth column, relating to the precedence of the cardiac or nervous symptoms, is occasionally incomplete, from the impossibility in many cases of obtaining the requisite information; but in most of the instances in which such information was procured, it will be seen that the cardiac affection preceded the development of the nervous phenomena.

Of the thirty-six cases here tabulated no less than eleven, almost one-third, terminated fatally. In all of these there was found some affection of the heart after death,—viz., pericarditis in 8, and endocarditis in 9. The brain and spinal cord, when stated to have been examined, as they were in at least five cases (2, 7, 8, 19, 25), presented no trace of inflammation, and nothing which seemed to bear particularly on the pathology of the disease. This is important in reference to the opinion which has been advanced, that the choreic symptoms are dependent on extension of the rheumatic inflammation to the fibrous membrane of the brain or spinal cord; for it shows that such symptoms may ensue and terminate fatally, without leaving any trace of inflammatory affection of the membranes or substance of the nervous centres, and fairly allows of the inference that in favourable cases also the nervous centres are equally free from inflammatory changes.

SUBMISSION TO AUTHORITY IN MEDICINE.

SPECULATIVE truth and the resulting practice lie within the domain of opinion, which is by nature free, and cannot be brought into bondage to any man, or any number of men. Moreover, it is not necessary that all questions should be settled and ended; it is necessary only that the truth should be known, and when every man has given his testimony faithfully he has done his whole duty; a step farther, and he is trespassing on the rights of others. Settling a question of opinion by authority is only in fact unsettling a great principle, by arresting inquiry, and forbidding future experience to illustrate and modify the past. Science is not a despotism, and its real cultivators are all equally freemen; and their liberty is as essential to the progression of truth as it is to individual honour.—*American Journal of the Medical Sciences.*

ON THE RISE, PROGRESS, AND VARIOUS TERMINATIONS OF CHRONIC OVARIAN TUMORS.

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[Continued from p. 962.]

HAVING described the effects of ovarian tumors on the unimpregnated womb, we shall now enter upon a subject of more practical importance, and investigate—

2ndly.—The effects of ovarian tumors on the impregnated womb.

When pregnancy coincides with the existence of an ovarian tumor, it has been known to determine abortion when there was still ample room for the development both of the pregnant womb and of the tumor; but this is of very rare occurrence: it has more frequently brought on premature labour or necessitated repeated punctures, as in Emery's case, who thought it necessary to tap a patient six times to prevent this occurrence. Generally speaking, however, the morbid and physiological tumors progress side by side until the full period of pregnancy, as in a case which has recently occurred, wherein after a woman was delivered of a full-born child, "A large house-pail of fluid" was taken from an ovarian cyst. Impeded respiration rendered it necessary to tap the patient, who died under the operation, but unfortunately the relations would not permit a post-mortem examination.

When, from the length of its pedicle, or from its mode of attachment, the tumor is situated by the side or above the enlarged womb, it neither impedes labour nor aggravates its phenomena.

Often a small or moderately-sized tumor, or a lobe of an irregular shape, prolapses into the recto-vaginal space before parturition has come on, or descends into this space towards the end of gestation, when nature is preparing for the delivery of the woman by a general relaxation of the soft parts of the pelvic cavity. Such cases have been described by Merriman, Denman, Ingleby, Madame Lachapelle, and others; and they often seriously complicate labour, unless, as we shall

see when we come to the treatment of these affections, there is a possibility of pushing up the tumor above the womb, in imitation of what in similar cases has often been effected by the sole powers of nature. In the following case, related by Madame Boivin, the tumor was displaced by a species of evolution. It suffered with impunity the irritation of one labour, but in that which followed the cyst inflamed and caused the patient's death by its rupture.

On the 31st July, 1831, says Madame Boivin, I visited Mrs. M—— early in the morning. She had been in labour some hours. On examination, I found a tumor filling the vagina, which seemed to grow from its upper and posterior surface. It seemed to be a solid and fleshy mass, and its magnitude gave me great apprehensions for the result of the labour. I had attended the patient in former labours, when she had always done well, and no such obstruction had ever, till then, been met with. The pains were violent, and the head of the child made slow progress in front of the tumor. After more violent and protracted efforts, the child was born in the evening of the same day. Immediately after delivery I examined the vagina, but the tumor had disappeared, and the patient got well as usual without anything occurring worthy of notice.

May 9, 1834.—Again attended Mrs. M—— in labour. On examining, I found the tumor as before, and I was as much at a loss as ever to determine its nature. The labour was accomplished with much less difficulty than on the former occasion, and again the tumor disappeared. There was some hæmorrhage, but the placenta was expelled by pains; the uterus seemed to be well contracted, and the hæmorrhage ceased. Her pulse was quick, and she complained of unusually severe after-pains, especially in the back. In half an hour I was again called to her. She had now the appearance of sinking from hæmorrhage, but there was no discharge. The pain in the back was agonizing: opiates failed to produce relief; the exhaustion kept increasing, and she died in about two hours. Next day we examined the body, which had a very exsanguine appearance. The uterus was contracted and without blood in its cavity, nor was there any substance in the vagina. A fleshy-looking tumor

was seen rising from the left side of the pelvis, which on being cut into was found to contain about a pint and a half of pus, mixed with a considerable quantity of hair appearing like tow. The hair did not seem to be attached to the sides of the bag which held it, but to be loose in the cavity. This tumor I believe to have been the left ovary, and the same which, on the occasion of the two labours, had been found in the pelvis, protruding the vagina before it and producing the impediment to delivery.

From this narrative, it appears that the ovary was in each labour forced down by the child's head, and, excepting on the occasion of the last delivery, was drawn up again into the abdomen immediately afterwards.

The following interesting and unpublished case of mismanaged labour occurred in the practice of my friend Mr. Wollaston, whose judicious interference could not ward off a fatal termination.

A woman, 35 years of age, had become pregnant of her first child, and was taken with symptoms of labour at the eighth month. She was unmarried, and had taken several drastic purgatives to produce abortion at an early period of gestation, but not latterly. Prior to labour, she had frequently complained of severe pain on the right side of the abdomen, near the crista ili. After the labour had begun, the pains were inconsiderable, but continued at intervals for seven or eight days, when the membranes burst, and the liquor amnii escaped gradually, and continued doing so during the succeeding days; mixed with purulent secretion of a very offensive odour. The pains for nearly twenty-four hours had become nearly suspended; they were restored apparently by the use of the ergot of rye and nourishment, and progressively advanced the head into the pelvis. Again there was a complete remission of uterine power; the woman was greatly exhausted. On the 4th day after the membranes had ruptured, her medical attendant requested my assistance. I found the head of the child firmly fixed quite low in the pelvis; the os uteri was sufficiently dilated, but she had no expulsive pains whatever. I immediately applied the forceps, and delivered her without difficulty. The placenta soon followed quite entire, and the

uterus appeared well contracted, and very little hæmorrhage occurred. The child was dead, and seemed to have been so for perhaps three or four days. During the first twenty-four hours after her delivery she seemed doing well, was in tolerably good spirits, and expected soon to get well. On the second day she was restless, had some slight fever, and the abdomen became flatulent and swelled, yet *without any pain on pressure*. The milk was never secreted; the lochia were scanty, but mixed with a good deal of most foetid pus. The abdomen was carefully fomented, Dover's powder and antimony given, and her strength supported with beef-tea. In three or four days she seemed to mend, when she gradually sunk from exhaustion on the fourteenth day after her delivery. During the last ten days her pulse was extremely feeble. She complained of *no pain*, her tongue was slightly furred, and she had no acute symptoms of any kind.

Post-mortem examination.—The serous membrane of the intestines and the abdomen was *full*, and indicated no inflammation. A large ovarian cyst on the right side was the first thing to attract our attention: it contained about a quart of sero-purulent fluid; its inner membrane was very vascular and studded over with numerous depositions of lymph: the body of the right ovary was shrunk up; the uterus was flaccid; the mucous membrane intensely inflamed and disorganised; a collection of pus filled its cavity; the cervix and os uteri were black and almost sphacelated; the vagina also was greatly congested; the left ovary contained several small cysts, one of about the size of a bean, containing secretion similar to that of the large cyst of the right ovary.

REMARKS.—Connecting the symptoms accompanying the labour and the subsequent period with the appearances after death, there can be no doubt, I think, that the inflammation and supuration of the uterus accompanied the labour and preceded its termination. Suppuration exhausted the powers of the patient, and uterine contraction consequently ceased, and rendered her incompetent to give birth to the child, and the long period of labour—nearly four days—so aggravated the inflammation of the uterine cavity as to lead to a fatal result.

Mr. Wollaston thinks that the disease originated in the ovarian cyst, was propagated by the Fallopian tubes, and thus invaded the womb; but we cannot admit this explanation, for the cyst was a perfectly closed sac unconnected with the corresponding Fallopian tube. The protracted labour was sufficient to explain the lesions which were found, and which were quite serious enough to cause an abundant suppuration. Protracted labour brought on inflammation of the cyst, as it did that of the womb.

Thus the womb, when about to accomplish its important function, may displace a tumor which impedes the performance of that function. When it cannot do so, it frequently bursts the tumor, so as to evacuate its contents through the vagina, and to allow parturition to proceed in safety; and, when we consider what an enormous amount of pressure nature employs to produce delivery, we need not wonder that it often crushes in this manner an ovarian cyst. This occurred in a case related by Dr. Ashwell. The labour was obstructed by a tumor which burst into the vagina, and discharged a dark, offensive fluid. Delivery was then shortly effected, but death took place on the second day. On examination post-mortem, a large cyst was discovered growing from the left ovary, the vagina being ulcerated and gangrenous at the part where it communicated with the cyst (Guy's Hosp. Rep. No. 2). A case is also reported by Dr. Langley of an ovarian tumor which burst during labour, and was attended by the escape of several gallons of serous fluid (No. CXL. vol. vi. of Lond. Med. and Surg. Journ., Oct. 4, 1834, p. 319).

Mr. Brown has lately published a case which illustrates this termination of ovarian dropsy. It is as follows:—"I was called to see a lady pregnant with her second child. I found her ill and weak, complaining of the enormous size of her abdomen, and satisfied in her own mind that she should have twins. At the proper time labour came on, and the child was born without difficulty: but on placing my hand externally to grasp the uterus, I found I could not feel it, for the pelvic cavity was filled by a soft, elastic tumor, and the uterus had ascended out of the pelvic cavity, and was above this tumor. In introducing my hand into the vagina,

and endeavouring to reach the uterus to remove the placenta, and pressing my other hand over the uterus, I had the pleasure to feel the tumor, which I felt was an ovarian cyst, suddenly rupture, and discharge its clear amber coloured fluid down the side of my arm. On a subsequent confinement not a vestige of this tumor could be felt."

An ovarian cyst may produce scarcely any impediment to delivery, and yet the portion of the sac which has entered the septum may inflame, discharge its contents, and the patient recover. Thus Dr. Davis saw a tumor in the left iliac region, complicating but not impeding labour, produce dangerous symptoms, and burst in the vagina on the 27th day after delivery. The tumor disappeared, and the patient recovered. Whether the disease was ovarian or uterine could not be positively determined, but it was most likely the first.

The bursting of the cyst, and effusion of its contents into the peritoneal cavity, is a third termination of ovarian cysts, and we shall soon show that the accident is attended by much less danger than has been hitherto supposed, provided the contents be of a fluid and albuminous nature. Dr. Ingleby had noticed this termination during pregnancy. Mr. Headland (*Lancet*, June 22, 1844) relates a case, and Dr. Peddie (*Medical Times*, Feb. 22, 1844) mentions that a woman, in whom he clearly detected an ovarian tumor about the size of a child's head, became pregnant, and after delivery no trace of it could be found. It is therefore fair to conclude that it burst during delivery; and, as it did not re-appear, that it was effectually cured.

There is a fourth termination of an ovarian tumor—the expulsion of the whole tumor through the vagina. There is, as far as we are aware, but one recorded instance of this natural operation of extirpation of the ovary, and it has been published lately by Mr. Rankin, of Carlisle, in the *Edinburgh Monthly Journal* (July 1850).

Mrs. G——, 27 years of age, was the mother of five children. The first two labours were natural; but after a difficult labour, from an unascertained cause, she again became pregnant. A large tumor, occupying the cavity of the pelvis, interrupted the efforts of Nature at the next birth, and delivery with the

crotchet was with difficulty accomplished. The patient again became pregnant; and, as the tumor was now of greater size, delivery by the natural passage was considered impossible. Delivery was effected, however, by turning the child and forcing the tumor from its place in the pelvis upwards. Thirty-eight days subsequently a tumor was expelled from the body. It was a partially putrid mass, ten inches long, five broad, and weighing fourteen ounces, consisting of a dense, fibrous substance, fringed here and there with small cells, emptied of their contents. I examined the vagina (says Dr. Rankin), and detected an opening, small compared with the size of the diseased parts, which must have passed through it; yet, after contraction, it freely admitted two fingers. It was irregular in form; the parts around were indurated; and, with the aid of the speculum, they were found to be inflamed. The bulk from the tumor above the pelvis had entirely disappeared; but on pressure upon its former seat, and counter-pressure from within the vagina, there was still considerable fulness, which might arise from induration of the parts concerned in the recent separation and rejection of the morbid structure. If we listen to the testimony of Dr. Selkirk, of Carlisle, in whose practice the case occurred, and who followed it throughout, we must undoubtedly admit that the tumor was fibrous and ovarian:—

"I am convinced (says Dr. Selkirk—*Lancet*, Oct. 1850) for the following reasons—be the tumor ovarian or not—it was not intra-uterine. The tumor occupied, during the intervals of impregnation, the right iliac fossa, as was evident to the eye, and palpable to the touch; and, on internal examinations, it always pointed in the same direction. I remember well that, during the first stage of labour, in her confinement in 1846, which was protracted from what I at that time deemed an unusual thickening of the posterior portion of the cervix uteri, or the hand partially introduced in order to arrive at a more satisfactory knowledge of the presenting part, the fingers passed beyond the indurated point, and into the uterus. Again, at her labour in 1848, in which the perforator and crotchet became necessary, being obliged to remove the placenta by the hand, the uterus, gliding down by the margin of the tumor,

contracted ball-like, quite as low as usual, and to the left iliac fossa. Further, in her labour in February last, I again removed the placenta, the uterus contracting distinctly downwards by the margin of the tumor; and manipulation by the fingers externally detected the womb isolated from, and to the left side of the morbid growth. Now is it at all likely, admitting the tumor to be uterine, that the organ would contract so regularly, satisfactorily, and to its ordinary dimensions, around such a large, diseased mass? I opine not."

We have been careful in establishing the preceding facts, because the case is one of interest, and because it proves that we cannot as yet accord to the microscopical investigator, however eminent he may be, the right of deciding all pathological questions. The fibrous tumor was examined by Professor Goodsir, who said that it could be nothing else than a uterine fibrous tumor; and, influenced, no doubt, by so excellent an anatomist, a distinguished midwifery professor also considered the tumor to be uterine.

We have already shown that this cannot be admitted. Dr. Selkirk recognised its independent extra-uterine position at the labour previous to the one after which it was expelled. It was evidently torn from its pedicle by Mr. Rankin during the operation of turning; for we read (Edinburgh Monthly Journal, July 1850, p. 14):—"Very considerable force was necessary to bring the child into the pelvis. During the operation the tumor was gradually *forced up* from the cavity of the pelvis into that of the abdomen, and a sensation of *tearing was communicated to my hand, and rendered audible*, which led me to conclude that some fearful disaster was impending. This was soon found to be referable to the parts of the mother, for the child was born uninjured." The rent through which the tumor passed was seen by means of the speculum; and we may well admire the wonderful agency by which such a tumor was with impunity expelled from the peritoneal cavity of a patient thus almost miraculously cured of a usually fatal complaint; while the well-being of both mother and child shows that Mr. Rankin's conduct should be imitated in a similar case.

It appears that not only the vagina, but the uterus itself, has been perforated through the instrumentality of an ova-

rian tumor, which, by producing an obliquity of the uterus, effectually pushed the head of the child out of the axis of labour, bringing it to bear on the edge of the pelvis with sufficient force to cut through the walls of the uterus. Tumors of the fallopian tubes are less frequent than those of the ovary, so do they less frequently cause serious obstruction to parturition. We believe there is but one case on record when this occurred; it is given by Chambon de Boulage (Anciens Journal de Med.-Chir. et Phar.) The patient died, and the obstruction was found to have been caused by an irregularly-shaped, hard, and in part osseous, tumor. It is not surprising that this complication should be a frequent cause of mortality. Thus of the 31 cases given in Puchelt's valuable work

- 1 died without being delivered.
- 14 " soon after delivery.
- 3 " from other causes.
- 13 recovered.

—

31
Dr. Merriman states that out of his 18 cases

- 9 died.
- 3 recovered imperfectly.
- 6 " perfectly.

—

18
The danger in this complication of pregnancy to the children may be inferred from the fact, that out of Puchelt's 32 cases

Children.

- 21 died before delivery.
- 2 " after it.
- 7 born alive.
- 2 result not stated.

—

32
In Dr. Merriman's cases there were 16 still-born children.
4 born alive.

Even when not fatal to the child's life, the pressure of an ovarian tumor may produce a malformation of the skull in the fœtus, as in a case related by Mr. Langley (Lond. Med. Jour. vol. vi. 1844).

Effect of ovarian tumors on the bladder.—The bladder soon feels the pressure of an ovarian tumor, and it often affords the first indication of its slow

beginnings. It gives rise to a frequent want of passing water, which is the result of a nervous irritation, most distressing to the patient, long before the tumor has acquired a sufficient size to diminish the capacity of the bladder. At a later period of its growth, if the tumor cannot rise from the pelvic cavity, it may press on the urethra, and so impede the passage of the urine that the patient is obliged to bear forcibly backward in order to pass water. When the whole tumor is pelvic, or when a considerable portion of it is forced into the pelvis, it presses so forcibly on the urethra as to render impossible the flow of urine, and require catheterism. It has even necessitated the puncture of the cyst by the vagina, as in a case which occurred to Mr. Ogden (*LOND. MED. GAZ.* Jan. 1840).

When, however, the tumor completely, or even partially, leaves the pelvic cavity, all signs of vesical disturbance cease, and although the bladder may be displaced (as in a case related by Portal), or drawn up, pushed on one side, and diminished in size, still it, generally speaking, adapts itself with wonderful facility to the emergency. It is only in some cases, when a tumor is firmly lodged in the vesico-uterine cleft, that, as it increases, it presses against the bladder, and may thus divide its area into two cavities, both containing urine. This presents one of the most difficult problems of diagnosis; for when the attendant introduces the sound he only penetrates into the anterior chamber of the bladder, he only withdraws a small quantity of urine, which but little diminishes the size of the tumor, because the posterior chamber remains full. In another work we have given a case of this description wherein the urine is forcibly retained, and still continues to dribble out (*On Diseases of Menstruation and Ovarian Inflammation*, p. 30). In rare instances adhesions take place between the cyst and the bladder, and after a perforation of the walls of both cavities the contents of the cyst pass into the bladder, and may be thus ejected.

The passage of pus from an abscess into the bladder has been seen to bring on chronic cystitis by Emery, Louis (the elder), and Lisfranc, and Dr. H. Johnson (*Med.-Chir. Review*, 1836), saw it bring on frequent desire to pass water, dysuria, and hematuria. Generally,

however, the passage of pus into and through the bladder has no injurious effects. Cases of this description have been reported by Cæsar Hawkins, Dr. Gordon of Aberdeen; and abroad by Menière, Dancé, Martin, Madame Boivin, and in two instances by Marshal de Calvi. Out of these nine cases eight recovered. Besides, it is not pus but a fluid, which in general may be less irritating than pus, we say in general, for we have already mentioned that a distinguished surgeon of Brussels extracted from the bladder of a patient a stone having for its nucleus a tooth, which most probably came from an ovarian cyst. Although the following case occurred in the male, it so well exemplifies the innocuity of some of the contents of hydatid tumors when placed in contact with the urinary surface, and is so interesting for other reasons, that we shall give it as stated by Dr. Bright, in one of his valuable papers in the *Guy's Hospital Reports*.

W. Skingley, aged 54, was admitted into Guy's Hospital labouring under serous effusion, with dyspnoea and coagulable urine. He died, and on examining the body a tumor was found, about the size of a large orange, between the bladder and rectum, pushing the bladder forwards. The tumor was hydatid. It had given rise to no retention of urine, or at least none was noticed. The patient, who had been labouring under other disease, complained of the difficulty he had in retaining his water; and, when the examination was made, it appeared that the urine was continually passing away, and that a tumor bearing all the characteristics of a distended bladder presented itself at the pubic region. A catheter was introduced, and a few drops of perfectly healthy urine drawn off without producing any diminution in the bulk of the tumor, as it was still supposed that the urine was retained. More than one medical man attempted to draw it off, and at length the catheter became obstructed by the passing of some hydatids. A sucking-pump was applied to the catheter, and thus a considerable quantity of the "debris" of hydatids was removed. The patient died of his principal complaint, and a large hydatid tumor was found to fill the pelvic region, attached to the posterior part of the fundus of the bladder, pressing so much forward as to prevent the bladder from being filled with urine,

and causing its constant escape. The catheter had not passed through the bladder to reach the cyst, but, on the contrary, had passed by an opening from the urethra behind the bladder.

Dr. O. Heming relates a case in point in his translations of Mme Boivin and Dugés' work:—"The bladder being opened by ulceration, it allowed for a long time hair to pass with the urine; at last a body was abstracted from the bladder as large as a hen's egg, presenting at one of its extremities a shred of skin containing hair and bone, in which was a kind of tooth resembling a small molar. The communication of the cyst with the bladder was ascertained by the finger passed into the urethra. The person recovered."

The most remarkable instance of this favourable termination was published by my friend Dr. Bennett of Edinburgh (*Monthly Jour. of Med. Sci.* Feb. 1849), and deserves to be given in full.

Case of Spontaneous Cure of Ovarian Dropsy, by means of an Ulcerative Opening of the Cyst into the Bladder.

CASE.—Anne Pyper, a servant, aged 25, was admitted into the female clinical ward of the Royal Infirmary, Nov. 8, 1848. She had been delivered fourteen days previously of a male child in the Maternity Hospital; and, on inquiry, I found that the labour was a natural one, and presented nothing unusual. On the birth of the child, however, the abdomen still continued enlarged; and at first led to the suspicion that another fetus remained in the uterus. After a time the true nature of the case was rendered manifest, and a large swelling was detected, which was moveable to a certain extent, and presented all the characters of an encysted tumor of the left ovary.

When I first examined her in the Infirmary, I found the abdomen swollen to about the size of a woman's during the sixth or seventh month of pregnancy. The tumor extended from the epigastrium to the pubes, but bulged considerably towards the left side. Its surface was irregular; and two large nodules, each the size of a cocoa-nut, existed about its centre. It was tense and firm to the feel, somewhat elastic, but no fluctuation could be detected. The tumor was firmly fixed, and the seat of constant pain, especially in the

left lumbar region, which was increased by pressure, by lying on the right side or on assuming the erect posture. The urine was of a slight yellow colour, and presented its normal characters. The digestive, respiratory, circulatory, nervous, and integumentary organs appeared to be healthy. She had observed the tumor seven months before her delivery; and it has gone on gradually increasing, and been somewhat painful from the first. *Eight leeches were ordered to the most painful part of the abdomen.*

For four days the patient remained in the same condition, the local pain, however, having been relieved by the leeches. On Nov. 12th, my attention was directed to the urine, which now presented a copious white deposit, occupying two-fifths of the jar, while the supernatant portion was of a light amber colour, and unusually viscid. The deposit was determined by the microscope to consist of pus, mingled with a few compound granular corpuscles. The clear portion was strongly coagulable by heat and nitric acid.

At first I imagined that the cyst had burst into the vagina, but the patient and nurse assured me that there was no discharge between the intervals of micturition, and that all the fluid came from the bladder.

The urine presented the same characters during the next three days; the amount discharged during the twenty-four hours being about three pints. On the 15th, I observed that the tumor had somewhat diminished in size, its hardness and tensility had disappeared, and distinct fluctuation was perceptible in it. *A broad flannel roller was ordered to be firmly applied round the abdomen, and compression made by means of pasteboard, previously soaked and modelled to the abdominal surface.*

From this time the abdomen rapidly diminished in volume, while the amount of purulent viscid fluid discharged from the bladder varied from three to five pints in the twenty-four hours. The appetite and general health continued good; and she was ordered nutritious diet, with four ounces of wine daily. On the 23rd, the amount of pus contained in the urine was greatly lessened, and the clear portion presented a slight haziness on the addition of nitric acid. On the 27th the abdomen had regained its natural size, although a dense mass, evidently the collapsed ovarian sac,

could readily be distinguished, occupying the left iliac and hypochondriac regions. The urine now also was natural in quantity, and presented only a slight sediment, consisting, as shown by the microscope, of some crystals of oxalate of lime, and a few pus globules.

From this period she may be said to have recovered. She suffered occasionally from uneasy feelings on the left side, sometimes amounting to pain, which were relieved by the application of four leeches, followed by a small blister. One of the leech bites ulcerated superficially, but soon healed up. She was dismissed on the 18th of December, expressing herself as being well in every respect, having been sitting up and running about the ward for the fortnight previous. The indurated mass in the left iliac region was greatly diminished in size, but still very perceptible to the feel, though not to the eye."

Effects of Ovarian Tumors on the Kidneys.—The kidneys are very rarely implicated. Burns is the only author, with whom we are acquainted, who mentions their partial atrophy as the result of the pressure of an ovarian tumor, and we have never met with a similar appearance. It is not unreasonable to suppose, with the same author, that when the tumor occupies the entire abdominal cavity, suppression of urine may arise from the pressure of the cyst upon the kidneys: diuretics fail to increase the quantity of urine, though this may be effected by paracentesis. This assertion is well exemplified by a case given by Burns, of Madame de Rosney, "who in the space of four years was tapped twenty-eight times." For seven days after each puncture she made water freely, and in sufficient quantity; the appetite was good, and all the functions well performed; but in proportion as the tumor increased the urine, in spite of diuretics, diminished, and at last came only by drops.

This is the place to mention an interesting case, of a large monocular ovarian cyst, which we saw in consultation with Messrs. Wetherfield and Duncan, and in which the fatal termination was no doubt accelerated by extensive disease of the kidneys. Several weeks before the patient's death the urine was abundant, purulent, and very offensive, and it was a question with us whether there was not some com-

munication established between the bladder and the cyst, but on opening the body the cyst was found to be entire, without any trace of inflammation, containing five or six pints of albuminous fluid of a straw colour, while the right kidney was three times its natural size, and in a state of decomposition. The left kidney was tolerably healthy, and the bladder and urethra perfectly so. If this disease of the kidney was not caused by this large ovarian cyst, it no doubt increased it.

[To be continued.]

DECADENCE OF THE PROFESSION IN THE UNITED STATES.

It is most cheerfully conceded that in those schools which, twenty years ago, sent out their three or four hundred graduates annually, the graduates of every subsequent year have progressively improved, partly in consequence of their previous education being better, and partly because the medical courses have been perfected. But what shall we say of a large portion of the six or eight hundred annual graduates who are now poured out by more recently established institutions? We are persuaded that in average attainments they do not reach the standard of graduates of twenty years since; and, what is worse, that after entering the profession they are much more apt to remain stationary. For while in the olden time the student was at least impressed with the dignity, excellence, and sacredness of his calling, the class of modern students to which we have alluded become physicians with such motives as ought to lead them to be blacksmiths or carpenters, or members of some fraternity in which they might be really useful to mankind. But they take up physic as a trade, they are taught by traders in diplomas, and they practise in the true spirit of tradesmen. And as for improvement, they vote all discoveries a humbug, and study a bore. Yet these persons, physicians in name, and by virtue of a parchment title, claim and assert an equality of rank with the well-educated, the high-minded, the studious, and progressive members of the profession. They form a part of the same body politic, and by their character and conduct determine its estimation in the world. Twenty years ago nearly all graduates of medical schools were sure to possess a respectable degree of knowledge and skill; but now, although the graduates of a certain number of schools have higher attainments than ever, the remainder occupy the very lowest point in the scale, which begins at the zero of ignorance and ends at the climax of wisdom.—*American Journal of the Medical Sciences.*

CASE OF
REDUCTION EN MASSE.

By J. WEIR D. BROWN, M.R.C.S.E.
Demonstrator of Anatomy in the School of the
London Hospital.

I FORWARD for publication in your valuable journal the following interesting case:—

Lewis Lyon, a broker, aged 72 years, was admitted into the London Hospital on the 3rd day of October, 1850, under the care of Mr. Luke. The patient stated that he had, during the last seventeen years, occasionally suffered much from a reducible inguinal hernia, which, whenever the truss that he wore was for a short time removed, came down. On those occasions he said that he had always succeeded in returning the hernia himself, but that on the 29th ult., having taken off his truss, the hernia descended and speedily became larger and more painful than usual. He at this time grasped the swelling with both of his hands, pushing it violently upwards, and in this way succeeded in returning the greater part of it into the abdomen; but, from the force he used, his fingers, he said, became benumbed, and that he was on that account obliged to desist, and to send for a surgeon, who completed the reduction. The pain in the belly, however, soon after this became more severe, and afterwards, during that day, he had two scanty stools. The pain during the succeeding night increased in severity, and early on the following morning retching and vomiting took place, which, he said, had been constant ever since.

On admission there was a tumor occupying the right inguinal canal, which receded into the abdomen when the slightest pressure was applied to it with the finger, and the surface became like that on the opposite side; the tumor again appeared on coughing. It was observed that the ring was large, and readily admitted the finger, with the fold of skin which was pushed before it. The spermatic cord was clear, and unobscured by any overlying sac. When reduced within the abdomen, there could be felt a fulness above the inner ring, attended by pain on pressure. Thinking it probable from these circum-

stances that the hernia had been reduced with its contents still in a state of strangulation, Mr. Luke, without delay, determined to perform an operation of exploration.

The patient being placed on the table, Mr. Luke made an incision over the inguinal canal by pinching up and transfixing with a straight bistoury a fold of integuments, after which the parts intervening between the integuments and the tumor which had reappeared were carefully divided, and the sac opened, when its contents were brought into view. They were found to consist of discoloured intestine, lying nearly wholly within the parietes of the abdomen, and encircled by a stricture formed by the neck of the sac, the stricture being about one and a half or two inches distant from the internal ring. The sac was drawn partly from the abdomen for the purpose of facilitating the division of the stricture. When the stricture was divided, the hernial contents were replaced within the general peritoneal cavity, and the wound closed with sutures, &c.

Eight hours after the operation the patient had four copious stools; she then fell asleep, and passed a good night.

Oct. 4th.—The patient, this morning, is in rather a low state. He has had no vomiting since the operation, and says that he is free from pain. Ordered a pint of beef-tea, and eight ounces of wine during the day.

5th.—The dressings have to-day been taken off and the sutures removed. He complains of heat and pain, which, he says, extends from the wound to the bottom of the scrotum, and those parts are red and inflamed. The bowels have not been opened since the day before yesterday.—R. Ol. Ricini, \mathfrak{ss} . statim sumend' and an evaporating lotion to be applied to the part.

6th.—The patient appears to be weaker this morning. The redness, heat, and pain in the scrotum, &c., are greater than yesterday. Bowels have not yet been opened. Another half ounce of castor oil to be immediately given. The inflamed parts to be covered with collodion, and two pints of porter to be taken during the day; the wine also to be continued.

7th.—The patient has passed a restless night on account of pain about the

scrotum and spermatic cord. Bowels have been opened by the castor oil. Colloction to be reapplied to the inflamed parts.

Vespere, 9 o'clock.—Nearly as in the morning; restless, and unable to sleep.—*℞* Aqua Pur. 3j.; Tr. Opii, ℥xx. Fiat haustus statim sumend.

8th.—Nearly as yesterday. The wine to be increased to twelve ounces, and the anodyne draught to be repeated at bed-time.

9th.—The pain in the parts more severe, and the swelling of the scrotum greater. Patient very low. Pulse slow and weak; stomach irritable; occasional slight singultus.—*℞* Infus. Gentian. Co., 3iss., Ammon. Carb. gr. v. Fiat haustus ter in die sumend. *℞* Morphiae Acet. gr. ss.; Confect. Aromat. gr. iv. Fiat pil. hora somni sumend.

10th.—A fluctuation being this morning perceptible, a puncture was made with a lancet, and a considerable quantity of matter removed. Bowels open.—Infus. Gentian. ut antea.

11th.—As yesterday.

12th.—Much better in every respect. Abscess discharging freely. Pulse stronger; appetite returning: a mutton chop to be taken for dinner.

13th.—As yesterday. Bowels confined.—*Ol. Ricini*, 3j.

18th.—The patient has been rapidly improving since last report. The swelling of the scrotum has diminished, and the wound in the groin is now entirely healed, and slight discharge still continues from the puncture that was made in the scrotum.

25th.—Doing well.

30th.—The patient since last report has been out of bed for three or four hours every day, and is now quite convalescent. The wine to be continued, with nourishing diet.

Nov. 11th.—Discharged cured.

REMARKS.—With reference to the above case, it may be observed, that the result bears evidence of the value of the means of diagnosis adopted in it, and of the truthfulness of the observations which Mr. Luke has made in his paper upon this subject, published in the xxvi. volume of the Transactions of the Royal Medical and Chirurgical Society. It appears to be important that these means of diagnosis should be well understood; because, upon their admitted

accuracy, depends the promptitude with which surgeons may be disposed to have recourse to early relief; and it is to be regretted that so few cases of this description, relieved by operation, have been published, as there is reason to believe that such cases are far more common than has generally been acknowledged by authorities.

I learn that the above is the ninth case which has come under Mr. Luke's observation, and the sixth on which he has operated, the results being, that only two patients died,—one from lesion of the intestine, mortification having taken place previous to the operation, and the other seven days after the operation, from erysipelas attacking the wound,—the symptoms of obstruction having entirely ceased, and the bowels having recovered their perfect function. If so many cases of this kind have occurred in the practice of one surgeon, it is but fair to conclude that they may have taken place in a somewhat similar proportion in the practice of other surgeons, and yet we do not hear of their having been relieved by operation. Is, then, the inference here drawn respecting the frequency of such cases, correct, or is it not? If it be correct, and operations have not been performed on account of the obscurity by which such cases are surrounded, the publication of the above, may, in future, be useful in assisting the diagnosis, and in keeping attention alive to the subject of strangulated hernia reduced *en masse*.

London Hospital, Nov. 25th, 1850.

THE TREATMENT OF INTERMITTENT FEVER
BY FERROCYANIDE OF POTASSIUM AND
UREA.

[We substitute the following for a paragraph on the same subject which appeared at page 1014 of our last number.]

M. Lemaître, of the Hôpital La Charité, has employed a mixture of ferrocyanide of potassium and urea in the treatment of five cases of intermittent fever. In two of these it was successfully used, and in two unsuccessfully.—*Journal de Chimie Médicale*.

* * * There is no reason to suppose that it can be used as a substitute for quinine with any prospect of success.

MEDICAL GAZETTE.

FRIDAY, DECEMBER 20, 1850.

AN accurate statistical return of the comparative mortality in the Metropolitan and Provincial Hospitals would throw great light on the results of practice, and be attended with great benefit to medical science. The comparison might not merely extend to the relative mortality in the different institutions of this country, but to the proportionate number of deaths in these, and in the great hospitals of Paris, as well as of other continental cities. At present, however, we have no accurate data for such calculations; and great care is required in instituting comparisons, because by the assumption of an incorrect basis, an undue prejudice may be created against the medical superintendence of certain institutions. The plan adopted by the Registrar-General, of publishing a weekly return of deaths in the different metropolitan hospitals, is of no statistical value whatever, unless this be connected with a quarterly or yearly summary of the number of patients actually admitted into each institution.

With a laudable desire to arrive at some sort of conclusion on this important subject, Dr. Webster, in a pamphlet now before us, takes the number of deaths in hospitals for six months, as returned by the Registrar-General, and compares these deaths with the number of *beds* which each hospital is supposed to contain.* The conclusions at which he has arrived are as follows:

"1st. One death in every 21.75 of the total mortality throughout London, or 4.60 per cent., took place in the eleven general hospitals during the last six

months. 2d. One patient died for nearly every three beds, speaking in the aggregate. 3d. The highest rate of mortality generally occurred in the smallest hospitals. 4th. With one exception, the lowest comparative ratio of deaths was observed in the large or more populous institutions. 5th. The fewest deaths, in reference to the published number of beds, was reported from the Free Hospital; and 6th. The largest comparative mortality, according to the same calculation, occurred at University College Hospital."

The table from which these deductions have been made is elsewhere printed*. The general result from eleven hospitals is that there are 3228 beds, and there were in six months 1051 deaths, making 1 to $3\frac{1}{4}$ beds. The mortality at UNIVERSITY COLLEGE thus appears to be *one death to every* $1\frac{1}{2}$ bed, while at the FREE HOSPITAL there is only *one death to every* $6\frac{1}{2}$ beds!

The author appears to be aware that this mode of calculating deaths in hospitals may lead to fallacy; but, as we infer from his observation, he does not perceive the entire unsoundness of such a mode of computation. In remarking on the numerical results, he observes:

"On the other hand, when comparing one hospital with another, it is curious to observe that the mortality was nearly the same at St. Bartholomew's and Guy's, the two largest institutions of the kind, and having each the same number of beds—viz. 580. Again, at St. George's and the London hospitals, each having 320 beds, one being situated at the west end of London, the other at the east, where an immense number of serious accidents constantly occur, and certainly very many more than in the neighbourhood of Piccadilly, there is not much difference in the rate of mortality. This is, however, in favour of the latter hospital, although still inferior to the Middlesex, St. Bartholomew's, and Guy's, but especially to St. Thomas's Hospital, where the ratio of mortality was less than at any other similar institution, with the sole exception of the Free Hospital, as previously stated. 1 and

* On the Health of London during the six months terminating September 28th, 1850. Read before the Medical Society of London, and reprinted from the London Journal of Medicine.

fully aware that these calculations must not be taken as absolutely free from all fallacy, since the admissions of patients may be more numerous at one establishment than at another. But I think that this argument, if it has any force, would rather militate against the smaller and more modern hospitals, to which letters of recommendation from a governor, unless for accidents, are often required to ensure the reception of an in-patient; while at the two ancient Royal Hospitals of the city, and at Guy's, where the rate of mortality has been found to be lower, the facility of admission is greater, and the applications, consequently, likely to be more numerous. The subject here mooted is both so important and instructive that it deserves further inquiry: indeed, it is solely with that object I have now made these imperfect remarks, and based them upon the numbers contained in the average table, which, I believe, is the first ever compiled in illustration of the results obtained at the eleven general hospitals of London, viewed comparatively."

It is, we think, obvious that the table collected by Dr. Webster is merely calculated to show the mortality of beds, and not of patients; and that, as the relative number of admissions of patients within a given period of time is not even hinted at, it is impossible to make any use of these figures in computing the comparative mortality, even admitting that the number of beds assigned to each of the eleven hospitals is correct, and that the beds are always occupied.* The number of beds occupied in an hospital is very well known to be open to great fluctuation; the same bed may be occupied at one hospital by three or four different patients, and at another hospital by only one patient during the same statistical period. Thus this table, which would have the effect of causing all claimants for medical and surgical assistance to rush to the Free Hospital, and fly from University College Hospital as from a Pest

* It has been computed that in the large hospitals each bed, on an average, receives from nine to ten occupants per annum.

House, errs in taking no account of these material elements of the inquiry. If it happened that during the period of six months only one-half of the Free Hospital beds were occupied by patients whose cases were of a protracted kind, and required lengthened treatment, while the inmates of University College Hospital were frequently renewed, the deaths in the latter, arising perhaps from the greater number of patients, and the severity of the cases received, would appear very high if compared with the fixtures of the hospital—namely, the beds; while compared with the numbers actually admitted and treated, the mortality might be no greater than at the Free Hospital. In short, the comparison of deaths with beds, can show no more than the deaths compared with wards; nor is there any method of correcting so defective a basis of calculation, by any subsequent allowance for the situations of hospitals, or the class of persons who apply for relief. The only part of Dr. Webster's table which can be received as approximately correct is the column of deaths, which, as it has been compiled from the Registrar's return, may be taken to represent the Hospital mortality of London for six months, so far as the Registrar can procure information on the subject. If we double these numbers, we obtain the annual mortality in the eleven Hospitals of London, as under. The figures, however, are open to correction, by taking a period of several years:—

Hospitals.	Annual Deaths.
St. Bartholomew's	360
Guy's	328
London	240
St. Thomas's	230
St. George's	212
Middlesex	172
Westminster	158
University College	158
King's College	132
Charing Cross	72
Free Hospital	44

In order to arrive at anything like a

fair comparison of these numerical results, it is of course necessary to show, not only the number of patients admitted into each institution, but also the class of applicants,—whether labouring under severe accidents, or acute or chronic diseases, or whether the cases be of a kind in which destitution and disease are equally combined, and the latter not of a serious nature. A hospital situated in the vicinity of the places where accidents frequently occur may have a higher rate of mortality than one in the heart of the metropolis merely as a result of its local position. Information on these last-mentioned points it is very difficult to procure, but the first postulate—namely, the annual number of admissions, may be in some cases obtained; and we agree with

Dr. Webster in thinking that it is the duty of those who are connected with these large institutions to publish a yearly summary of cases admitted, the number of deaths, and other particulars throwing light on the treatment of disease. To a certain extent this has been carried out for many years by the Governors of the London Hospital, and in a less perfect manner by those of St. Bartholomew's and St. Thomas's.

In Paris a quarterly return of the *admissions and deaths* of patients in the public hospitals is regularly made. We subjoin the Parisian table for the second quarter of 1850. It may not only serve for guidance in these statistical inquiries, but it will hereafter enable us to compare the mortality of the London with that of the Parisian hospitals.

Deaths in the Hospitals of Paris, exclusive of Infirmeries (Hospices), during the Second Quarter of 1850.

Months.	In-Patients on the 1st of the Month.	Patients admitted during the Month.	Total In-Patients.	Patients discharged during the Month.	Deaths.
April	6034	7328	13362	6752	596
May	6014	7129	13143	6483	661
June	6009	7174	13183	6800	559
Totals during the Quarter . . . }	18057	21631	39688	20035	1806

If we deduct the number of patients discharged during the quarter from the total of those admitted and already in the hospitals, it will be perceived that there were during the quarter 19,053 inmates and 1,806 deaths; thus giving a mortality of 10·8 per cent.

According to Dr. Webster's table, after King's and University College, the London and Westminster Hospitals occupy a most unfavourable position as to the rate of mortality: in fact, taking round numbers, the patients who die in these institutions are equal to half the number of beds contained in them—i. e. for every two beds and a fraction one death must be reckoned! It would also

appear that the deaths in the London are comparatively twice as numerous as those in St. Thomas's Hospital, and that the mortality at St. George's is rather less than in the London Hospital. We have procured some authentic documents which will tend to show the inaccuracy of the results to which this faulty mode of computation must necessarily lead. The number of beds in each hospital is taken from Dr. Webster's table, with the necessary correction, that at the London they have amounted, since 1847, to 400, the number actually occupied by patients varying at different periods from 396 to 253.

Hospitals.	No. of Beds.	Total In-Patients.	Deaths.	Deaths per Cent.	Accidents.	Deaths from Accident.	Deaths from Accident per Cent.
1844.							
St. George's . . .	320	3284	293	9	1120		
London . . .	320	3691	224	6	2140	79	3½
1847.							
St. Bartholomew's	580	5801	428	7½			
St. Thomas's . . .	487	3709	283	7½			
London . . .	400	4159	276	6½	2396	73	3
1848.							
St. Bartholomew's	580	5826	480	8½			
St. Thomas's . . .	487	4340	276	6½			
London . . .	400	4185	264	6½	2442	80	3½

From these returns it will be perceived that, when the deaths are compared with the in-patients actually received, and not with the beds, the comparative mortality of the London Hospital does not reach seven per cent., and at least three per cent. of the deaths arise from accidents! In these periods, which are not selected, but taken merely because authentic comparative returns are procurable for the years mentioned, the relative mortality of St. George's, St. Bartholomew's, and St. Thomas's, invariably exceeded that of the London.

The mortality at the London Hospital has of late years undergone a very considerable reduction. Since the year 1846 the per centage of deaths from accidents, has been equal to nearly one-half of the per centage from all other causes. The table subjoined, the accuracy of which may be relied on, throws an interesting light on this question.

As a general average of the returns for the fifteen years, it may be stated that there were for each year 3471 in-patients; and, including the cholera year (1849), 292 deaths; thus giving a mortality of 8·1 per cent. If we divide the maximum number of beds occupied at any one time during this period (396) by the average yearly deaths, we shall find that the deaths are to the beds in the alarming proportion of 1 to 1·4—i. e., three times as numerous

as at St. Thomas's Hospital, according to the table published by Dr. Webster. But when we compare the yearly deaths with the yearly *occupants of the beds*, the deaths at the London are fewer by 1·7th, or 14 per cent.!

Return of the Number of Patients admitted annually into the Wards of the London Hospital, from the year 1835 to 1849, both inclusive, and also the number of deaths in each year.

Date.	No. of patients.	Deaths.	Per cent.	Per centage of deaths from accident.
1835	2785	277	10	
1836	2815	309	10	
1837	2961	418	14½	
1838	2987	371	12½	
1839	3247	305	9½	
1840	3389	296	9	
1841	3308	331	10	
1842	3300	260	8	
1843	3530	244	7	
1844	3961	224	6	
1845	3625	228	6½	
1846	4092	230	7	4½ p. cent.
1847	4159	276	6½	3 "
1848	4185	263	6½	3½ "
1849	4090	298	7½	4 "

We shall shortly return to this subject, and consider the comparative mortality at the other London hospitals.

Reviews.

The Races of Man; and their Geographical Distribution. By CHARLES PICKERING, M.D., Member of the United States Exploring Expedition. A new Edition, to which is prefixed, *An Analytical Synopsis of the Natural History of Man*, by JOHN CHARLES HALL, M.D., &c. 8vo. pp. 345. London: H. G. Bohn. 1850.

THE objects of a medical journal scarcely permit an extensive notice of this very interesting work, and we therefore limit ourselves to a brief analysis of its contents.

Dr. Pickering, after a close observation of the varieties of men in different parts of the world, visited by the Exploring Expedition, arrives at the conclusion that eleven races may be distinguished in the countries visited by him, and that these may be arranged in four orders, according to the colour of their complexion; thus,—White, Brown, Blackish-brown, and Black. Of these, the first embraces Arabians and Abyssinians; the second, Mongolians, Hottentots, and Malays; the third, Papuans, Negrillos, Indians, and Ethiopians; the fourth, Australians, and Negroes. The distribution of these varieties over the world is represented by a coloured map, after Dr. Prichard's plan. These several tribes are mentioned in the order in which the countries they inhabit were visited; and a short history of the characters, habits, &c., of each tribe is given.

Several allied subjects are also treated of: such as the relations of races, the geographical progress of knowledge, migration by sea and land, the origin of agriculture, zoological deductions, distribution of animals and plants, and lastly, the introduced animals and plants of Egypt.

It is needless to say more respecting this volume, than that it contains a large amount of valuable information, with respect to aboriginal tribes, collected on the spot by a careful and skilful investigator; and that it forms, therefore, a most important contribution to ethnology.

Dr. Hall's synopsis presents a brief but clear exposition of the *Natural History of Man*, without which the

general reader would not be able fully to appreciate the value of Dr. Pickering's work. The present state of the question as to the origin of man from one or more pairs of parents, is ably and lucidly stated by Dr. Hall.

We must do this gentleman the justice to say that he has rendered the work as interesting and instructive to the professional as to the general reader.

The Commercial Hand-Book of Chemical Analysis, &c. By A. NORMANDY. 8vo. pp. 640, with illustrations. London: Knight, Foster Lane. 1850.

DR. NORMANDY has here produced a very useful manual for the guidance of the chemist in detecting the numerous adulterations to which articles of food, commerce, and manufactures, are subjected. A book of this kind has been long wanted, and it is surprising, considering the demands now made upon science by merchants and manufacturers, that such a treatise has not hitherto been published. The small manual of Accum was a very imperfect production at its first appearance, and it is now quite obsolete.

Dr. Normandy arranges the various substances treated in his book in alphabetical order, thus greatly facilitating reference. The list of substances is very copious, and comprises, as far as we can judge, all that are likely to give rise to the necessity for chemical examination. Tables of analysis of the strength of different products and appropriate illustrations are offered to those articles which require them.

From our perusal of various sections of the work relating to the adulterations of food, we consider that this book will prove a most useful companion to the chemical student.

Influence of Physical Agents on the Development of the Tadpole, of the Triton, and the Frog. By JOHN HIGGINSBOTTOM, Hon. Fellow of the Royal College of Surgeons of England. Reprinted from the *Philosophical Transactions*, part ii. for 1850. 4to pp. 8, with a plate. London: R and J. Taylor. 1850.

A series of experiments is here related, by which the absence of lig

retarding the development of the tadpole, while temperature exerts an important influence on its growth. The plate which accompanies this essay presents, by a simple and easy diagram, the respective influences of these two physical agents. This contribution to natural history and physiology will be very acceptable to the students of these sciences.

Proceedings of Societies.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, Dec. 10, 1850.

THE PRESIDENT in the Chair.

Case illustrating the Difficulties of Diagnosis of Morbid Growths from the Upper Jaw; with Remarks. By PRESCOTT HEWETT, Assistant-Surgeon to St. George's Hospital, &c.

THE patient, a man *æt.* 25, was admitted into St. George's Hospital under the care of Mr. P. Hewett, in May 1848, with a large tumor, of an irregular shape, occupying various regions of the left side of the face. Presenting every appearance of having originated in the antrum, this tumor was found in the front and back part of the cheek, in the temporal fossa, in the orbit, and in the nostril extending to the back part of the pharynx; round in shape, but lobulated, it was firm and elastic to the touch, perfectly moveable, and in the nostril of a dead white colour and glistening appearance. The skin, conjunctiva, and mucous membrane of the nose were quite healthy, and no enlarged glands could be detected in any part.

The history of the case was, that six years previous to his admission into the hospital the patient was troubled with a disease, supposed to be a polypus of the nose, which had been easily removed with the forceps; subsequently, however, the cheek began to swell, and the tumors gradually made their appearance in the regions in which they were found. All this had occurred without any pain, and with very little inconvenience. A year ago caustic had been extensively applied in two different places, large cicatrices marking the spots: this treatment had produced no effect on the disease, and no fungating growths followed the application. At different times there had been extensive bleed-

ings from the nose, which had somewhat reduced the patient.

At a consultation of the surgeons of the hospital, it having been resolved that in all probability the disease was of the fibrous kind, and connected with the antrum, the removal of the upper jaw was decided upon. Dr. Snow, to whom the surgeons of St. George's are so much indebted for the able manner in which he, for a long time, administered chloroform at the hospital, having kindly undertaken to give it on this occasion.

The patient being seated in a chair, the operation was performed in the usual manner; but on removing the superior maxillary and malar bones, it was discovered that the disease was not connected with the upper jaw; it was altogether behind it. The larger portion of the tumor was dissected from off the pterygoid process, to which it was firmly attached. Those portions which were in the orbit and temporal fossa were removed without difficulty, being for the greater part simply connected with some very loose cellular tissue. The patient having become faint, was placed in the horizontal posture, and a small quantity of stimulant administered, after which he soon rallied. The portion of diseased structure in the back of the nostril was then removed with a strong pair of curved scissors. The pulse having again failed, the patient was at once laid on a bed and carried into an adjoining room: different restorative means were made use of, and he appeared to rally somewhat; but shortly afterwards, as the breathing became embarrassed, an opening was at once made into the crico-thyroid membrane; and, as a last resource, an attempt was made to carry on artificial respiration with a tube, but every effort proved of no avail; the patient soon died.

But few vessels were met with during the operation, and no great amount of blood was lost. Little or no bleeding followed the incision in the neck.

A careful examination of the bones removed during the operation, shewed that in the superior maxillary the antrum was all but obliterated, the posterior wall of the sinus having been forced, by the tumor lying behind it, against the anterior one; there was merely a chink left, the cavity of which was quite free, and lined by healthy mucous membrane; the malar was much more curved than natural. The structure of both bones was perfectly healthy. The tumors were of a purely fibrous character. At the dissection of the body it was made out that the tumor had originated in the roof of the left nostril; its main point of attachment having been to the under part of the body of the sphenoid, and inner

surface of the pterygoid process. Portions of diseased structure were still found in the sphenoidal sinuses, as well as at the upper and back part of the septum nasi. Some loose bits were also found deep in the temporal fossa, and at the back of the orbit; these were lying in the cellular tissue; they were all connected to each other by slender pedicles, one of which passed through a hole in the perpendicular portion of the palate bone; that in the orbit had reached this situation by creeping through the spheno-maxillary fissure. The bones were throughout healthy in structure. The tissue of the growth was purely fibrous.

The trachea and bronchial tubes, even to their minute ramifications, contained a quantity of frothy blood; the structure of the lungs was crepitant throughout, but each section presented numerous small dark spots of ecchymosis produced by some of the air-cells having been also filled with blood; these organs were otherwise free from disease. The heart was healthy; its cavities contained small black clots, but the greater part of the blood was thin and fluid, and did not coagulate on exposure to air. The other viscera were quite healthy.

In his remarks, Mr. Prescott Hewett principally drew the attention of the Society to the great difficulties which, at times, were found to exist as to the correct diagnosis of the precise region in which a tumor of the upper jaw had originated. Of these difficulties the present case afforded a good illustration. The history of the patient, and the various regions in which the tumor existed, had led to the conclusion that the disease, having sprung from the antrum, had gradually burst through the walls of this cavity, and thence spread to the spots where it was found. The operation and the subsequent dissection proved, however, that the antrum had not been the starting point of the disease. Mr. P. Hewett had little or no doubt that the morbid growth had first begun in the nostril, and had subsequently reached the pterygo-maxillary fossa, either by making its way through the spheno-palatine foramen, or by breaking down a portion of the palate bone. Once in the fossa, the subsequent progress of the tumor may easily be traced; it passed into the orbit through the spheno-maxillary fissure, and, in the face, it had in some parts made the bones yield, and, in others, it had so completely moulded itself to their shape, creeping over their cutaneous surface, that the outlines of the bones were scarcely discernible. Mr. P. Hewett's remarks were altogether confined to tumors of a fibrous character.

The morbid appearances observed about the lungs led Mr. P. Hewett to ask the question, whether the administration of

chloroform was advisable in operations about the mouth where there was likely to be a certain amount of bleeding. He had no doubt that the blood found in the lungs had got there by passing through the glottis; and he doubted very much if such would have been the case had no chloroform been used. Many surgeons, fearing this accident, had of late not failed to condemn altogether the use of chloroform in these cases; but some, being unwilling to submit their patients to such serious operations without it, had adopted a middle course, administering this agent in the first steps of the operation only, hoping thus to avoid all risk. It remained still to be proved, however, whether, even with this precaution, there might not be danger in using anaesthetics in some operations about the mouth.

Mr. FERGUSON said that, in common with every member of the Society, he considered this paper most interesting, as illustrating the differences of danger in those cases; but it was clear the tumor turned out different from what was supposed. At first it was said to be situated in the superior maxillary bone, but afterwards it proved to be only in connection with it; and there could be but little doubt of its being situated behind the antrum. No allusion was made to the shape and form of the superior maxillary; and there being no distortion of the alveoli or nostril, would lead to the opinion that the tumor was in the antrum, a matter of great importance where there was no alteration in front: it was evidently deep-seated, from the expansion of the zygomatic arch. In conclusion, he would not let the opportunity pass without giving an opinion respecting the use of chloroform in such cases. From numerous cases of protracted operations about the mouth, in which he applied it to both mouth and nostrils, and renewed it when its effects passed off, he considered he never had seen any case deterring from its use. At first he supposed such operations as the one described those in which it would not be applicable, and in a similar case at first did not try it, but soon discovered from using it in operations on both the jaws as well as mouth, where blood would flow back into the trachea, that no evil result had arisen: he wished to ask Mr. Hewett how long the operation lasted, as it might be tedious even in the hands of the most experienced, and we should weigh well all the circumstances which may cause death in such cases.

Mr. HEWETT did not exactly understand Mr. Ferguson's question, but he thought out of their places

minutes; but from fainting of the patient could not say exactly, but imagined about twenty minutes altogether.

Dr. WEBSTER asked whether chloroform was the cause of death, as in all cases where death so occurred the blood was found fluid; and in France it was stated to be black, and that air was found in the heart and veins.

Mr. HEWETT replied that the blood was fluid and dark coloured, and that if chloroform caused death in this case it was by suffocation.

Dr. SNOW said he was sure the blood did not get in during the operation, from the patient experiencing no difficulty of breathing, and that he had since administered chloroform in five or six cases: one for tumor of superior and one of inferior maxilla; also in other cases of similar difficulties, as polypus, &c.; but that there was no better test of its applicability than in operations for hare-lip. He had used it in twenty such cases, and had seen as much appearance of suffocation and stuffing when it had not been used. In this case there was not enough blood found in the lungs to cause suffocation: he thought the blood got in after the operation, while the patient was lying in a state of collapse, as there was oozing of blood up to the time of his death. From experiments on animals he found, as in the case of a cat held for two minutes under water, that none was drawn in until the animal was almost dying; and in a kitten killed by chloroform, the trachea was not stained. In asphyxia from drowning the lungs do not fill with water. In animals killed by chloroform the blood was found semi-coagulated; and he thought the fluidity in this case was attributable to the artificial respiration. The glottis retains sensibility as long as the patient has sense to breathe, and he believed the opinions respecting the use of chloroform were as much in favour of as against it.

Mr. H. C. JOHNSON stated that three grounds of death had been raised unconnected with chloroform:—1st. The length of the operation; 2dly. The inhalation of blood during syncope; and 3dly. The operation for artificial respiration. He said that the operation was not longer than usual, in fact the first step quicker than customary in operations about the face. He confessed the syncope was very severe; but although he had seen many such cases, he had never witnessed an instance of blood being received into the trachea. And in answer to the third, it was performed when the patient was almost moribund, and he did not consider that any blood escaped. From the result of this case he would abandon the use of chloroform in all similar ones; he was of opinion that death

was caused by the inhalation of a quantity of blood.

Mr. BARLOW was willing to admit Dr. SNOW's care in the administration of the chloroform, but there was no way so easy to account for death as its influence; and although it was gratifying to know Mr. Fergusson's success, he considered no agent diminishes muscular irritability like it; and although in five cases it may not have this effect, yet it may in the sixth. A frog under its influence cannot be so easily galvanized as one that is not; and he believed Mr. Hewett correct in stating, that if the glottis had been sufficiently active, this sudden result would not have occurred.

Dr. MARSHALL HALL divided the effects of chloroform into three stages: in the first of which voluntary motion is diminished; the second, in which respiration fails; and the third, in which circulation fails;—and from the quickness of its fatality in experiments on animals, considered it a most fearful poison. He feared many of its fatal results in private practice have not been made known, and considered if its influence is carried beyond its effects on the cerebrum, its application was certainly dangerous. In cases of asphyxia there are more efforts of expiration than of inspiration. He thought it was ill-judged to have changed from ether to chloroform, as the former is less dangerous, and as capable of producing anæsthesia. If a patient is in a state of anæsthesia, he can be moved; and the mesmeric experiment performed some five years since to produce this state had been confessed to Dr. Hall by the party operated on as all a trick.

Dr. COPLAND asked whether the shock might not be greater when the system is insensible than under other circumstances, and believed it is so; also, that reaction is more salutary when chloroform is not used, for suppose you deaden sensibility, in that case the shock is greater. He thought the source whence the blood came in this case was not ascertained. Under chloroform the irritability of the small vessels being impaired you might have exudation of blood from the bronchi. He inquired whether the shock was great.

Mr. TRACEY had never seen a case of death when the patient had rallied from its effects, or a case where blood got into the lungs from operations about the mouth, although he administered chloroform frequently.

The PRESIDENT asked Dr. SNOW, as the patient never rallied after the cessation of hæmorrhage, what prevented chloroform being the cause of death.

Dr. SNOW in answer said the patient's breathing not being stertorous, or muscles relaxed, and only being in, according to his

division of its effects, the third degree of narcotism (he making five). He quite agreed with Dr. Hall in the order of its effects.

Mr. BARLOW considered there could be no other cause of death. Although the operation itself, the hemorrhage, the extent of the tumor, and oozing of blood after the operation, all shewed the case to be a most severe one, and would of course go greatly against the patient.

Dr. WEBSTER had got an answer to the blood being fluid, but not as to air being found in heart or veins.

Mr. HEWETT answered that there was no air in the heart, but that the veins were not examined.

Mr. CHARLES HAWKINS.—It is a point of great importance as to whether we are to use chloroform; and I consider that the suddenness of death in this case was such as would result from air getting into the veins, and should like to know what the cause of death was. It had appeared to him, until Dr. Copland's observation, that chloroform lessened the shock; but never saw a patient die so suddenly as in the present case.

Mr. CESAR HAWKINS said it was very natural Dr. Snow should throw off the blame from chloroform, but he felt convinced that death was caused from blood getting into the lungs, although this was rare; but that it was evidently increased by chloroform. In St. Bartholomew's and St. George's Hospitals, it had been used in every operation, and no direct death had occurred from its use, but it is never used in teeth extraction or minor operations,—deaths only having happened from its use in the Borough hospitals, twice at Guy's and once at St. Thomas's, and the latter was occasioned by its inexperienced administration.

Mr. SOLLY.—In the fatal case at St. Thomas's, no proper person was appointed to administer it, and without any warning the heart became paralyzed: he always made allowance for cases similar to Mr. Hewett's, in which he considers that loss of blood producing syncope the heart becomes paralyzed. He would not use chloroform in such cases. He concluded by asking whether the patient was sitting or lying?

Mr. HEWETT.—Sitting.

Mr. BENJAMIN PHILLIPS remarked that before the use of chloroform we had a great many cases of sudden deaths.

The PRESIDENT announced that the next meeting of the Society would take place on January the 14th, 1851.

PATHOLOGICAL SOCIETY OF LONDON.

Dr. LATHAM, PRESIDENT.

December, 3, 1850.

MR. HENRY CHARLES JOHNSON exhibited the preparation of

A Bladder, removed from a Man at 72, who had undergone the operation of Lithotripsy more than once, and with which was connected a Large Sac, at its upper part, filled with Small Calculi.

About three years previous to his death (having suffered many years from symptoms of stone in the bladder), he underwent the operation of lithotripsy in St. George's Hospital, and left the hospital apparently well. Prior to the operation the urethra was gradually dilated till it would allow of the passage of a large bougie, size No. 20.

About two years after he quitted hospital the former symptoms of stone returned, and became troublesome; the urine alkaline, and filled with ropy mucus, and being passed constantly; the general health much shattered, and the pain preventing sleep at night.

He was again admitted into the hospital, and, under treatment, somewhat improved, so that the operation of lithotripsy was again performed, and several small calculi were broken up.

For a fortnight after the operation everything went on well, and numerous fragments and sand were daily passed. A month after the operation he suffered more pain in passing the fragments, and the urine was more ammoniacal and ropy; his tongue became moist and dry, and he gradually sank and died about six weeks after the last operation.

Examination.—The kidneys were imbedded in much fat; their surfaces dark, and dotted with points of lymph and pus, the right one being in the most advanced stage of inflammation, with more evidence of pus mixed with the lymph: in its substance was a small abscess filled with pus. The mucous membrane of both pelvis was almost black, and covered with bloody secretion. The ureters were dilated, and their lining membrane in the same condition of congestion and inflammation. The bladder was filled with bloody urine, and much corrugated internally, and the muscular coats hypertrophied; the mucous membrane inflamed. Above and to the right of the bladder was a small pouch, which communicated with its cavity by a contracted opening, and which was filled

with calculi of a triangular shape and soft consistence, formed chiefly of the phosphates; the bladder itself contained some fragments of calculi. The prostate gland was much enlarged. Some fragments of calculi were found impacted in the membranous portion of the urethra; there was no suppuration external to the urethra.

In this specimen Mr. Johnson called the attention of the Society to the circumstance of the impacted portion of stone in the urethra, an accident of frequent occurrence, and which generally requires an incision in the perineum for its relief, but in the present instance would have been of no avail, as the patient died from abscess in the kidney, and did not present the ordinary symptoms of impaction within the urethra.

Mr. POLLOCK exhibited some specimens showing

Portions of Calculi impacted in the Bladder after the operation of Lithotripsy.

One specimen was from a man, aged 62, admitted into St. George's Hospital, under the care of Mr. Caesar Hawkins. He had suffered from symptoms of stone in the bladder for two years. His water was occasionally tinged with blood, and the irritability of the bladder considerable at times. He appeared in good health. The stone was readily detected, and was apparently of considerable size. It was crushed by Mr. Caesar Hawkins without any difficulty, and without much pain; and before the instrument was withdrawn the fragments were broken down ten or twelve times. The stone was a very hard one. One fragment was fixed into the eye of the instrument, and gave pain in its passage through the urethra, meeting with some difficulty in its withdrawal at the orifice. He passed several fragments during the few succeeding days. On the evening of the operation he was attacked with severe rigor, followed by profuse sweating. Three days after the operation he was again attacked with severe rigor, followed by profuse sweating. The catheter was used daily. The urine was alkaline, foetid, and contained much mucus and sand, and was tinged with blood. The bladder was washed out with warm water daily, and many fragments of stone brought away by this means. A succession of severe rigors and profuse perspirations recurred daily till the sixth day, when the irritative fever was considerable: he was better. Subsequently the urine was again dark, alkaline, and passed with much straining. The quantity of blood in the water increased. He died on the twelfth day after the operation.

Examination.

Thorax.—Evidence of slight recent pleurisy on left side. The surface of lung dotted with spots of congestion, in many of which were small specks of lymph or pus. The spots of congestion existed generally throughout the structure of the lung. The right lung was gorged with serum, and much congested, and softened posteriorly. The heart flabby. The blood chiefly fluid.

Abdomen.—The kidneys were flabby in texture, and contained a number of small cysts throughout their entire substance, and in both patches of congestion, in the centre of which were specks of lymph and pus. The bladder much contracted and thickened; its lining membrane thrown into irregular folds, which were slightly coated with phosphate of lime. The mucous membrane greatly inflamed, and that lining the membranous portion of the urethra sloughy and shreddy; small ulcerated openings passed a little way into the surrounding tissue. The bladder contained numerous fragments of calculi; one very large, about a third of the original stone. There were also several small pieces in the membranous portion of the urethra. The prostatic veins were inflamed, and blocked up with coagula: the prostate was much enlarged.

The second specimen was from a man, æt. 32, admitted, under Mr. Cutler, with all the symptoms of stone in the bladder, and for which he underwent the operation of crushing. At the first operation it was broken several times, nor did any disturbance follow. The fragments were again broken down twelve days after the first operation. This time a slight rigor followed the withdrawal of the instrument, and soon afterwards a piece of stone became lodged in the urethra in front of the scrotum, which was cut down upon and removed. A fortnight after the second operation the fragments were again crushed, and several portions were extracted at the time, and much followed through the catheter. Considerable irritative fever followed, with rigors and profuse perspirations, and he died about three weeks after the last operation.

Examination.—Left pleura filled with yellow serum mixed with much lymph. Lung much congested, and contained several foul abscesses. Recent pleurisy on right side. Right lung inflamed, and contained several abscesses. Blood generally fluid.

Abdomen.—Liver natural; kidneys rather large; veins surrounding bladder, prostate, and rectum congested, and many of them filled with pus; a foul abscess in the perineum communicating with the urethra at the membranous portion through several

openings; the bladder contracted; the mucous lining dark coloured; some fragments of calculi in the bladder; the membranous portion of the urethra was ulcerated, and communicated with the abscess in the perineum: about three and a half inches beyond this ulceration another lacerated opening existed in the urethra at its lower part, which communicated externally: this had been made during life to extract the fragment of stone.

Mr. Pollock alluded to a third case in many respects similar to the two preceding ones, in which a man underwent the operation of lithotripsy two or three times, and subsequently died, having suffered from great irritation and constitutional disturbance following the last operation. On examination, a large portion of stone was found fixed in the membranous portion of the urethra, having partly worked its way out through the floor of the urethra, where a large opening existed, communicating with a foul abscess external to the urethra.

The cases presented to the Society were specimens of one of the accidents attendant upon the operation of lithotripsy, and deserved attention as specimens of an accident which, fortunately, did but rarely prove fatal. If the specimens were evidence of any one point in common, they were evidence of the tendency that occurred after this operation for fragments of calculi to become lodged and retained in the urethra after the crushing of the stone in the bladder. The case exhibited by Mr. Henry Charles Johnson was evidence of this fact; but in his case the occurrence was not attended by unfavourable symptoms, nor did it apparently add to any of the causes of death. The urethra in this case had been most amply dilated, and had not been apparently lacerated by endeavouring to withdraw the lithotrite loaded with fragments of calculi.

In the other specimens exhibited the fragments had most probably lodged in some lacerations of the urethra, and had there set up the mischief which was found after death. In this operation every precaution should be taken to avoid injuring the urethra when the lithotrite is withdrawn from the bladder. With every precaution, however, this accident cannot always be avoided, as when a piece of stone is fixed in the eye of the instrument and cannot be removed until the instrument is withdrawn.

The stoppage of a fragment of stone is not an uncommon event after the operation of lithotripsy; but the danger occurs, first, from the stoppage taking place in consequence of a laceration of the urethra, and, secondly, when the stone, not being detected, remains in the urethra. When the stoppage of a portion of stone is detected,

it may be either taken away or pushed back into the bladder, or else removed by an incision. The removal of the impacted portions by incision in either of the cases related by Mr. Pollock would have availed nothing. The operation was proposed, but not performed, as the patients were evidently dying from the effects of absorption of pus.

MEDICAL SOCIETY OF LONDON.

Dec. 14, 1830.

DR. J. R. BENNETT, PRESIDENT.

Aneurism of the Arch of the Aorta.

DR. LANKESTER exhibited an aneurism of the arch of the aorta, extending over the whole of the transverse portion of the arch; the heart being healthy and small, the lungs also healthy. He considered the disease had been between six and seven years in progress, and he had noticed that in proportion to the enlargement of the cyst the sufferings of the patient seemed to diminish. Auscultation discovered the second sound only to be accompanied by a slight *bruit*.

Non-United Fracture—Resection.

MR. GAY produced the opposite ends of an ununited fracture of the femur at the upper third, of twelve months standing. After the trial of various means, he had resorted to the operation of resection. The result not being successful, at the end of three months he amputated. The patient died. The specimen exhibited scarcely any attempts at reparation. The patient before the occurrence of the fracture was a fine, healthy man.

Mr. Gay stated in answer to a question from Mr. Canton, that the *permanently* contracted state of the muscles was the chief obstacle to the re-union of the bone in the various measures tried by him before the last resource to resection, the ends of the bone being thereby drawn away from each other.

MR. HAYNES WALTON read a paper upon

Excision of the Head of the Femur.

After remarking upon the diversity of opinion concerning the propriety of this operation, the author said, the leading question was, at what stage of the disease the operation should be performed. There were two considerations to be taken into account—1, the local; 2, the constitutional. With reference to the first, he thought, when the discharge was excessive, thin, dark, and of bad odour: in respect to the second, when there was much hectic

fever. If, on examination, disease of the internal organs could not be discovered, especially of the lungs, the operation should take place. There was a question whether disease would not sooner or later come on in these from the effects of the local disorder upon the constitution, if the local mischief were not removed.

The author did not consider the acetabulum to be so often diseased in morbus coxae as the head of the femur; and that, when diseased, it had greater power of reparation.

He believed non-dislocation of the head of the femur to be diagnostic of soundness of the acetabulum; and that, by exploratory incision, or by passing the finger through a sinus, the state of the acetabulum might often be discovered. If there were no disease in the acetabulum, the operation would most probably be successful, although cases had turned out well where there had been disease in that portion of the joint. Out of fourteen cases, twelve of which had been collected from different sources—the other two having occurred in his own practice—six had proved fatal: one had died from renal disease, another from hæmorrhage from the profundic vein, another from diarrhoea: the cause of death in the other three was not given.

The operation was in reality much less severe than it appeared to be: the wasted state of the parts facilitated the operation, while the loss of blood was remarkably small.

The author did not advocate removal of the trochanter as well as the head of the bone. The long interrupted splint was the best apparatus to apply after the operation.

Mr. B. TRAVERS gave great weight to the opinions of Cooper, Cline, and Hunter, all of whom were averse to the performance of the operation in question. He thought that confidence should be placed in the reparative powers of the body, and that, if the case were really curable, the operation would not be required. Even if the operation were successful the limb was of but little service. From all he had read, heard, and seen, he was of opinion that the successful cases would have got well without interference with the knife. The specimen that had been handed round showed attempts at reparation had been made in those where the operation had succeeded.

Mr. DAMPIER agreed in the main with Mr. Travers.

Mr. CLARKE did not consider a case to be cured even if the patient lived twelve months after the operation, and that death was hastened by the operation.

Mr. CHALK spoke of the difficulty of diagnosing this disease from lumbar and psoas abscess, and questioned if the disease were removed with the head of the bone.

Mr. GAY could not coincide with Mr. Travers; the process of reparation in bone was so tardy, that the knife should be employed to assist nature, and that by its use much constitutional irritation could be spared the patient. He would not advise the use of the knife when the manifestation of scrofulous disease was very active.

Mr. H. SMITH mentioned the result of some successful cases. One, a boy, æt. 13, operated in 1845, was now hearty and active, and could walk from Holloway to London. Another, operated on two years ago, a female, æt. 13, was seen yesterday by him, and found in a very comfortable condition, and could walk a mile without assistance. Both were Mr. Fergusson's cases. Another, Mr. Morris's (of Spalding) case, operated on in 1849, was quite well, had perfect motion with the thigh, and could walk a short distance.

He had seen mistakes made concerning the position of the head of the bone, and the operation given up in consequence after the first incision had been made. The operation was advisable, because by preventing ankylosis the mal-position of the limb was obviated.

Mr. LLOYD had paid considerable attention to the operation in question. In some cases, but very rare ones, the operation was to be performed: he had seen patients who had died from the effects of the profuse suppuration solely; no disease could be found in the internal organs. He thought that most cases of the disease commenced as synovitis, and not from scrofulous deposit in the head of the bone, which latter cause of disease he looked upon as unfrequent.

Mr. COULSON was of opinion that hip disease was generally of a scrofulous origin, and as amenable to constitutional treatment; that the operation should be put in force at the last stage of the disease, when all hope of recovery by other means was given up; that the constitutional disturbance was not due to the local malady, but both had the same origin.

He stated that four post-mortems at the Margate Infirmary had shown the acetabulum to be extensively diseased. In respect of the cases brought forward by Mr. H. Walton, he thought the operation should not have been performed in many of them.

SURGICAL SOCIETY OF PARIS.

Dec. 4, 1850.

Osteosarcoma on the Upper Extremity—Amputation—Cure.

M. H. LARREY, on behalf of M. Lâcome, of Argelès, exhibited a cast of an arm on which was situated an enormous osteosarcoma. The limb was amputated about four years ago, since which time the man had enjoyed perfect health. The arm, without the forearm, had weighed twenty-three pounds; its circumference at its widest part measured sixty centimetres (= 23·62 Eng. inches).

ACADEMY OF SCIENCES, PARIS.

Dec. 2, 1850.

Repeated Injections of Iodine in the Treatment of Encysted Tumors.

M. BORELLI, of Turin, transmitted a work on this subject, in which he stated his practice. M. Borelli makes a small oblique opening into the cyst, by which he evacuates its contents. He then injects, by means of a syringe, an alcoholic solution of iodine, which he leaves in the cyst, closing the aperture with diachylon or charpie. Pain ensues in the course of two or three minutes, and continues for about twenty-four hours. The inflammation which is excited may usually be allayed by a poultice. The tumor which before was tense and painful becomes softened and diminished in size, while a coloured fluid oozes from the incision. Sometimes, after the first injection, and as soon as the local inflammation has subsided, the cyst becomes detached, and may be extracted from the tumor with the forceps; but usually it is necessary to repeat the injections two or three times before obtaining a complete separation. When the cyst is withdrawn, the radical cure is easily completed.

A Particular Appearance of the Uterus during Menstruation.

M. RIPAULT, of Dijon, stated that he had observed that for a few days before menstruation one or two veins on the anterior surface of the neck of the uterus became enlarged or varicose; that towards the end of the menstrual period they decrease in size, while other veins appear enlarged on the posterior lip; and that with the cessation of the catamenia these appearances altogether subside.

On the Action of Salts of Silver.

M. DELIOLUX, of Rochefort, proposed that, to prevent the discolouration of the skin by nitrate of silver, it should be administered in combination with the iodide; and that to obviate topical irritation the salts of silver should be given in albuminous fluids, with alkaline chlorides, which facilitate their absorption.

On the Treatment of Scabies.

M. BOURGUIGNON transmitted a letter in which he answered the charges of M. Bazin.

Correspondence.

THE CENSUS OF 1851 IN RELATION TO PUBLIC AND PRIVATE LUNATIC ASYLUMS.

SIR,—As the subjoined correspondence between myself and the Registrar-General will be read with interest by a large class of professional gentlemen associated with public and private Lunatic Asylums, I offer no apology for transmitting it to you for publication in your journal.

It certainly would have been most unjustifiably inquisitorial if the proprietors of private asylums for the insane had been compelled to make a return of the names of the patients placed in confidence under their care.—I remain, sir,

Your obedient servant,
FORBES WINSLOW.

Dec. 9th, 1850.

To the Registrar-General.

SIR,—I perceive by the printed form of the Census for 1851, issued, I believe, under your authority, that it will be necessary for each householder to make, agreeably to a prescribed tabular form, an accurate return of the christian and surname, age, occupation in life, &c., of each person sleeping under his roof on a specified night.

As I am much interested in behalf of a numerous and influential section of the medical profession, viz., those engaged in the care and treatment of the insane, may I be permitted to ask whether the resident proprietors or medical superintendents of private and public asylums will be required to make a return of the names of patients placed under their care, and actually resident with them at the period when it is proposed to take the census? Your early attention to this matter will much oblige, sir,

Your obedient servant,
FORBES WINSLOW, M.D.

Albemarle Street, Dec. 5th, 1850.

Census Office, Craig's Court,
Dec. 7, 1850.

SIR,—In reply to your letter of the 5th inst., I have to state that I should think that, rather than return the names of all patients in Lunatic Asylums, it may be sufficient if the initials of their names be recorded when the Census of 1851 is taken, to which I should think there would be no objection.—I have the honour to be, sir,

Your faithful servant,
GEORGE GRAHAM, Registrar-General.
Dr. Forbes Winslow, M.D.

**FEES FROM INSURANCE OFFICES.—THE
STANDARD LIFE ASSURANCE OFFICE.**

SIR,—If it were a personal matter I should not trespass on your space by a reply to Mr. J. R. Fisher's letter, but as the interests of the profession are involved I must needs do so.

Without admitting for one moment Mr. Fisher's explanation of his reason for paying me my fee, or the ingenious turn he has given to his motive for so doing, in requesting me to recommend their office on condition that my fees should be paid, I shall not offer any refutation of the one or explanation of the other, because his admission that he paid a "brother practitioner" a fee when he had not any *charitable motive* for doing so, is sufficient for my argument, "that the Standard Office will pay when sufficiently pressed;" and this position is corroborated by what happened since the letter of which this is a copy was written,—namely, another "Brother Practitioner" (having seen my former letter) told me that he had lately received a fee from the agent of the Standard in a neighbouring town.

It matters little, I presume, to the profession, whether the agent or the office pays the fee (and they will judge for themselves who does,) so that they get it; but to my mind Mr. Fisher has done little for the honour of the Standard Office by his *virtual* admission, that he is so liberally paid that he can afford from his salary or commission rather to pay medical fees or dispense charitable donations, whilst the just demands of the profession are withheld by the Company.

However, the matter is virtually settled; with the Royal Exchange at the head of nearly fifty "paying Offices," the result cannot be doubtful; and if the profession will only be true to itself, which it will, we shall soon have the Standard Office in self-defence not only a *paying*, but a "*recognised paying*" office.

Your obedient servant,

J. H. HOUGHTON.

Dec. 1850.

OBITUARY.

On the 12th inst. at Camden New Town, Robert Calvert, M.D., Deputy-Inspector of Military Hospitals, aged 68.

On the 28th ult., on board the bark Clara, off the Island of St. George, Azores, John Garnett Courtenay, Esq., Staff Surgeon, late principal medical officer of New Zealand.

On the 11th inst., at Uxbridge, in the 70th year of his age, Richard Andrews, Esq., many years surgeon at Great Stanmore, Middlesex, much respected by all who knew him.

Medical Intelligence.

UNIVERSITY OF LONDON.

M.D. EXAMINATION—1850.

Examination for Honours.

Monday, Dec. 25.—Afternoon, 3 to 6.

*Commentary on a Case in Medicina. Cal-
sus de re Medica.*

Examiners, Dr. BILLING and Dr. TWINKLER.

R. W. *et.* 28, when first visited on the 20th of December, stated that he generally enjoyed good health, that his present indisposition, which was of three weeks' duration, and had been preceded for ten or twelve days by slight cough, commenced by severe headache, loss of appetite, thirst, and chilliness succeeded by hot skin and tendency to sweating. The chilliness, which recurred every day at the same hour during the first week, had entirely disappeared. The bowels were at first confined, but for the last few days he had felt gripping pains in the abdomen, accompanied by frequent liquid evacuations from the bowels, and on the previous day he had voided by stool about half a pint of dark-coloured blood.

On the 21st he complained of epigastric tenderness and urgent thirst. The tongue was clean at the edges, brownish in the centre; but there was no headache and no pain of limbs; the pulse was only 76. There was little cough, the respiration was easy, the chest clear and resonant on percussion, and no kind of râle accompanied the respiratory murmur. His manner was calm, and, though he replied to questions rather slowly, his intelligence was unimpaired. In the evening there were slight chills, followed by heat of skin, but no sweating. The accession returned on the next and following days, but a little sooner. On the 25th there was no chill, but the diarrhoea, which had ceased since he was first visited, reappeared, and he had passed fifteen liquid stools. By the remedies administered it was again restrained, though in the next few days it was evident that his powers were much depressed.

On the 1st of January he had frequent bilious vomiting, and passed from eight to ten small liquid dejections. Next day the expression of his countenance was visibly altered, the speech more slow, the pulse quicker, the skin moderately hot: there was cough accompanied with slight mucous expectoration. He had passed only three evacuations from the bowels during the day.

During the night of the 3d and 4th he

suddenly experienced very severe pain in the hypogastrium, so severe as to cause him to utter loud shrieks; it continued with undiminished severity for an hour, and then abated considerably. When visited on the following day the belly was soft, and could be examined without apparently inducing any pain. The features, however, were much contracted, and his general powers very feeble: the pulse 110, respiration frequent, tongue clammy, and he complained of much thirst. On the morning of the 5th his countenance had become more animated, the whole body was covered with a profuse sweat, the abdomen was slightly tender, the tongue white, but his thirst had abated, and he had passed no stool for 24 hours. This apparently calm state was, however, interrupted in the evening by bilious vomiting. In the course of a few hours he complained of feeling very ill, and expired before midnight.

What was the nature of this disease?

What appearances would you expect to find on examination of the body after death?

How would you have treated this disease?

Translate:—

Ubi verò febris aliquem occupavit, scire licet non periclitari, si in latus aut dextrum aut sinistrum, ut ipsi visum est, cubat, cruribus paulùm reductis; qui ferè sani quoque jacentis habitus est; si facillè convertitur, si noctè dormit, interdum vigilat, si ex facili spirat, si non conflictatur; si circa umbilicum et pubem cutis plena est: si præcordia ejus sine ullo sensu doloris æqualiter mollia in utràque parte sunt. Quòd si paulò tumidiore sunt, sed tamen digitis cedunt et non dolent, hæc valetudo, ut spatium aliquod habeat, sic tuta erit. Corpus quoque, quod æqualiter molle et calidum est, quodque æqualiter totum insudet, et cujus febricula eo sudore finitur, securitatem pollicetur. Sternutamentum etiam inter bona indicia est; si cupiditas cibi vel à primo servata, vel etiam post fastidium orta. Neque terrere debet ea febris quæ eodem die finita est; ac ne ea quicquam, quamvis longiore tempore evanuit, tamen ante alteram accessiorem ex toto quievit, sic ut corpus integrum, quod *εισπρηγνύς* Græci vocant, fieret. Si quis autem incidit vomitus, mixtus esse et bile et pituita debet, et in urinâ subsidere album, læve, æquale; sic ut etiam, si quæ quasi nubeculæ innatarint, in imum deferantur; at venter ei qui à periculo tutus est, reddit mollia, figurata, atque eodem ferè tempore quo secundâ valetudine assuevit, modo convenientia his quæ assumentur.

Pejor cita alvus est: sed ne hæc quidem

terrere protinus debet, si matutinis temporibus coacta magis est, aut si, procedente tempore, paulatim contrahitur et rufa est, neque fœditate odoris similem alvum sani hominis excedit. Ac lumbricos quoque aliquos sub finem morbi descendisse nihil nocet. Si inflatio in superioribus partibus dolorem tumoremque facit, bonum signum est sonus ventris inde ad inferiores partes evolutus; magisque etiam si sine difficultate cum stercore exeat.

Case in Midwifery.

Examiner, Dr. RIGBY.

Mrs. B., ætat. 42, mother of ten children.

July 18, 1850, eight months pregnant.

The membranes broke this morning, and there has been more or less discharge of blood; she was at a party last night, and was much oppressed by the heat of the weather; moreover, she slipped down two or three days ago: her bowels have been confined and unhealthy; they have been but partially rectified by medicine; the liver is torpid, and the tongue fissured.

Examination per vaginam.—The vagina is full of clots; the os uteri, which can barely be reached with the finger, is just beginning to dilate: no presenting part can be reached: liquor amnii dribbles away: occasionally she has a slight pain, which increases the discharge.

July 19.—Slept well, had occasional slight pains all day; at about 6 P.M. they increased, and a hand could just be reached, the os uteri being about two-thirds dilated: both it and the os externum were very dilatable, and the attendant passed his hand with great ease: the other hand of the fœtus was found close by that which presented, but it was so soft and pliable that he did not feel certain until he had reached the elbow. On passing towards the abdomen of the child, he found the legs, and brought one down: there was no pulsation in the cord, which passed between the child's legs. When the foot was brought into the vagina, it was left there until pains should come on. In half an hour they returned; more liquor amnii and blood followed; then came the nates of the child with the other foot; the arms, shoulders, and head followed with great ease: the child was dead: there was some hæmorrhage afterwards, which was stopped by ergot, cold, and pressure.

The candidate is requested to make a few remarks on every practical point which may strike him.

Case of Surgery for Commentary.

Examiners, Sir STEPHEN HAMMICK and Mr. HODGSON.

A gentleman, 65 years of age, of robust

stature, but apparently of a phlegmatic temperament, of active habits, moderate in the use of fermented liquors, but addicted to much and good eating, rubbed the skin off the first joint of the left second little toe by wearing a tight boot. He suffered but little inconvenience for a week, when the toe became excessively painful and slightly inflamed. He thought he had an attack of gout; but he had never suffered from that disease, nor had he any other symptoms of it. In the third week from the time of the injury, the pain having continued unabated, the toe became dry, black, and in a gangrenous state, to its metatarsal joint. The pain then ceased, and only a very slight degree of inflammation was observable over the metatarsus. The pulse at the wrist and the movements of the heart were feeble, but no unnatural sound could be detected in the precordial region. In the sixth week the inflammation extended over the metatarsus, accompanied with acute burning deep-seated pain in the foot generally, increased feebleness in the pulse at the wrist and at the heart, dry brown tongue, loss of appetite, and great prostration. In the seventh week gangrenous patches formed on the upper part and on the sole of the foot, preceded by slight swelling and vesications. The gangrenous patches extended, and in the eighth week reached to the ankle, having become partially detached in some places from which offensive putrid matter and sloughs were removed. The powers of the system gradually declined, low delirium and subsequently coma came on, and the patient died at the end of the eighth week from the time when he first perceived the abrasion of the toe.

In commenting on this case you are requested to describe the different forms of gangrene and their causes, symptoms and pathology, both with reference to remote organs and the state of the parts in which those conditions exist. You will also mention the modes of treatment, both general and local, which you would adopt, with your reasons for their employment.

Tuesday, November 26—Morning, 10 to 1.

Medicine.

Examiners, Dr. BILLING and Dr. TWEEDIE.

1. Explain the general principles on which the treatment of dropsy is founded.
2. Describe the different forms of small-pox. Sketch the symptoms, progress, and treatment of confluent small-pox.
3. Explain how pneumo-thorax may arise. Give its physical signs and treatment.
4. Describe the anatomical characters, diagnostic symptoms, and treatment of acute hydrocephalus.

Afternoon, 3 to 6.

Medicine.

Examiners, Dr. BILLING and Dr. TWEEDIE.

1. Describe briefly the conditions of the heart (organic and functional) which are accompanied by abnormal sounds.
2. Enumerate the principal varieties in the expectoration in pulmonary diseases. What are the diagnostic indications to be drawn from each?
3. Sketch the causes, nature, and treatment of anaemia.
4. Describe the anatomical characters, diagnostic symptoms, and treatment of laryngitis.

EXAMINATIONS FOR THE FELLOWSHIP AT THE ROYAL COLLEGE OF SURGEONS.

THE following questions on *Anatomy* and *Physiology*, in addition to those published in our last number, were submitted to the four Candidates:—

1. Describe the minute structure of bone.
2. Describe the apparatus for the excretion of the bile, the composition of the fluid, and the purposes in the economy of this secretion.
3. Describe the ganglia, connected with the three divisions of the fifth cerebral nerve; their size, situations, their roots and offsets.
4. Describe how you would expose the trunk of the internal maxillary artery; its course, relative position, and branches.
5. Describe the disposition of the pelvic fascia in the male.
6. Describe the structure of muscle, explaining the difference between the muscular fibre of organic life and that of animal life.

The following were submitted in *Pathology* and *Surgery*—viz.,

1. Describe the injuries which occur at the shoulder-joint, and how they are to be distinguished from each other.
2. Describe the swellings met with in the groin, and how they are to be distinguished from each other.
3. In what forms of secondary syphilis is the use of mercury advisable? Describe the modes of administering this remedy, indicating that which is to be preferred, and the precautions requisite during a mercurial course.
4. Under what circumstance and to what joints is excision of the articulating extremities applicable? What objections may be taken to this operation?
5. When, in operating for strangulated hernia, the bowel is found in a state of mortification, partially or wholly, what treatment is to be adopted, and how does

recovery take place under these circumstances?

6. Describe the operation for the removal of stone from the urinary bladder by lithotripsy, the cases to which it is adapted, and those to which it is not.

OPERATION FOR CATARACT ON A GRIZZLY BEAR. BY MR. GUTHRIE.

THE patient having been separated from the other bears, a strong leathern collar and chain was buckled round his neck, and the chain being passed round one of the front bars of the cage, two strong men proceeded to pull him to it as a bullock is hauled to the slaughtering ring. The bear was the size of a young donkey, and his resistance was quite remarkable; for full ten minutes he set their efforts at defiance, and ultimately it was only by the united strength of four men that he was placed in a position favourable for the application of chloroform. Dr. Snow endeavoured to hold a sponge to his nose, but it was only by fairly tying it to his muzzle that the object was attained. The dropping of the paws, with the cessation of his roarings and struggles, told that he was insensible; and the sponge being removed, he was laid with his head on a plank outside the den. A solution of atropine had been placed in the eyes on the previous evening, and the pupils were found fully dilated. The instrument used was a strong straight cutting needle. The extraordinary toughness and thickness of the cornea rendered the insertion of this difficult; and the lens was singularly large in proportion to the eye. This rendered the breaking up of that body by no means easy, and it occupied full a minute. The morning was dark, and the margin of the deep brown iris not distinctly seen, which led to its being wounded in one eye and bleeding freely. The cataracts having been thoroughly destroyed, the bear was drawn again into the den. For some minutes he remained in a state of profound coma, giving scarcely any evidence of life, but gradually recovered, and rising with some difficulty he staggered into his sleeping apartment.

On my going to his den the following morning the door of the dormitory was thrown open, and to my surprise he came out with his eyes open, faced the light without the slightest inconvenience, and began licking his paws with perfect satisfaction. A dose of Rochelle salts was given to him rather as a matter of precaution than because they were needed, and his recovery was rapid, not one red vessel discolouring the conjunctiva at any time.

MEDICAL WITNESSES AT THE CENTRAL CRIMINAL COURT.

ACCORDING to an order just issued, medi-

cal men bound by recognizance or subpoenaed and attending professionally, to be allowed one guinea per day, and no more.

No witness to be allowed for his attendance before a committing magistrate, except by the certificate of such magistrate. No witness to be allowed his expenses upon more than one prosecution on any one day, although he may be a witness on several, unless for his attendance before a committing magistrate.

MEDICAL APPOINTMENTS.

MR. ERASMUS WILSON has been appointed chairman of the sectional committee for the exhibition of surgical instruments at the forthcoming display of the industry of all nations. Dr. Stevenson Bushman has been elected senior physician to the Metropolitan Free Hospital. Mr. R. A. Stafford has been appointed by the Council of the Royal College of Surgeons to deliver the annual oration in memory of John Hunter, for the year, 1851.

APOTHECARIES' HALL.

NAMES of gentlemen who passed their examination in the science and practice of medicine, and received certificates to practise, on Thursday, 12th December, 1850:—William Morse Graily Hewitt, Badbury, Swindon, Wilts—Henry Penfold, Brighton—John Chamberlayne Barry, Draycot, Chippenham, Wilts—James Jeken, Dover—Lucius Warrillow, Birmingham—Albert Fleming—Samuel Morris, Colchester—Henry Manley, Crediton, Devon—Thomas Henry Stocker Pullin, Sidmouth, Devon.

The registration of the medical students attending the metropolitan schools has been conducted at Apothecaries' Hall.

1116 students have been registered for the medical session 1850-51,—the largest number since the session 1842-43. Of these 371 are new students.

AN ACCOUNT OF A RECENT EPIDEMIC OF SCARLATINA IN BERLIN.

DR. HELFFT states that the scarlatina which has lately prevailed epidemically in Prussia, and other parts of Germany, has presented several anomalous features, which he deems it his duty to lay before the profession.

In many cases all the symptoms of scarlatina except the eruption were noticed in members of the same families among whom the fully developed disease was present.

In very many patients the disease assumed a malignant character; and here the surface was pale, or the skin presented a dark purple colour, and in others was studded with petechia. In these cases the temperature of the surface

of the body was much lower than natural. The pulse was small, frequent, and sometimes intermittent. The fauces were of a dark red colour. The tonsils presented deep ulcerations, by which they were partially or totally destroyed. Diphtheritis was frequently observed, spreading over the upper part of the pharynx and the fauces, extending to the Eustachian tube and the posterior nares, and accompanied by a copious, acrid and fetid discharge from the mouth and nostrils. The tongue, at first of a dark red colour, its papillæ enlarged, soon became covered with black sordes, which also coated the lips. The lymphatic glands in various regions of the body were swollen and inflamed; those of the neck were swollen in every case; and sometimes the inflammation of these glands proceeded to suppuration and wide-spreading ulceration, extending into the pharynx and adjoining parts. Inflammation of the conjunctiva, with ulceration of the cornea, was noticed in some cases. Œdema of the integuments and effusion into the cavities was seldom met with. In one case hæmorrhage from the nose, mouth, and intestinal canal occurred; and in all the worst cases the evacuations were of a black or dark green colour. The urinary secretion was suppressed in the beginning of almost every case; towards the close, however, it passed involuntarily. In one case, in which after death distinct degeneration of the kidneys was found, no albumen could be detected in the pale and copious urine: its sp. gr. was 1010.

The kidneys in almost every case were found enlarged, or acutely inflamed; but the granular or Bright's disease was not observed. The liver was paler than natural. In one case the spleen was observed of a yellowish colour, and infiltrated with pus. In another case, attended with swelling of the upper extremities, the glands of the axilla were found enlarged and containing pus. The lungs were free from disease. The pleura presented ecchymoses. Various pathological changes were found in the intestinal canal, *e. g.*, softening of the mucous membrane, ulcerations of the mucous membrane of the ileum, paleness of the membrane in other portions of the intestines. In most cases the ileum and colon were of a dark colour, varying through several shades from green to deep black.

The treatment, at first of a tonic character, was not successful; a change to small doses of calomel, as advised by some English authors, was found very serviceable. Subsequently, when hæmorrhages occurred, bark and acids were employed. Nitrate of silver was applied to the local diseases. — *Zeitschrift für die Gesamte Medicin.* X

BIRTHS & DEATHS IN THE METROPOLIS

During the Week ending Saturday, Dec. 14.

BIRTHS.		DEATHS.	
Males....	728	Males....	543
Females..	706	Females..	567
	<hr/> 1434		<hr/> 1090

CAUSES OF DEATH.

ALL CAUSES	1869
SPECIFIED CAUSES	1869
1. <i>Zymotic</i> (or Epidemic, Endemic, Contagious) Diseases	280
<i>Febrile Diseases, viz.—</i>	
1. Dropsy, Cancer, &c.	46
2. Brain, Spinal Marrow, Nerves, and Senses	149
3. Heart and Bloodvessels	46
4. Lungs and organs of Respiration	271
5. Stomach, Liver, &c.	46
6. Diseases of the Kidneys, &c.	17
7. Childbirth, Diseases of Uterus, &c.	12
8. Rheumatism, Diseases of Bones, Joints, &c.	6
9. Skin	10
10. Premature Birth	30
11. Old Age	45
12. Sudden Deaths	7
13. Violence, Privation, Cold, &c.	27

The following is a selection of the numbers of Deaths from the most important special causes:

Small-pox	24	Convulsions	40
Measles	20	Bronchitis	112
Scarlatina	20	Pneumonia	112
Hooing-cough	42	Phthisis	116
Diarrhoea	10	Lungs	14
Cholera	3	Teething	7
Typhus	37	Stomach	5
Dropsy	17	Liver	9
Hydrocephalus	21	Childbirth	7
Apoplexy	20	Uterus	3
Paralysis	20		

REMARKS.—The total number of deaths was 76 below the average mortality of the 50th week of ten previous years.

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer	29.93
Thermometer	40.1
Self-registering do. Max. 59° Min.	21°

* From 12 observations daily. * Sum.

RAIN, in inches, '43.—Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was about the mean of the month.

NOTICES TO CORRESPONDENTS.

We have been compelled to postpone the papers of Dr. Pollock, Dr. Griffith, and Mr. Galloway, as well as the report of the Liverpool Society and other articles, which are in type.

J. M. M., Edinburgh.—We regret that it is not in our power to furnish any other reference than that already given.

We are obliged to the Rev. J. Barlow for the admission ticket.

We thank Mr. H. Janson for his letter and advice. Our opinions of Mesmerism continue unchanged.

REMARKS UPON THE
CRYSTALS OF OXALATE OF LIME
OCCURRING IN THE URINE.

By J. W. GRIFFITH, M.D., F.L.S.

Member of the Royal College of Physicians, and
Physician to the Finsbury Dispensary.

THE crystals of the oxalate of lime belong to the two and one axial or square prismatic system,* and not the cubic or tesseral. The knowledge of this fact enables us to explain the production of the colours, cross, and rings frequently seen on subjecting its various forms to the action of polarised light. The octohedra of the oxalate of lime do not exhibit the phenomena of depolarisation to any extent unless they are large: when so, and especially when united at their edges, as we not infrequently find them, they exhibit the usual colours. Other varieties of oxalate of lime are not infrequently met with, some of which are of a flattened ellipsoidal form, depolarizing light beautifully; others, again, are of this form, but constricted in the middle. The latter have been denominated dumb-bell crystals. These three principal forms, and numerous intermediate varieties, are, as I have stated in my Manual, of the same composition, and exhibit all the chemical characters of oxalate of lime; and they may be prepared artificially from chemically pure oxalate of lime; but, if proper care be not used in washing, and other details of preparation, prior to the application of reagents, all accuracy of investigation, and all certainty, vanish.

I believe that inattention to one or two of such points has led to the adoption of the erroneous idea that the dumb-bell forms of crystal not infrequently met with in the urine consist of oxalurate of lime. There can be no question that this view is totally untenable. The oxalurate of lime is a

somewhat troublesome salt to prepare on account of its solubility in water whereas the oxalate of lime is wholly insoluble in this menstruum. In the statements communicated to the MEDICAL GAZETTE for Oct. 25th, 1850, with a view to show that these dumb-bell crystals consist of oxalurate of lime, there is an evident source of fallacy. Thus we are told that, when they were kept under water, at a temperature of 85°—90°, for ten days, in a greenhouse, the water in which they were immersed became turbid, and evolved a fetid ammoniacal odour. Now I certainly have never made the experiment under such circumstances, but I should have no doubt that, if pure oxalurate of lime were decomposable or decomposed in this manner, the products would be oxalate of lime and oxalate of urea, which substances would require more than a lapse of ten days at this temperature to undergo further decomposition. From no other cause, however, could any fœtor accompany the ammonia evolved than the admixture of some foreign organic matter: this, in the case alluded to, was undoubtedly some remains of the urine from which the crystals had been obtained: hence also the cause of the crystallization of the muriate of ammonia in cubes, granting that the cubes were not chloride of sodium.

Oxalurate of lime has not hitherto been shown to assume the form of the dumb-bell. I have, however, sometimes artificially obtained the prisms of this salt aggregated into a somewhat similar shape, which is not unfrequently met with in the case of many other crystalline compounds; but in the instance of the oxalurate, the crystals of which they consisted have been larger, and more resembling the aigrettes of uric acid than the fine needles of which the dumb-bells of the oxalate consist.

The oxalate of lime is, however, readily obtained in the dumb-bell form by dissolving recently precipitated, chemically pure oxalate of lime in nitric acid, and evaporating the solution rather rapidly. These crystals exhibit the form in perfection, yielding the cross and rings with polarised light. It is quite clear that in this case they cannot possibly contain anything more than oxalic acid, lime, nitric acid, and water. They do not contain any nitric acid, as I have ascertained by

* This is the opinion of M. Schmidt, who has examined the point carefully. I should, however, be inclined to regard the oxalate of lime as belonging to the right prismatic, or 1 and 1 axial system; for the base of the common octohedron is frequently rectangular, and oxalate of lime is easily obtained in the form of flat rhombic plates. M. Schmidt appears to regard the latter as composed of a hydrated salt; but, from the consideration of some circumstances under which I have obtained it, this view does not appear probable. There can, however, be no question that oxalate of lime does not belong to the cubic system.

carefully testing them. When dried at a temperature beginning at 212° , and ceasing at 266° F., they were unchanged in every respect: they exhibit all the chemical characters of the dumb-bell crystals, and of chemical pure oxalate of lime. The only remaining consideration, then, in regard to them is, whether they contain hydrate water: although the fact that no change was produced in them by the elevated temperature to which they were exposed would render this view improbable, yet, as the accurate determination of this point was foreign to the object of the present paper, I have not at present carried the investigation further. It is, however, a common occurrence for an insoluble salt containing no water, when crystallized from an acid solution, to separate in a hydrated form.

Again, the dumb-bell crystals found in urine re-crystallize in the same form from a nitric solution. I have not been able to obtain this result with the oxalurate of lime, although, as already stated, I have with the oxalate. I believe the oxalurate of lime is decomposed by boiling nitric acid. Moreover, when the dumb-bell crystals of oxalate of lime are heated to redness, the remaining mass retains its primitive form, which is not the case with the oxalurate: this fuses, and forms an irregular porous mass, from the union of adjacent crystals in a state of fusion, spaces unoccupied by portions of the substance being left vacant.

I have made many other experiments upon the crystallization of oxalate of lime from various acid solutions, with interesting results; but, as they do not refer to the present paper, I shall omit any account of them.

The dumb-bell crystals of the oxalate of lime, as also those of the oxalate of soda, consist of aggregations of minute needles radiating from a centre: in some cases these prisms may be distinctly seen projecting beyond the general outline formed by the others.

THE MEDICAL SCHOOLS OF CANADA.

WE understand that the following numbers represent approximately the students in attendance at the various medical schools in Canada:—Toronto—King's College, 35; Upper Canada School of Medicine, 10; Dr. Rolph's school, 25. Montreal—McGill College, 47; School of Medicine, 23. Quebec—School of Medicine, 20.—Total, 160.

A CASE OF PORTAL PHLEBITIS, AND CASES OF JAUNDICE.

BY W. B. KESTEVEN, M.R.C.S.

Jaundice—Ascites—Death—Coagula and Pus in the Portal, Splenic, Mesenteric, and Pancreatic Veins—Pathology of the Case—Other Cases of Jaundice—Hæmorrhage from the Bowels.

As carefully recorded cases constitute the *materiel* of medical reasoning, it clearly the duty of practitioners to bring forward such as appear to them to possess features of interest or importance. This circumstance, together with the obscurity which frequently attends the diagnosis of hepatic derangement and chronic structural disease of the liver, and the peculiarities of the following cases, may, it is hoped, sufficiently apologise for their publication, with their accompanying remarks. Besides these reasons, the writer has thought that the case first to be related might be permitted to stand in the relationship of companion to a case of Uterine Phlebitis lately published in the MEDICAL GAZETTE,* with the pathology of which it presents points of resemblance and of contrast. Both cases have the same essential pathological character in consisting of inflammation of veins, while they respectively illustrate the wide difference that may be exhibited by the symptoms and course of diseases arising out of causes essentially similar in their nature.

Inflammation of the portal system of veins is a somewhat rare occurrence. The writer has examined the works of the best English pathologists and systematic authors on medicine that he has at hand, and finds mention of one instance only of a like character, by Dr. Copland. The records of the Pathological Society of London do not supply a parallel case.† In Hasse's Pathological Anatomy,‡ however, he learns that it has been observed more frequently among our Continental neighbours—e.g. twice by Bouillaud, twice by Rehard, once by Dance, once by Belling, twice by Schönbein, once by

* November 30th.

† See LONDON MEDICAL GAZETTE, *passim*.

‡ Published by the Sydenham Society.

Mohr, and once by Hasse himself. The symptoms assigned to this form of disease by Hasse are, briefly, emaciation, ascites, diarrhoea, &c., being nearly the same as noticed in the following imperfect history:—

The writer must here premise that the patient was admitted into St. Bartholomew's Hospital, under the care of Dr. Latham, in the year 1836, at which time the writer was clinical clerk to Dr. Latham. He has had the notes of the post-mortem examination which he then took in his possession since that period and he makes his present report of them by permission of that physician, his highly-esteemed preceptor in *rebus medicis*.

CASE.—M. B., aged thirty-two years, in a jaundiced state, and suffering under ascites, with what was regarded as Peritonitis. All that could be learnt of her history was, that she had been an inveterate spirit-drinker, that she had been jaundiced four months, and that she had had quotidian ague seven months before admission into the hospital. Her end was, that she died comatose a few days afterwards.

Post-mortem examination.—The body was much emaciated: every tissue and organ in the body was tinged with bile. Sero-sanguineous effusion was found in the sac of the arachnoid covering the upper surface of the hemispheres of the brain. On the right side, a thin coagulum covered the convolutions on the superior aspect of the cerebrum. The thoracic organs were all healthy. The abdominal cavity contained several quarts of serous fluid of a deep yellow colour. The peritoneum exhibited no appearance of inflammatory action. The liver was somewhat enlarged, and presented externally a deep red colour, and was studded with small whitish specks, varying in size from that of a pin's head to that of a sweet pea. The latter were much more abundant on the upper than on the lower surface of the liver. On making an incision into the substance of this organ, pus exuded at numerous points, corresponding for the most part with the orifices of portal veins. The gall-bladder was distended with secretion from its mucous membrane, tinged of a yellowish colour, but contained no bile. A few biliary concretions were found within it. The ducts leading to and from the gall-bladder were rendered

imperforate by an indurated state of the areolar tissue where the ducts join, and along their course to the intestine.

The spleen was enlarged and somewhat indurated in its texture. The pancreas was apparently healthy, but tinged of a deep yellow hue. The splenic and mesenteric veins in their chief trunks, with their junction at the portal vein, were filled with pus, which extended throughout the ramifications of this vein in the substance of the liver, and exuded from their orifices when divided. The veins proceeding immediately from the spleen and mesentery did not contain pus, while the primitive branches of the pancreatic veins were found to convey this fluid. Coagula of blood were found in the pancreatic veins, as also in the splenic and mesenteric veins. These coagula were adherent to the inner surface of the vein, or rather to a yellow fibrinous deposit which lined these vessels, as far as the liver, and in the opposite direction as far as the small omentum. The deep yellow colour of this fibrinous deposit was doubtless attributable to the bile which had pervaded and discoloured every tissue in the body. No pus was found in any other veins that were examined.

At a spot about the middle of the course of the trunk of the portal vein, one large coagulum of blood seemed almost to have closed the calibre of the vessel. When cut into, this coagulum presented a laminated appearance, each layer becoming more devoid of red globules, and less dense, until in the centre was found what resembled a small quantity of pus. The other smaller coagula were likewise covered with a deposit of lymph. The internal living membrane of the veins presented in some parts a reddish tinge, especially where the coagula were adherent. Corresponding to these internal adhesions, the outer cellular tissue was thickened, and adherent to the vessel. In some of the smaller portal veins phlebolites were also discovered.

The stomach and intestines presented numerous patches of congestion. All other organs were healthy.

REMARKS.—There can be no room for reasonable doubt that the jaundice, ascites, and death here recorded, were attributable to the obstruction offered to the portal circulation by the presence of the

coagula and pus contained in the portal veins. The source of these products could only have been inflammation of those veins producing the formation of pus at some one or more points, and the subsequent inevitable coagulation of the blood around the globules of pus, which it has been shown by Mr. Lee, in his recently published Jacksonian Prize Essay, it is the property of pus to produce, when mixed with blood, either in or out of the vessels. The coagulum thus formed would become adherent to the first surface with which it came in contact, in the mesenteric, splenic, pancreatic, and portal veins. But clearly as this may appear, after the microscopical examination, the true nature of the disease would doubtless have remained hidden during life, had it lasted even a much longer period. All the symptoms that were observed might have been referred by the most competent authorities to several different pathological conditions, and to each or either of them with great show of probability. Thus they might have been with justice assigned to chronic inflammation of the liver, to simple chronic congestion of the portal system, to chronic degeneration of the hepatic structure, to disease of the gall-bladder or ducts, or their obstruction, to chronic disease of the spleen, or lastly, to disease of the mucous membrane of the duodenum preventing the discharge of bile from the common bile-duct. One or all of these forms of disease being familiar to the practitioner, in connection with the same set of symptoms, it is possible that the real pathological conditions, as revealed on dissection, might not have been conjectured by any who witnessed the case during life.

The readiness with which veins take on the inflammatory process is well known, and is often too well exemplified in the puerperal state. The processes by which obstruction of the vein takes place were not so well known to the profession until the publication of Mr. Lee's experimental researches, by which it is shown that the coagulation of the blood in the veins is a secondary consequence of their inflammation, while the adhesion of the coagulum is an occurrence quite independent of inflammation, although inflammation may subsequently result, and lymph be secreted as its effect, the obstruction having

caused increased action of the internal surface of the vessel, by reason of the impediment offered to the circulation detaining irritating matter in contact with its surface.

The existence of inflammation and pus in so large an extent of the portal venous system as was here divulged by dissection could not have been so long tolerated by the system unless the blood had been slowly undergoing such morbid changes, under the influence of the depraved habits of the patient, that its coagulability, which it is obvious existed at some period of the progress of the case in its integrity, had become so far impaired as to admit the circulation of a very large admixture of pus with itself throughout an extensive district of the vascular system. At what period, or even to what extent, this alteration may have taken place, the history of the case furnishes no data for determination. It may be stated as more than a mere conjecture that the large coagulum found in the trunk of the portal vein was the first to attach itself to the surface of the vessel, and offer obstruction to the circulation. A retrograde circulation would then ensue, throwing blood containing pus globules, the result of inflammatory action excited by the presence of the coagulum, back into the splenic, mesenteric, and pancreatic veins, where coagula were found adherent at several points.

The subsequent dilatation of the portal vein and contraction of the coagulum permitted the transmission and universal diffusion of pus through the portal system which was observed to have obtained when incision was made in the course of those vessels in the substance of the liver, and which had proceeded to the formation on its upper surface of numerous small abscesses in its parenchyma.

Although in such a case the circulation may be carried on by anastomosis, yet it is clear that such diversion was not sufficient to relieve the abdominal circulation otherwise than by serous effusion—ascites. It is also equally clear that the state of the portal circulation did not entirely prevent the secretion of bile, from the fact of its having been found to have pervaded tissue in the body, its entrance into the gall-bladder having been prevented by the effects of chronic inflammation external to the ducts, by which their cavities

were obstructed.* M. Raikew mentions cases in which complete closure of the portal vein, from the presence of pus or coagula, as well as from chronic thickening and contraction of the vessel, were found to coexist with abundant secretion of bile.

Mr. Abernethy† inferred that the hepatic artery was sufficient for the supply of blood requisite to the secretion of bile, from having found the portal vein opening into the inferior *cava* near the emulgent veins, notwithstanding which bile was poured into the intestines and filled the gall-bladder. Mr. Lawrence‡ has also related a similar case. The same fact has been ascertained experimentally by several experimentalists, who have applied ligature on the portal vein in dogs, and other living animals. Müller§ is of opinion that the capillary branches of the hepatic artery and portal veins permit, by their anastomoses, of the secretion of bile from the former vessel. This case, therefore, lends further confirmation to the view which regards the hepatic artery as performing, under certain circumstances, the twofold office of conveying nutritious blood to the liver, and also contributing the pabulum of secretion.

The history of the present case would almost seem to bear out the opinion held by some pathologists, as Armstrong, Breschet, Meli, &c., that intermittent fever is to be referred to phlebitis of the portal system; but although this system suffers in common with other internal organs, there does not appear sufficient evidence to conclude that the *ague* could be regarded as the effect rather than the cause of the latter. In the case before us, the *ague* of which the patient spoke was doubtless merely the occurrence of rigors attending the manifestation of infection of the blood, or the hectic thence resulting. The pathology of intermittent fever points to the derangement of the organs whence originate the roots of the portal veins, as evidenced by enlargement of the spleen; but it certainly affords no proof of its immediate dependence on phlebitis, the effects of which exhibit a far graver character.

* See Edinburgh Monthly Journal, April 1850, for a full abstract of a paper by M. Raikew, read before the Brussels Academy of Medicine.

† Quoted in Edinburgh Journal, p. 363.

‡ Medico-Chirurgical Transactions, vol. v. p. 174.

§ Physiology, translated by Dr. Baly, vol. i. p. 567.

The termination of this unfortunate woman's life was doubtless the immediate effect of the morbid state of the blood, induced by the circulation of pus, superadded to the effects of the previous bad habits of the patient. The effusion of blood within the cranium was a termination, however, common to any form of extensive vascular obstruction; but at the same time it exhibited, at a later period, the usual effects produced on the nervous centres by the presence of pus or morbid matters in the general venous system.

Several other features of this case might furnish the grounds of further remarks; but as the preceding, it is believed, embrace the chief of those that have an immediate reference to the pathology of this case, it may be as well to pass to the notice of the following three interesting cases of jaundice, which have occurred in the writer's private practice, and the origin of which is invested with considerable obscurity.

CASES.—In one family, three girls have each, on arriving successively at about the age of nine years, been the subjects of attacks of jaundice, which have in each instance been attended by the same symptoms. The icteric attack has been ushered in by general malaise and pyrexia; the yellowness of the skin has appeared on the third day, and disappeared in from seven to nine days, leaving slight debility for several weeks. The parents of these children have resided in the same house in an open and healthy situation during these several illnesses; they have since removed to a distance of a few miles, with one female child not yet arrived at the same age; it therefore remains to be seen whether this also shall become the subject of jaundice at the same age; while at the same time the only male child has passed this period without a similar attack. There is no further reason to suppose the existence of an hereditary tendency to this disease than is here stated, as neither of the parents has ever been the subject of the disease.

How can the occurrence of the disease in these cases be explained? Should these be referred to the class of cases which arise out of simple exuberance of the secretion of bile? or, must they be supposed to be owing to some family peculiarity of organization, which the progress of growth causes to inter-

fore with the portal circulation at a particular age? The peculiarity of structure or of nature is the more remarkable, as it must be confined to the females of the family, since their brother, of the age of fourteen, has not exhibited the same phenomena.

Before closing these remarks, it may be worth while to inquire how far disease or obstruction of the portal system may be concerned in certain obscure and intractable forms of hæmorrhage from the bowels, not seldom met with in women who have borne children.

It has occurred to the writer to meet with several cases of obstinate hæmorrhage from the bowels, occurring at uncertain intervals, and varying much in quantity; the blood usually evacuated being of a dark colour, from the chemical action of the secretions of the intestines. In most cases that have occurred to the writer, pain has preceded occasionally, but irregularly, the evacuation of blood, while at other times the purging of blood has taken place with scarcely any warning. In these cases piles or disease of the hæmorrhoidal veins have been excluded, as has also amenorrhœa. The treatment has been difficult and unsatisfactory.

It has appeared to the writer that many of these cases have owed their origin to some disease of the portal system, independent of any interference with the functions of the liver, as jaundice or deficiency of the biliary secretion has not been present. The precise pathological condition to which these are owing is not apparent. In many other cases the source of the hæmorrhage has doubtless been dependent on loss of tone in the vessels of the mucous surface of the intestines.

The quantity of blood thus parted with in the course of a few weeks or months is often very large. The patient assumes an anæmic aspect, as if he suffered from menorrhagia, or puerperal hæmorrhage. In but few cases, however, does this aggravation exist; the menstrual functions are usually normal, and seem to exert little or no influence on the occurrence or amount of the hæmorrhage.

The cases of this kind that the writer has met with have lasted a long time, having existed in one instance as long as fifteen or sixteen years. No oppor-

tunity of making a post-mortem examination has occurred to him. The treatment has been obviously difficult; at times attended with apparent success, then again all efforts have been baffled by a sudden relapse.

CONSIDERATIONS ON THE CLIMATE OF ITALY,

WITH REFERENCE TO THE TREATMENT OF CHRONIC DISEASE.

By JAMES EDWARD POLLOCK, M.D.,

Licentiate of the College of Physicians, and Medical Resident at Rome from 1842 to 1849.

No. III.

BELIEVING that on the comparative integrity of the nutritive function depends the retardation of chronic phthisis in almost all cases, and its cure in the earliest stages, we conceive that too much importance cannot be attached to a careful inquiry made into the state of the chylopoietic apparatus before selecting a marsh climate as a residence, and allaying local irritation in the lungs at the expense of congestion of the digestive organs. We should remember that the natural decarbonising process, and the depuration of accidental impurities from the blood, are shared by the liver, lungs, kidneys, and skin; and that the system is so admirably endowed with a compensating activity, that if one of the elaborators of the surplus material of the economy be deranged, a vicarious and exactly equivalent action is invariably undertaken by some other; that this superfluous waste is more than sufficient to employ for its elaboration the organs remaining healthy, and that their overaction results in engorgements, hypertrophy, and finally in alterations of structure; and that, if in addition to one depurating organ diseased we add the complication of a blood poison of marsh production, which gives rise to supplementary, and therefore difficult processes for its elaboration, we have a formidable state of things to contend against, an increased amount of depuration to be effected, with fewer organs to accomplish it, and a local irritation in one, which should be in a state of increased activity. If it be true that the pulmonary surface offers to morbid atmospheric matters the chief door of ingress to the system, it is natural that

it should also be a chief agent in their elaboration, so extensive an area for the operations of endosmose and exosmose with the blood not presenting itself in any other structure of the body accessible to such agents. Again; the decarbonising function of the lungs is not regarded by us with sufficient attention, yet it is of such vital importance, that it literally admits of no adequate compensation by other organs engaged in a similar task.

The phenomena of asphyxia from mechanical stoppage of the act of respiration should ever be present to us as the type of blood poisoning from retained carbon, and a modification of its symptoms in proportion to the amount of surface thrown out of action may be traced in a regular descent through all affections of the lungs, down to the most minute alterations from a healthy state. Double pneumonia is the nearest approach to a ligature round the trachea in its effects on the blood; general bronchitis comes next; then the one-sided affections. In pleurisy the dyspnoea and distress is more from the pain accompanying the respiratory movements, than from the system feeling a want of aerification; and its diagnosis from a pneumonia may be often facilitated by an observation of the colour of the face, lips, &c., in the latter,—a remark, of course, only applicable to the earlier stages of the pleuritic inflammation,—as in the later the resulting effusion is of itself a cause of non-depuration of the blood by simple diminution, from pressure, of the respiratory surface. Again; in phthisis we must presume that a class of symptoms exists quite distinct from either the local irritation or the constitutional sympathetic fever, but which we do not always distinguish from the latter, and which are referable to non-aerification of the blood, and the resulting retention of a surplus quantity of carbon. To this division belong, to a certain extent, the innutrition of the whole frame, the coldness of the extremities, the loss of rest, and uneasy sensations of the head, the brain being supplied with an impure quality of blood. The great heat of skin observable in many cases indicates the effort made by the cutaneous surface to get rid of the retained carbon; and physiological experiments have left little doubt that an interchange of gases, resembling that taking place in the

pulmonary membrane, does to a certain extent supply the place of the latter, when it is injured or diminished in extent by disease.

These observations may at first sight appear extraneous to the subject of climate, but the possible introduction of a marsh poison into a system whose elaborating powers are already enfeebled, and with one principal organ of depuration diseased, is a consideration of importance; especially as a restoration to health, or even a long endurance of disease, must be the result of an improved quality of the blood, and of strengthened vitality,—a state of things incompatible with the existence in the vital fluid of either a surplus quantity of carbon or of any foreign matter, such as we must suppose the marsh poison to be. To reduce this to practice, the writer can state from observation, that those who are liable to frequent recurring derangements of digestion, especially of the processes of secondary assimilation, or, what is nearly the same thing, to congestion of the liver, kidneys, &c., are not likely to benefit by a residence in Rome or Pisa, nor, as a rule, in any malarious district. The shelter from winds and evenness of temperature which is advisable for the phthical patient of this temperament, is better found in some of our own southern residences possessing these advantages. Derangement of the secondary organs of digestion is so frequent in Southern Italy, that the healthy northern constitution soon feels depressed, and the consumptive are plagued by “bilious attacks,” through which they must either struggle by a difficult and annoying process of elaboration, or have recourse to evacuates, for which they are but little fitted in strength. To this class of patients belong, not only the purely sanguineous who are plethoric, and by high colour, full pulse, and activity of brain and of muscular actions (including the tissues concerned actively in circulation) exhibit the marked features of the temperament, but a far more numerous class, whom we might perhaps designate as possessing the sanguineous diathesis *degenerated*. It is quite possible to have a tendency to overaction of the circulation and a highly excitable nervous system combined with *debility*, a prostration of vital power succeeding its too great exercise, forming, in fact, a feature of

the physiological division of which we are speaking. Debility, in such cases, is apt to be more felt than even by those of constitutionally low tone of fibre, both muscular and nervous. Physiologists are aware of the fact with which the physician in practice is familiar, that acute diseases of severity will often be better borne by those in whom the standard of vital power is uniformly low than by the plethoric or muscular. That such a debilitated state of those endowed naturally with the sanguineous temperament is a frequent—shall we say the most frequent—precursor of the development of tubercle in the English constitution is, we think, borne out by a careful observation of disease. As a nation we are eminently gifted with this temperament, modified by some of its several combinations with the phlegmatic and melancholic; and by it we are tempted to commit excesses in over-exertion of mind and body—mentally, in the race of life ever beginning, never ending, in the competition for wealth and fame by their thousand avenues; and physically, by the bodily craving for the over-stimulating in food and drink to which prolonged activity of mind is so apt to give rise when the cares of the day are over. One nation alone, perhaps, exceeds us in this temperament and its results, and they alone surpass us in the number of consumptive carried off as its victims. Some experience of the American constitution has given the writer unfortunately strong confirmation of this observation. Now the debility which follows the over-exercise of the sanguineous temperament should not be increased by exposure to a *relaxing* climate, and *this class of persons should never be transported to Southern Italy*. These observations on the sanguineous temperament have led to considerations which are easily applicable to various forms of debility; and we would wish to extend our propositions so as to include *all cases of chronic disease with lowered vital powers depending on such alterations of the secondary assimilative functions as are most commonly accompanied by congestion of the liver and kidneys*, and which, of course, are met with in others than individuals of the sanguineous temperament.

For the same reasons as above stated we must confess to the great surprise with which we have seen cases sent to the

South of Italy which were complicated with obscure congestions, manifested by such symptoms as pain in the right side or renal regions, deficient action of the liver, habitual constipation, loaded tongue, yellow conjunctiva, unhealthy stools, cold extremities, weak pulse, and general capillary congestion, with perhaps frequent lithic, oxalic, or other deposits in the urine, and very often by some form of skin eruption; and yet, perhaps many physicians at home would argue that the determination to the surface, which is a known effect of a warmer temperature, and the general stimulus given to the system by the mental as well as bodily occupation afforded by travelling, are sufficient reasons for exporting such cases to a southern air. The second of these ends can be equally attained by travelling in other climates of a more bracing character; but the question whether an increased action of the skin is generally produced by a *winter* residence in any part of the South of Europe is very important, as perhaps no plea is more frequently made use of for sending dyspeptic patients to travel than that of promoting the healthy action of the skin. Nor can we wonder at it, since in the earlier stages of dyspepsia the comfort and energies of the invalid so often depend on the vigour of cutaneous transpiration; and in the more advanced a variety of skin diseases arise, manifestly depending on internal causes, and which, to be relieved by art, must be viewed and treated, not as idiopathic affections, but as symptoms of such derangements.

If the production of diaphoresis, or any less degree of action of the skin, depended on temperature alone, or even principally, there would be a show of reason in thus arguing, although (as we shall afterwards notice) the average *winter* temperature of any part of the South of Europe is absolutely insufficient for this purpose; but as, in fact, the skin capillaries are only part of the great chain of secretive circulation, and the very portion of the circle most dependent on the integrity of the other organs for its uniformity of action, an almost constant want of success attends every effort to promote its secretive powers, unless preceded by an effectual opening of the internal capillaries, be they of the lungs, liver, kidneys, or of the vital mesh spread over the great

mucous canal. The best diaphoretic is, therefore, to cleanse the bowels and restore the internal secretions—a fact daily verified by the observation of the effects of mercury in quickening the action of antimony on the skin, a result which it produces not so much by a specific power over the capillaries, as by freeing the internal obstruction in the secretive circle, and thus permitting the natural action to proceed on the surface, which, if not checked in the liver and kidneys, would (the skin being in a healthy state) have proceeded without interruption. Let us not be misunderstood as undervaluing temperature as an agent in the restoration of the capillary action of the skin; for if the internal congestion be removed, it then becomes of importance as a means of determining to the surface; but we contend that, neither by climate, or by so-called “diaphoretics,” by muscular exercise and frictions, nor even by a high artificial temperature, can we permanently improve the cutaneous functions if internal congestions remain unrelieved. Now the sending a patient with such—be they of the mucous surface, or of the digestive viscera themselves—to a climate where such congestions are produced with facility even on the healthy system, with the hope that the higher and more uniform temperature natural to the place may relieve the skin capillaries which are not in fault, but only evidencing some internal stoppage, is surely as illogical in theory as it is unsuccessful in practice.* That it is unsuccessful in every case presenting the above features, the writer can attest from experience. To sufferers of this kind he has invariably recommended in Rome a *journey to England or Switzerland*, exercise on the mountain side, and avoidance of the plains, abstinence from stimulants, and great moderation in the quantity of food, preceding these measures by an attempt to restore the deranged internal organ by moderate local depletion, and an appropriate me-

dicinal remedy if indicated. That the climate of southern Italy, and of Rome in particular, favour such congestions, is sufficiently proved by their frequent occurrence in the healthy English system exposed to its influence, by the uniform intolerance of even customary stimulants felt by visitors in an ordinary state of health, by the occurrence of headache to an extent which has become proverbial, and of an unusual amount of more serious brain-affections—by the type of the fevers there prevalent, which are either of the pure intermittent form, which we know lead to enlargements of the abdominal viscera as the most common resulting organic lesion; or of a prolonged and intractable gastric character, evidencing congestions of the whole mucous canal. Lastly (and not of least importance), arguing from analogy, by the frequency of urinary deposits, the kidneys being the principal elaborators of poisons received into the system, and such poisons frequently determining the nature of abnormal products in the urine, if they do not absolutely constitute their material.

The above considerations have appeared to us to account for the prevalence of congestive biliary and renal derangements among the English in Rome, and to furnish sufficient reasons to decide us to prefer sending patients in these circumstances to a lighter, purer, and more stimulant air, free from even the chances of marsh influence. Should an irritable state of the bronchial membrane render a mild and sheltered climate advisable, Hastings, Torquay, or Queenstown in Ireland, afford advantageous residences: but should even these prove too relaxing, and that the patient seem depressed rather from want of the nervous power which gives tone to the muscles, vigour to the digestive system, and energy to both mind and body, than from the local irritation in the lung, the philosophic physician will surely not be misled by a name or an exploded theory, but will recommend a removal to an absolutely cold, stimulant, bracing air, such as can only be obtained among mountains, or in a decidedly northern latitude. Careful observation has proved that a calm, dry, cold air, is borne with facility by the respiratory organs. It was only when a wind arose that Ross's men suffered much during their Arctic excursion;

* The great prevalence of certain forms of skin disease in the districts alluded to is another proof of the correctness of this reasoning. Porrigi, in some of its most inveterate forms, is more frequent in Rome than we have seen it in any other city, while an absolutely peculiar affection, not seen elsewhere (pellagra), is localised in the great plains of Lombardy, where the extensive rice-fields, annually flooded and allowed to evaporate under an August sun, are a perfect hot-bed of “malaria.”

and we do not find that the pulmonary apparatus was that most frequently deranged when sickness occurred? A greater vigour of digestion, an increased demand for animal food, and a muscular energy wonderfully sustained, were the healthy results of their exposure. But theirs was an extreme case, and we only use it for illustration. A less degree of cold, and *the absence of currents and moisture*, can be secured elsewhere, if circumstances permit of our sending the invalid to districts where such a climate may be found. We have had, for instance, several opportunities of hearing from the patients themselves experiences on the different effects of the northern and southern parts of the American continent. Indigestion and phthisis, the twin diseases of the United States, are decidedly more prevalent in the central and southern than in the northern districts; and we have received from Canadians and Virginians corresponding accounts of the good influence of the former, and of the pernicious effects of the air in the latter country on individuals debilitated by deranged digestive organs, with a complication of chest affection. We will not enter here on the question how far a cold, dry air, of even temperature, such as Norway affords in the winter, might not be eligible for the consumptive patient, for reasons derived from a consideration of the pulmonary lesion alone, but for the present confine the remark to the good effects of a cold, bracing climate, on the above complication. No better proof of the good effects resulting from a removal from the influence of a sedative, malarious air, could be found than in the advantages which we have often known obtained by removing patients in such circumstances from Rome to the Alban Hill, or to some of the villages on the Sabine and Volscian chain of mountains, where in old days Horace and Cicero, and perhaps all patrician Rome, were wont to retire for pure air from the stagnation of the busy capital.

[To be continued.]

APPOINTMENT.

MR. HIGHLEY, of Fleet Street, has been appointed by the Royal College of Surgeons Bookseller to that Institution.

AN ESSAY ON UNHEALTHY INFLAMMATIONS.

BY M. BROKE GALLWEY, ESQ.

Assistant-Surgeon Royal Regiment of Artillery,
Woolwich, Kent.

[Continued from p. 1014.]

ERYSIPELAS (continued).

SOME of the most original and philosophic remarks that have graced the medical literature of our own country on this disease, will be found in the No. of the *Lancet* for March 1844, from the pen of Dr. Arthur Wilson, of St. George's Hospital. I may say, I believe, that I have pretty nearly waded through everything that has been written, in ancient and modern literature, upon erysipelas; and I know of no author who, in a very small compass, has delivered himself of so much valuable matter upon the *natural history* of the disorder. "Erysipelas," says Dr. W., "may be described as a *severe depressing fever*, lasting from eight to twelve days, and determined, by a special effect of inflammation, to that peculiar organic structure, the integuments of the head and face. Like other fevers, it often supervenes on any of the various causes that induce a bad state of the general health" (p. 11-12). "Like the other eruptive fevers of this country, erysipelas, in bad cases, always becomes typhoid towards its close. Its pathology, by dissection, is that of scarlet fever, which, in its several stages, it very much resembles" (p. 12). Again: "Regarding its symptoms, whether local or general, as a train of actions *tending of necessity to their own relief*, we should, in most cases, be content to watch over their safe development, and to wait patiently for the result, which, in this fever, soon arrives. Idiopathic erysipelas, within ten days from its invasion, seldom fails to cure itself" (p. 12). The following is a severe but just satire upon the thousand and one contrivances devised by physicians to thwart and defeat Nature's efforts when oppressed by this poison:—"In our application, by treatment, of these principles to the local effects of the disease, while we are careful to protect from lasting injury the structures in which its action is most declared, we should continually remember that *in the progress of the eruption is the ad-*

vancement of the cure. It is under an imposed task of swelling, vesication, and excretion, that the skin, which bears the strain of this fever, is enabled to relieve the other vital organs, and, in the end, to maintain its own integrity. How rash and mischievous the interference that would seek to mislead the actions thus determined to the surface, by the introduction of belladonna to the system already charged with morbid poison in the blood,—that would prescribe, in all cases of the disease, an exact limit to its eruptive action, by pencilling the inflamed face and scalp with designs in lunar caustic! The mask which, in erysipelas, the patient is compelled to wear, should never be adapted by his physician!" Again, speaking of the *natural course* of some diseases, and of the fact that their *ebullitions* have a tendency to work out a "good and wholesome result," Dr. Wilson well observes—"The practical application of this principle in the treatment of disease is a continual rebuke to the vanity that would in all cases attribute the interruption or alteration of symptoms to the efficacy of the last prescription. There is no better test of the physician's general character than is afforded by his practice in erysipelas. From the rapidity with which its symptoms are developed (generally to a good end), much of the treatment in this fever is superfluous, yet much affects to be specific: and thus the boaster triumphs in a cure where the true physician is content with acknowledging a result" (p. 13).

There is much more in the paper from which I have been quoting that merits our serious attention. But I have done an injustice to its merits in that part of my essay where they should more appropriately have been noticed—to wit, under the head of what I had to offer upon the morbid condition of the blood in erysipelas; for, the primary object with Dr. Wilson has been to establish the *constitutional* nature of this disease, having its departure, its rallying point, and, indeed, its entire essence, in a blood-poison. I have reason to know that, for years before the publication of his remarks on erysipelas, Dr. Wilson had laboured at St. George's to establish the great truths of the humoral pathology; but that those labours were sneered at by some, and openly ridiculed by others incapable of appre-

ciating their importance. And, it cannot but be gratifying to this estimable* physician to witness the *almost rush* with which the tide of opinion is now setting in this direction. Copernicus was beaten with many stripes for asserting that the earth moved round the sun; and, on rising from beneath the uplifted arm of the oppressor, he still muttered out, "*And it still moves round the sun, for all this.*" Dr. Wilson was nothing daunted by those who inflicted stripes upon his adopted; and his consistent support of it ever since bespeaks the fondness with which, all along, it had lain in his bosom. Verily, he has his reward.

Dr. Graves has an interesting chapter, in the second edition of his work on Clinical Medicine, upon erysipelas, with the merits of which I must detain the reader for a moment. "Erysipelas, you are aware," he says, "is generally looked upon as an inflammatory disease, and its treatment is always more or less antiphlogistic (?) during the early stage. At this period, it is customary to treat it with general bleeding, leeching, scarifications, purgatives, mercury, and tartar emetic; and, I will allow that many cases should be treated in this manner. But, the gentlemen who have attended this hospital

* I trust Dr. Wilson will forgive me for dragging him before the public in this place in a character additional to that of a *writer* who has deserved well at our hands. Having occasion to be at St. George's on two occasions not long after the commencement of the present medical session, I had the pleasure of profiting by a clinical address which this physician happened to be delivering to his class at those moments. And I cannot resist alluding to the feeling and eloquent terms in which, on each occasion, he pleaded with his auditors for tenderness, for sympathy, and for kindness in their dealings with the poor souls who were so much at their mercy within those walls. I wish in these remarks to make no invidious comparison of Dr. Wilson with other public teachers of the day: far from it. I believe kindness to the poor inmates of our public hospitals to be the rule, and the opposite the rare exception. Only, I could fondly wish that our hospital physicians, in their solicitude to interest the pupil in the disease, were more systematically to plead with him for the *patient*, so prone are we, especially by indulging in the habit *early*, to sink the interests of humanity in those of an absorbing science. Having seen Dr. Wilson on these occasions for the first time in my life, I shall not, I am sure, be misinterpreted in the passing tribute here paid to his heart as well as to his head. But, I honour the man who, amid the bustle and anxieties of public life, can halt by the road-side to drop a tear himself, and to ask one from his neighbour, for the sons and daughters of misfortune. And let none of us forget that "he that hath pity upon the poor lendeth unto the *Lord*; and that which he hath given will he pay him again."

within the last three months have witnessed a form of erysipelas which required, from the beginning, an exactly opposite line of treatment. In the management of the cases which fell under our observation, no one in his senses would think of using general or local depletion, purgatives, or tartar emetic. The moment the disease appeared we were obliged to attack it with tonics, narcotics, and stimulants" (p. 321-2). Dr. Graves continues—"I have seen many instances in which this affection appeared in a distinct and well-marked inflammatory form; and I have treated cases with venesection, leeching, purgatives, and tartar emetic, and found these means admirably well fitted to remove the disease" (p. 322). Dr. Graves then introduces two cases, which I humbly here submit as good illustrations of what I would call the *artificial form* of the disease, *which, if not manufactured, is greatly aggravated, by the nimis diligentia* of the physician. "Mrs. B., a lady of middle age, was attacked with feverish symptoms. Notwithstanding the *diligent employment*" (query, *more Mr. Lawrence?*) "of antiphlogistic treatment by Mr. Barker, the pyrexia increased. In the course of a few days her throat became sore, and shortly afterwards erysipelas appeared on the face. Her case assumed a very dangerous aspect. She continued seriously ill for some days, and was saved with some difficulty" (p. 323).*

The son of this lady, a youth of 18, contracted erysipelas whilst closely engaged in attendance upon his mother. While employed in fomenting her, "he got, to use his own expression, 'a whiff of sickening air from the bed-clothes,' and immediately felt unwell."

This second case appears to have been first treated by Mr. Carmichael, who *twice leeches* the patient for headache, and ordered a continuance of "antiphlogistic and aperient medicines." He was "leeches freely twice," at a later period, on the trunk, for some fullness and tenderness, in which locality erysipelas afterwards developed itself. During

the whole of this period he manifested an extraordinary restlessness and nervous excitement, having, in one night, "changed from one bed to another at least one hundred times"—a condition alone, I should say, contraindicating the removal of blood. Well, in less than a week after the last application of leeches (Dr. Graves had been called in in the interim), "nearly the whole of the right side of the abdomen and the scrotum was superficially gangrenous;" and the wind-up of the case may be surmised. To meet that argument which might be raised, in explanation of the gangrenous sequelae, by the *inflammatory* philosophers, I shall add, in the words of the author—"It is worthy of notice that the gangrene did not originate in the excessive violence of the cutaneous inflammation; for, it did not appear in those portions of the skin which were primarily and most violently affected. On the contrary, we observed that the parts which became gangrenous had been paler and less tense than those which did not assume that condition; and that the portions of the skin which died were those which had become engaged in the latter stage of the disease" (p. 325).*

Dr. Graves presently adds—"I am anxious to impress upon the minds of my younger auditors that there are certain forms of disease, termed inflammatory, in which the ordinary treatment by depletion is quite inadmissible" (p. 326).

I know not what Mr. Lawrence, *et hoc genus omne*, will have thought of this *unhandsome* declaration! I strongly recommend a perusal of the rest of this chapter to such practitioners as may still be under the dominion of their early blood-thirsty conceptions in connection with this disease. Dr. Graves does not appear to be acquainted with Dr. Robert Williams's admirable article on "The Poison of Erysipelas,"† if I

* Sauvages, Cl. III. Ord. i. 2, relates the case of a lady who was bled *fourteen times* for a typhoid or gangrenous erysipelas, and yet was not killed by her doctors!

Andral records a case in which erysipelas ensued in the punctured parts in an anasarctous subject, the erysipelas speedily passing into gangrene. In connection with this subject, he says—"La mort fut évidemment le résultat de l'érysipèle gangréneux qui s'empara de la peau de l'une des jambes à la suite des scarifications." "Certes," he adds, "la terminaison de cet érysipèle par gangrène ne sera point ici attribuée à un excès d'inflammation."—Clinique Médicale, Maladies de l'Abdomen, vol. iv. p. 206.

† Elements of Medicine, vol. i.—The Morbid Poisons. By Dr. Robert Williams.

* I have elsewhere endeavoured to show (vide *Lancet*, Nov. 24th, 1849) that Dr. Graves occasionally wields these powerful weapons in very inappropriate cases; whence I look with suspicion upon the *prudence* with which they may have been employed in his hands in erysipelas—a distrust sufficiently excusable, it is hoped, in the writer, who believes them at all times unnecessary, if not injurious.

may judge from the silence which that physician's precept—to treat the disease systematically with wine—has experienced in a didactic address.

"Another way," says Dr. Watson, "in which erysipelas may kill, is by gradual *asthenia*. Without any stupor or much wandering, without any marked affection of the breath, the pulse becomes weaker and weaker, the surface cold, and the heart at length ceases to pulsate"*

"Erysipelas further resembles the disorders of the group in which I have placed it, that it sometimes prevails *epidemically*; and, on those occasions, like the rest of the group, its occurrence is promoted by all the circumstances that tend to debilitate the body,—by intemperance, by previous disease, by low spirits and anxiety, by insufficient nourishment, and by foul air" (p. 831).

In relation to the question of treatment, after stating his opinion that the *in medio* will be found the safest rule in practice, and, in particular, that erysipelas "is not to be *cut short* by any particular mode of treatment,—that it will run a certain course, and that it will *generally* terminate, sooner or later, by resolution, whether remedies be employed or not,"—Dr. Watson adds this observation—"If you look at the history of erysipelas, and of the notions which have *prevailed* respecting it, you will find that the opinions in favour of giving support, and of abstaining as much as possible from the abstraction of blood, greatly preponderate." Again: "In most instances you will soon perceive evidence of great debility,—a *feeble* as well as a *frequent* pulse, tremors, a dry and brown tongue often; and these symptoms increase if you *then* persist in drawing blood. The carbonate of ammonia does great good sometimes in such a condition; and this I am very much in the habit of giving. The bad cases of erysipelas are apt to baffle us all. I am not aware that I lose more of them than my neighbours. Speaking generally, a large majority of my patients get well; but, I do not bleed one patient in a hundred from the arm."

Dr. Watson subsequently states, that "in the purer air of the country, and in young, strong, vigorous country persons, bleeding may be proper and,

requisite" (p. 832-3). Altogether, I am bound to declare that in my opinion very little information is to be gained from Dr. Watson in relation to the management of erysipelas, and that, in the counsel he affords to his readers, there is far too much of the *blowing hot and blowing cold* with the same breath.

"It is always to be remembered," says Chelius, "that erysipelas, even when connected with inflammatory fever, will not bear the same active antiphlogistic treatment as other inflammations, and is especially prone to *return* after any exhaustion."* And Mr. South, in allusion to the above, is still stronger in his condemnation of depletion:—"Bleeding, either from the arm, or by leeches, from the head, is, I believe, very unadvisable. The disease almost invariably occurs in constitutions without power, and therefore bleeding *favours rather than diminishes it*." This is pretty strong, I think, in condemnation of depletion—"Bleeding favours the disease!"

In the course of his Lumleian Lectures, the adynamic nature of erysipelas, and its immediate dependence upon a blood-poison, have been insisted upon by Dr. Todd in the most unqualified terms. Indeed, so forcibly does this eminent writer consider it a disease of low power, that he characterises "*alcohol as an antidote to the erysipelatous poison*," and treats the disorder in all cases and under all circumstances, if I mistake not (for I quote now from memory), with brandy.

Dr. Copland describes erysipelas as "an *asthenic* inflammation depending upon constitutional disorder."† Like Mason Good, he speaks of an "erysipelatous diathesis," which appears to him "to be connected with great irritability or tenderness of the cutaneous surface, and *defective power of the capillary vessels* and secreting surfaces and viscera" (p. 823). Among its most frequent predisposing causes he ranks "feeble, leucophlegmatic, and plethoric habits, unwholesome and low diet, general cachexia, and exhaustion of vital or constitutional power," &c.; and in that portion of his elaborate article on erysipelas which is devoted to the "nature" of the disorder, he observes—"The diffusive character of the

* Principles and Practice of Physic. 2d ed. vol. ii. p. 829.

* Vol. i. p. 106.
† Dictionary of Practical Medicine, vol. i. p. 818.

inflammation, whatever tissue it may attack, is referrible entirely to defective vital power, to the changes in the circulating fluids, and to the imperfect tone of the extreme vessels: these conditions being inadequate to the formation of coagulable lymph: the products of inflammatory action in this state of vital power, and of vascular action and impurity, being a turbid, puriform, ichorous, or sanguineous serum, which produces a septic effect, or poisons the adjoining parts, especially the cellular tissue" (p. 827). Considering that Dr. Copland insists very strongly upon the intrinsically *asthenic* nature of erysipelas, there is, to my mind, some inconsistency in the terms with which we behold him flirting with the lancet in his management of the disease. "In some instances," says he, "large depletions are required; in others, moderate or local depletion only is admissible; and in many, depletion is most injurious, the most energetic tonics being often indispensably necessary" (p. 828). And, in speaking of the "phlegmonoid" form of the disease, he adds—"Depletion and other evacuants should be prescribed with a promptitude and to an extent co-ordinate with each, and with a due regard to the circumstances of the case: always recollecting that, in diseases like this, which are connected more or less with imperfect excretion and a morbid state of the circulating fluids, vascular action may be excessive, whilst nervous power and vital resistance are reduced to the lowest states; and, therefore, that vascular depletion, in order to be salutary, or even not to be injurious, should be rarely employed, and with strict attention to its effects, both at the time and immediately afterwards" (p. 831).

While much of the same diversity of opinion exists in France, as to the true nature of erysipelas, which characterises the professional mind in this country, the balance inclines decidedly, as with ourselves, in favour of its *asthenic* associations. I have already quoted the decided views of Dupuytren as to the necessity which he considered the disease to lie under for copious evacuation. M. Boyer would appear equally favourable to free depletions in the phlegmonous form of the disease—"On proportionne le nombre des saignées à la violence de la maladie, à l'âge du malade, à son tempérament, et à l'état

de ses forces avant le développement de l'érysipèle."* In the other varieties of the disease (of which he enumerates several), M. Boyer counsels his readers to use much circumspection in the employment of depletion. He was much under the dominion of Desault's views as to the immediate dependence of erysipelas, when not arising from a "cause externe," upon a vitiated state of the bile ("elle consiste dans un amas de bile de mauvaise qualité"—p. 10); and considered internal remedies addressed to the liver and first passages as of primary import for its relief.

Lepelletier says that in France the greater number of practitioners are agreed on the danger of bleeding in the majority of cases of erysipelas, and that this operation ought not to be had recourse to but when the inflammation *marche franchement*, the patient *plethoric*, the fever violent, and the pulse full. Blache and Chomel say experience has proved that general bleeding has often no other effect than to blanch the eruption (*pâleur l'éruption*) without notably abridging its duration. Bauquier, in his account of the practice of the hospitals de la Pitié et de Cochin, says that M. Bally abstained altogether from bleeding, or the application of leeches, in his treatment of erysipelas of the face; for he speaks of it "comme propres à aggraver les symptômes, à faciliter l'invasion du délire, à lui donner de l'intensité, et à prolonger la maladie." Bauquier also states that the only cases of erysipelas, according to his observation, attended with danger, were those in which antiphlogistic treatment had been employed.†

Andral gives us the history of a man, who, in good health, began suddenly to manifest a dropsical disposition, which was treated by punctures. The day after this operation "la cuisse droite était devenue le siège d'un vaste érysipèle qui en occupait toute la partie supérieure interne. La douleur qu'éprouvait le malade dans cette partie était assez vive pour lui arracher des cris." On the next day "l'érysipèle s'était étendu; toute la cuisse était dure et douloureuse; les bourses s'étaient prodigieusement tuméfiées depuis la veille; le pouls était très fréquent, la peau chaude."

* *Traité des Maladies Chirurgicales*. Par M. le Baron Boyer. Vol. ii. p. 17.

† *Elements of Medicine*. By Dr. Robert Williams. Vol. i. p. 279-80.

A large slough soon followed. The erysipelas extended to the anterior walls of the abdomen, and the patient died invoking death, his head quite clear, and "le poulx très petit, d'une extrême fréquence." The internal organs manifested so remarkable a deficiency of blood, that M. Andral observes—"On dut dit du cadavre d'un individu mort d'hémorrhagie."

His concluding remark upon this anæmic condition is interesting, and, to our present purpose:—"Enfin, au milieu de cet état anémique, une congestion de sang s'opéra, néanmoins, là où des piqûres, pratiquées pour faire écouler la sérosité, avaient appelé une légère irritation,—preuve, entre mille autres, que la production de l'inflammation ne dépend pas d'un état pléthorique; et que, dans plus d'un cas, comme on la déjà dit, quand même il ne resterait qu'une seule goutte de sang dans l'économie, elle fluait vers le point irrité. C'est là, pour le dire en passant, une des grandes objections que l'on peut faire à la méthode généralement adoptée en France, qui consiste à ne combattre tout travail inflammatoire que par des émissions sanguines plus ou moins abondantes. Il est bien certain, cependant, que si par ce moyen on opère un *déorgement momentané dans la partie inflammée, on ne détruit en aucune manière la cause inconnue sous l'influence de laquelle le sang, soustrait aux lois ordinaires de la circulation, tend à s'accumuler sans cesse le point où existe le travail d'inflammation.*"

[To be continued.]

M. ROYER-COLLARD.

LATELY at Paris, in his 47th year, Dr. Hippolyte Royer Collard, Professor of Hygiene in the Faculty of Paris. The name of the deceased has been long known to men of science in Europe. The French journals are filled with maudlin sentimental orations delivered over the grave of the deceased, but they give no biographical account of the deceased, or his labours. He was born to comparative wealth, and the great stimulus to exertion—necessity, which has carried forward others to the highest rank in the profession, did not exist in his case. M. Royer-Collard has filled the professorship of Hygiene in the Faculty, and has died in the prime of life, regretted by his colleagues and pupils.

* Clinique Médicale—Maladies du Poirtrine, vol. i. p. 123, et seq. The *italics* in the foregoing passage are my own.

MEDICAL GAZETTE.

FRIDAY, DECEMBER 27, 1850.

IN the present number we bring to a close the forty-sixth volume of this Journal. The large portion of our space which is necessarily occupied by the Index, prevents us from doing more than taking a brief retrospective view of its contents. In the LECTURE department of the Journal we have presented to our readers the valuable lectures of PROFESSOR PAGET on Inflammation, and the sequel to the Lumleian lectures of DR. TODD. A course of CLINICAL LECTURES on Practical Medicine has also been contributed by Dr. Todd. We have reason to believe that these lectures have been well received by the profession, as practical and original contributions to medical science. They display careful observation, sound induction, and a thorough knowledge of physiology and pathology. It is with satisfaction we announce that this valuable course will be continued in the new volume. In this department of our publication, we must not omit to enumerate a short but instructive course of lectures on the Medical Jurisprudence of Insanity, by DR. ROBERT JAMIESON, of Aberdeen, and the lectures on Diseases of the Heart, by DR. O'B. BELLINGHAM, of Dublin. The reputation which this gentleman has already acquired by his researches on Diseases of the Heart, and his practical improvements in the treatment of Aneurism, is fully sustained by the course of lectures, of which the first part has just been brought to a close in the present volume. The second part of the course will be devoted to the consideration of special diseases of the Heart. The publication of this part will be for the present postponed.

A large portion of the volume is occupied by ORIGINAL PAPERS contributed by gentlemen whose names are well known to the profession, and they embrace subjects relating to every department of Practical Medicine. They indicate the progress of Medical Science in all its branches, and convey to the practitioner that kind of information which can be supplied only by periodical medical literature.

In the REVIEW department more than one hundred works have been noticed in this volume; and these notices have been uniformly inserted without favour or partiality to authors or publishers. The number of works almost weekly remitted to us for review, is a proof that authors and publishers have confidence in the spirit of justice which prevails in the reviewing department of this journal.

Of the PROCEEDINGS OF MEDICAL SOCIETIES in England and France, we shall only remark, that they constitute a very important part of the contents of a weekly periodical, whereby it must be greatly distinguished from any monthly or quarterly publication. In addition to the papers read at the London Societies, the reader will find a regular weekly summary of the proceedings of the Academies of Medicine and Sciences of Paris, a feature which is peculiar to this journal. The Proceedings of those useful Provincial Societies,—namely, the Liverpool and Newcastle Pathological, are exclusively published in the MEDICAL GAZETTE.

The publication of HOSPITAL REPORTS in the form of cases observed in our great metropolitan hospitals, as well as in the Hospitals of Paris, will be continued in the new volume. We have made arrangements for the weekly insertion of these reports, which will be drawn up by competent observers.

The CORRESPONDENCE and MEDICAL INTELLIGENCE furnish information on

all the current medical topics of the day: and we believe we may confidently state that no event of any interest to medical men has occurred during the last six months which has not been duly recorded in our pages. We shall only remark, in conclusion, that the principles which have hitherto guided us will be carried out in the new volume. The professedly "high moral" tone may present some attractions to sentimental readers who trust to flourishing promises in advertisements; but a medical journal must be judged not by what its editor or publisher promises, but by that which it really contributes to the instruction of medical practitioners and the advancement of medical science.

We have to announce for publication, in the early numbers of the new volume, the following papers, which are now in hand:—"A Series of Cases of Arsenical Poisoning," with remarks by Dr. OOSTON, of Aberdeen; "Notes on the Structure of the Teeth," with engravings, by Mr. C. S. BATE, of Swansea; "On the Morbid Dynamics of the Nervous System," by Dr. THOMAS WILLIAMS, of Swansea; "A case of Lateral Curvature and Excursion of the Spine, with severe Spinal Irritation," by SAMUEL HARE, Esq.; "Observations on the Practice of Physic," by Dr. CHILD, Consulting Physician to the Westminster Dispensary; "On the Duration of Human Pregnancy," by Dr. T. G. MELHUISE; "On the Effects of Concussion of the Spine," by JOHN GRANTHAM, F.R.C.S.E.; "On the Treatment of Hydrocele," by Mr. R. OKE CLARKE; "A Case of Diabetes," by Dr. F. J. BROWN; with a continuation of the papers of Dr. POLLOCK, on the Climate of Italy; of Dr. TILT, on Ovarian Tumors; and of Mr. GALLWEY, on Unhealthy Inflammations.

Medicines.

The Bath Waters: their Uses and Effects in the Cure and Relief of Chronic Diseases. By JAMES TUNSTALL, M.D., &c. 12mo. pp. 144. London: Churchill. 1850.

THE author has observed, as Resident Medical Officer of the Bath Hospital, that the thermal waters of that place are at present almost exclusively used by the poor, and that while the Hospital is full, the Baths and Pump-Rooms are comparatively deserted. The reason that the middle and upper classes do not avail themselves of these baths is, according to Dr. Tunstall, because no work specially devoted to Bath waters has issued from the press for a period of nearly thirty years. Dr. Tunstall, we suppose, is anxious to remedy this defect; and with that view has written this interesting little work, in which the history, properties, and composition of the waters, are all most fully treated of. The diseased conditions in which the use of these waters are admissible, as well as the reverse, and the diet and regimen to be observed under their employment, are all discussed at sufficient length.

An ingenious and novel form of diagram forms a frontispiece to the volume, and shows at a glance the diseases in which the Bath waters are useful, as well as their comparative utility in different diseases.

The study of this work may much assist the practitioner in his recommendation as to change of residence, &c., and may save patients long and expensive continental tours.

A Practical Treatise on the Therapeutic Uses of Terebinthinate Medicines; with Observations on Tubercular Consumption, Gout, Mineral Waters, &c. By THOMAS SMITH, M.D., &c., &c. 8vo. pp. 95. London: Longman. 1850.

THIS volume comprises a history of terebinthinate drugs, with mention of the authors by whom they have been chiefly recommended; their physical and chemical properties, and modes of

preparation; their physiological and therapeutical effects. This portion of the work occupies eighteen pages. Following thereon are forty-seven pages occupied with illustrative cases; the remaining thirty-seven pages are devoted to several Appendices on the subjects indicated on the title-page.

Although we grant that the author has not erroneously named the work a *practical* treatise, we think nevertheless that its size is greatly disproportionate to its contents, which might have been contained in, and been far more useful as, a short pamphlet. The value of terebinthinate remedies has been long admitted by the profession; therefore their efficacy in certain diseases did not require to be established by the large number of cases reported in this work.

A Practical Synopsis of Diseases of the Chest and Air-Passages, with a Review of the several Climates recommended in these Affections. By JAMES BRIGHT, M.D., &c. 12mo. pp. 271. London: Churchill; Hatchard. 1850.

DR. JAMES BRIGHT here submits the results of his experience in the study of diseases of the chest, and of the best means for their alleviation. "In order to enhance the utility of this synopsis," he also favours his readers with twenty-five pages on the A B C of the anatomy and physiology of the organs of respiration, in order to complete which he has availed himself "of the excellent treatises of Dr. Bock, of Leipsic, and of MM. Barth and Roger of Paris, as both are replete with valuable information." Has the author felt obliged to travel so far for elementary works on these subjects, because he could find none in England "replete with information?"

We are not able to point out in this book any features possessing the attraction of novelty, or the importance of originality.

Notice of an Unpublished Manuscript of Harvey. By G. E. PAGET, M.D. Pamphlet. 8vo. pp. 20. London: Longman and Co. 1850.

THE Manuscript here alluded to consists of rough and concise notes, forming a syllabus of a course of Lectures on

the Muscles of the human body. It was found in the Sloane collection of MSS. at the British Museum. Owing to the extreme rarity of Harvey's handwriting, some doubt existed as to the genuineness of this MS., but the writing has been identified to be Harvey's; other circumstances also have been mentioned by Dr. Paget which confirm the authorship. The general observations made by Dr. Paget on the remains of Harvey's writings are highly interesting.

Proceedings of Societies.

MEDICAL SOCIETY OF LONDON.

December 31, 1850.

MR. HIRD in the Chair.

Erysipelas in an Infant.

MR. H. SMITH detailed the particulars of a case of erysipelas in a very young child. The disease was erratic in its course. It commenced in one of the knees, subsequently it attacked the scrotum, producing a slough; after having apparently subsided, it returned in the other knee; the patient eventually recovered. Mr. Smith considered it was an interesting case, on account of the extreme youth of the patient, and the circumstance of recovery from so severe a disease taking place in an infant.

Anencephalous Fetus.

MR. BORLASE CHILDS exhibited a fetus of the above description. The upper portions of the frontal and occipital bones were undeveloped, as well as the squamous portions of the temporal bones. The cranial cavity was occupied by a pulpy vascular mass. Mr. Childs gave the opinions of many authors who had treated of this deformity, and stated that he looked upon the specimen produced by himself to belong to the second of the two varieties described by Geoffroy St. Hilaire.

A communication from Mr. Edwin Lee was read by one of the secretaries upon Functional Paralysis.

[Neither the paper nor the discussion which followed was of a nature which would render a report of it interesting to the reader.—*REP. MED. GAZ.*]

LIVERPOOL MEDICAL AND PATHOLOGICAL SOCIETY.

November 1850.

Calcareous Concretions from the Lungs.

DR. TURNBULL showed some calcareous concretions, varying in size from a pea to a small bean, taken from the lungs of a patient who had died in an advanced stage of phthisis. The right lung contained several of these deposits, and there were also several cavities, apparently of old date, filled by false membrane, almost resembling mucous membrane. The larynx was ulcerated, and the liver was fatty.

DR. TURNBULL thought that the deposition of calcareous matter generally indicated a tendency to recovery, and that in this case there had been recovery from some old previous formation of pulmonary abscesses, which had left calcareous matter behind, and that the patient had been carried off by a more recent attack.

DR. NOTTINGHAM doubted the connection between phthisis and calcareous deposits, as he had found them without any tubercular affection.

MR. BANNER questioned whether they were any thing but deposits in the bronchial glands, which are not uncommon.

DR. TURNBULL was still of opinion that they were connected with pulmonary consumption, because he had found them principally in the upper lobe of the lungs, and in every stage, from soft tubercle, to hard calcareous matter.

MR. BALMAN confirmed this opinion, by stating that he had found sufficient animal matter, even in the hardest of these concretions, when the earthy matter had been removed by muriatic acid, to prove distinctly under the microscope that it was lung tissue.

Death from Caries of a Rib. By

DR. TURNBULL.

The patient died rapidly from an acute attack of pleurisy, following obscure pain in the back, with slight difficulty in making water, and very indistinct fluctuation in the region of the kidneys.

Post-mortem.—The last rib was found to be carious, which had caused the acute pleurisy of which he died; and also a large abscess in the psoas muscle, the occasion of the slight fluctuation.

Air or Water Pessary.

MR. HIGGINSON showed an ingenious contrivance for acting as a pessary, as a plug to check extensive hæmorrhage, or for dilating the os uteri. It consisted of a

common small bladder, into the neck of which a gutta percha tube is inserted and secured. Whilst flaccid the bladder was to be introduced into the vagina, or even so far, if needful, as through a partially dilated os uteri; and an enema pump, or elastic bottle, being fitted to the gutta percha tube, air or water might be injected into the bladder, until it was distended so as to fill the vagina, or to dilate the os uteri to the required degree.

Hydatid Degeneration of the Chorion.

By MR. HAKES.

The patient in this case was supposed from circumstances to be only two months pregnant, though the abdomen was as large as that of a patient after delivery of the child; but before the expulsion of the placenta symptoms of miscarriage came on, which terminated in the expulsion of about three pints of hydatids. No fœtus could be detected in the discharged mass, though it was carefully sought for.

Abscess of Spleen. By MR. HAKES.

Miss W—, æt. 20, more than ten years ago suffered from rheumatism. Two months before death varicella occurred, accompanied with acute fever: on convalescence she went into the country, and took all sorts of exercise without any feeling of ill health, or affection of the respiratory organs. A month before her death she was attacked by a disease resembling ague, rigors followed by sweating, coming on at first twice in the twenty-four hours,—morning and evening,—leaving her well in the intervals; afterwards but once a day, and later still only once every other day. During the paroxysm she was harassed by violent palpitation, and her pulse ranged from 110 upwards; in the interval the pulse sunk to 60 or 90. Towards the close vomiting distressed her.

The cardiac sounds were listened to for the first four days attentively, and afterwards from time to time, but no abnormal sound was heard till about six days before the end, when a murmur with first sound was present, and from that time till death. A few days before death a very slight amount of œdema of the feet and ankles was visible.

Two or three days before death she was seized with acute pain in the foot, followed after a few hours by blackness of the great toe; next in the other foot, with blackness of the little and neighbouring toes; and afterwards a black spot appeared in the palm of the left hand.

No abdominal tenderness could be detected, though sought for with care.

Autopsy, July 27, 7 P.M.—Lungs crepitating, posterior lobes friable, loaded with much frothy mucus; left lung entirely adherent to walls of chest by old adhesions.

Pericardium contained two or three ounces of dark-coloured serum, rather turbid, with flaky or cloudy lymph. No congestion of its vessels. Heart's size natural. Its cavities contained coagula, extending into the veins. Right side healthy.

Great degeneration of valves of the left side. Of the three aortic valves, two were healthy; of the third, nothing appeared natural but its free edge. The lamina of the valve was gone, and had given place to a number of warty-looking soft vegetations, growing from the attached margin of the valve, but allowing free passage of the blood between them, from the aorta into the ventricle. Both surfaces of that lamina of the mitral valve next to the diseased one of the aorta were profusely covered with the same growths, and at one or more points the growths from the two valves were continuous. No increased vascularity, nor staining of any of the valves.

The vegetations were pale straw colour, or very nearly white, very soft, and appeared to consist of concentric laminae.

Spleen of normal size. On its surface, and elevated a little from it, were two straw-coloured projections, elastic, one triangular in shape, the other forming a band across the viscus, three-fourths of an inch in breadth. These were seen in a section to penetrate about an inch into the structure of the spleen, and consist of straw-coloured purulent matter, of the consistency of thick cream.

Liver and kidneys healthy.

Mr. LONG mentioned Rokitsanski's observation of the frequent occurrence of abscess of the spleen along with disease of the valves of the heart, and thought the connection between them might be explained as follows:—The spleen is a reservoir for venous blood before and after digestion, and easily becomes distended with blood. During the cold stage, described in this case, the blood would accumulate in all the viscera, which would be thereby predisposed to disease. The patient then had an attack of endocarditis, according to the history of the case, which caused violent palpitations; thereby affecting the spleen still more, and terminating in abscess of that organ, and degeneration of the valves of the heart.

Dr. HILBERS confirmed the remark about the connection between valvular disease and abscess of the spleen. He had seen cases of it in Vienna.

Dr. INMAN remarked upon the active exercise that the patient had taken shortly before her severe illness, and alluded to the suddenness with which acute symptoms of heart disease sometimes show themselves

after long concealment. A man felt a sudden violent pain in the heart, having been perfectly well up to that time, as far as he knew. He died, and the mitral valve was found to be ruptured.

Dr. NEVINS mentioned another case in confirmation of this:—A navigator fell into a dock whilst excavating it, and was killed on the spot. On examination of his heart, calcareous deposits, the length of the first joint of the little finger, were attached to the border of the mitral valve; yet he had continued at his laborious occupation up to the moment of his death.

ACADEMY OF MEDICINE, PARIS.

Dec. 10, 1850.

Electro-physiological Researches on the Functions and Endowments of the Chorda Tympani.

Dr. DUCHENNE transmitted an analysis of a work on this subject. The author stated that having limited the galvanic excitement to the chorda tympani in a healthy state, there had been produced a sensation of tingling, pricking, or itching in the anterior two thirds of the tongue on the side galvanized. A higher degree of galvanic stimulation had produced a metallic taste on the tongue. During the operation no muscular contraction of the tongue or velum palati, nor any modification of the state of the papilla, or augmentation of the mucous or salivary secretion, took place. These phenomena were not observed where paralysis of the seventh pair existed. From these facts Dr. Duchenne concludes that this nerve is appropriated to the gustatory function of the anterior two thirds of the tongue.

The Treatment of Gout and Rheumatism.

M. LEVRET, of Lyon, addressed to the Academy some conclusions at which he had arrived on the nature and treatment of gout and rheumatism, both of which he considered as being specific diseases, consisting of an inflammatory and a poisonous element; that their causes act directly on the stomach and indirectly on the skin and nervous centres, while the disturbance of these organs reacts sympathetically on the assimilative functions; that its treatment consists in the use of medicines which exert a special action on the elements of the disease; and, that the preparations of colchicum are those specific remedies.

Annual Reports on Epidemics—1849.

M. GAULTIER DE CLAUDEY read the report of the Commission, from which it appeared that seventy-two reports had been received from physicians in the departments.

These reports enumerated five epidemics of dysentery, five of sweating sickness, (*quætie militiaire*), six of typhoid fever, and one of each of the following diseases,—catarrhal fever, bilious fever, intermittent fever, angina couenneuse, and measles.

Transmission of Syphilis from the Nurse to the Infant.

M. CUTLER read an essay which purposed to solve the problem of the law of the manifestation of consecutive symptoms of syphilis in the infant received from its nurse. This essay was divided into two categories of observations, the first consisting of five infected nurses, the children suckled by them remaining healthy. The second category comprised six observations of constitutionally infected infants whose nurses remained free from disease. From the critical analyses of these cases and of many others, the author concluded that syphilis may be communicated to infants equally with adults, and that the contrary opinion is founded upon defective observation, on oversight of certain indispensable details, and in many cases on the incomparably more rapid course of the disease in infants than in adults.

Treatment of Acute Rheumatism by Local Anæsthetics.

Dr. ARAN related the results of his researches on the application of anæsthetics to the joints, in acute rheumatism. A moist compress on which the agent is sprinkled is applied and renewed once in twenty-four hours, being enclosed by impervious bandages so as to prevent its evaporation. It is applied to each joint in succession as it becomes inflamed. Having experimented with various agents, Dr. Aran decides in favour of the *Dutch Liquid*. The relief afforded, as regards the local pain, is very decided, and lasts from one to six or eight hours according to the severity of the attack. The movement of the limbs is restored, and the swelling subsides, and the disease gradually declines in from six to eighteen days according to the duration and acuteness of the disease. The complications of rheumatism may be treated on general principles at the same time.

APOTHECARIES' HALL.

NAMES of gentlemen who passed their examination in the science and practice of medicine, and received certificates to practise, on Thursday, 19th December, 1850:—William Sutherland; Nathaniel Clement Wood, Norfolk; James Hereford Jerwood; Robert Bolton; John Morton, Norfolk; Nathaniel Stenson Wood; Robert Bohun Kidd, Norwich; Robert Rooke France, Adelands; Alfred Beckett, Thame; John Gabriel French, Burton-on-Trent.

Hospital and Infirmary Reports.

ST. BARTHOLOMEW'S HOSPITAL.

Fracture of the Spines of the Cervical Vertebrae—Partial Dislocation of the Axis on the Atlas—Death eleven days after the accident—Post-mortem examination.

JOHN JONES, a strong labouring man, was admitted Nov. 16, 1850, under Mr. Lloyd, with the following severe injuries, caused by a fall from a scaffold twenty feet high:—The scalp over the occiput was lacerated and bruised, and the bone exposed; there was a great deal of effusion of blood beneath the skin, rendering it tense at the back of the neck; and there was great tenderness in this region, especially over the fourth and fifth cervical vertebrae; any attempt to move the head was rigidly opposed by the action of the muscles; sensation and motion in the lower limbs, and a great part of the trunk, was completely lost; the paraplegia extended as high up as within two inches of the nipples; and no involuntary movements could be excited by irritating the legs: the motor power of the upper extremities was unimpaired, the breathing natural, the pulse 60; the pupils acted on exposure to light, he answered slowly when spoken to, and his countenance had a dull heavy appearance.

17th.—The paralysis of the lower part of the body continues, and is attended with priapism; the power of voiding urine is entirely lost; the movements of the right arm are now much impaired, and he says it feels numb; the skin is warm, the pulse 60, and labouring.—He was ordered some aperient medicine, and to be bled to 12 oz. from the arm; and to have the urine drawn off by a catheter.

18th.—The movements of the left arm are now impaired, as well as the right; and there is also diminished sensation in both, extending as far as the clavicles. The breathing is natural, the skin warm, the pulse 70, full and soft; the bowels have not been moved.—He was ordered an enema.

23d.—He has been placed on a water-bed on account of the appearance of redness and vesication on the heels and tuberosities of the ischium. The bowels were moved after several doses of aperient medicine; he has no power of retaining the motions; the paralysis, retention of urine, and priapism, continue the same.

24th.—His face is flushed, he sleeps a great deal, the respiration is rather laboured, the pupils are contracted, there is increased

warmth of the skin throughout the body, the pulse is 90, full and sharp, the sensation about the chest is more impaired.—Rep. V.S. ad 3xij.

26th.—The respiration is more laboured, and the breath escapes with a whiff from the corners of the mouth; he is continually dozing, his face is flushed and sunk, the skin hot and dry, the tongue parched and fissured, the pulse feeble and frequent, bowels relaxed; has had seminal emissions on the last two nights; the priapism and paralysis remain the same as before.—He died on the 27th at 5 P.M.

Post-mortem examination 22 hours after death.—There was fracture of the spines of all the cervical vertebrae except the first and last; the transverse ligament of the atlas was ruptured, and there was rupture of some of the ligaments in front of, and in connection with, the odontoid process, and partial dislocation of the axis from the atlas. The membranes were not ruptured, nor was there any blood effused within them. The spinal cord was congested and softened as low as the sixth dorsal vertebra.

Excision of Soft Cancer of the Breast.

M. K., æt. 55, a married woman, has had four children: discovered, four years ago, a small swelling, about the size of a pea, above the left nipple; it was not painful, but increased in size slowly until eighteen months ago, when she accidentally received a blow upon it, which was followed by the formation of an abscess in the tumor, a more rapid increase of its growth, and subsequently by occasional profuse discharges of blood from some slight abrasions on its surface. Admitted into St. Bartholomew's Hospital, Oct. 7, 1850, under Mr. Lloyd.

Both mammae were large: from the left, near the nipple, there projected a soft tumor, the size of a small orange, with an irregular nodulated surface, of a deep purple colour, and apparently highly vascular, covered by extremely thin skin, and presenting a few deep fissures about its centre, and connected at its base to the mammary gland. It did not give rise to much inconvenience. There were no enlarged glands in the axilla.—Tonics were prescribed, and a lead and opium lotion was applied to the breast: the lotion was afterwards changed for an application of conium.

After she had been in the Hospital a short time, the skin on the surface of the tumor ulcerated, and some deep fissures extended through its centre, from which a thin purulent and foul discharge issued. It occasionally bled freely.

Its removal was not recommended, but undertaken at her own urgent request: accordingly, on Nov. 21, Mr. Lloyd excised the diseased mass, together with the entire

mammary gland. On cutting through the tumor, it was found to be composed of a soft cancerous deposit in the interior of the mammary gland, differing in appearance from ordinary scirrhus of the breast, being of a pinkish colour, soft and friable, and having a defined margin. Some of the healthy structure of the mammary gland still remained around it.

Dec. 4th.—The wound is gradually healing, and the patient doing well.

*Femoral Hernia—Operation—Death—
Post-mortem appearances.*

Ann —, æt. 52, a thin emaciated woman: has been the subject of a femoral rupture two years, has never worn a truss, and has always been able to return the gut herself; but, on Nov. 23, was unable to do so, and was seized with violent pain in the abdomen, and vomiting. She took some aperient medicine, which failed to produce any natural action of the bowels; and on the following day, as the pain and sickness continued, she consulted a surgeon, who, in endeavouring to reduce the hernia, gave her a great deal of pain.

25th.—A portion of the intestine was returned into the abdomen; but the symptoms of strangulation, though somewhat less severe, still continued, and in the afternoon she came to St. Bartholomew's Hospital, and was admitted under Mr. Lawrence. She was much exhausted, and had an extremely anxious appearance. The tongue was furred, the pulse quick and very feeble; the abdomen was tense, distended with flatus, and intolerant of pressure; the rupture was rather soft, about half the size of a pigeon's egg; the skin over it was red, and she said it was not so tender as it had been; she had passed no motion since the morning of the 23d; and she continued to vomit frequently a dark-brown offensive fluid.

The condition of the patient, and the too great probability that the intestine had already suffered severely from tight constriction, rendered it necessary that an operation for its relief should be performed without delay. Owing to the very feeble state of the pulse, no chloroform was given. Mr. Lawrence divided the coverings of the hernia in the usual way, and, on opening the sac, some dark fluid escaped: the stricture was so tight, that a small director was introduced beneath it with difficulty; the intestine was very dark, but smooth on its surface, and had not lost its vitality, and was therefore returned into the abdomen on the division of the stricture. After the operation the vomiting ceased and the pain was relieved: the patient, however, did not rally, and died at 5 p.m. on the 26th.

Post-mortem examination.—The intes-

tines were covered by a recent layer of lymph, and there were about six ounces of turbid fluid in the abdomen: a small oval portion of the wall of the ileum, including the greater portion but not the entire circumference of the gut, had formed the contents of the rupture; was of a dark livid colour, had lost its smoothness on the peritoneal surface, and was surrounded by a deep mark caused by the stricture. The mucous membrane in the interior of this portion of the intestine was soft and pulpy, and presented a line of superficial ulceration corresponding to the situation of the mark caused by the stricture on the peritoneal surface of the intestine. Two inches of the intestine above this point were also dark and discoloured, but in a less degree, and had most probably formed the additional contents of the rupture when the symptoms commenced, and was that portion which was returned on the 25th, before her admission.

Correspondence.

CONVENTION OF POOR LAW MEDICAL OFFICERS.

SIR,—The Committee have reason to believe that Lord John Russell has expressed a desire to obtain information on the subject of Poor Law Medical Relief, and that he entertains opinions favourable to the objects of the Convention; they have, therefore, with the view of improving this auspicious indication, determined to seek an interview by Deputation, in order to present to his Lordship a Memorial setting forth the grievances under which the Union Surgeons labour. Should an interview be granted, the Committee purpose inviting those Members of Parliament, who have already taken an interest in the subject, to accompany the Deputation, and also Delegates from Provincial Medical Societies.

It is hoped that all in the Profession who have access to Members of Parliament will immediately draw special attention to the subject, in order to secure their co-operation in the objects of the Memorial.

The Committee trust that by a direct appeal to his Lordship they may induce him to call the attention of the President of the Poor Law Board to the evils complained of, so that an amelioration of the present system may be effected, or, if necessary, that the President be advised to seek from Parliament an extension of power, to enable him to amend the regulations under which medical relief is now administered through the Poor Law Board.

Your obedient servant,

C. F. J. LORD, Hon Sec.

4, Hanover Square,
December 23, 1850.

Medical Intelligence.

THE RAVAGES OF THE CHOLERA AT KINGSTON IN JAMAICA.

THE latest accounts brought from Jamaica were up to the 14th November, and from them we find that the Cholera has prevailed in a most severe form in the chief towns of the Colony. It first appeared in Port Royal, where it has now almost entirely disappeared, having previously cut off *a fourth of the entire population*. In Kingston the havoc was frightful; so great indeed, that the faculty had ceased to report it officially to the Board of Health. Some say that the absence of official information is owing to the inability of the medical gentlemen to ascertain the precise number of deaths, whilst others attribute it to a prudent disinclination to excite further the public mind. The whole community were panic-stricken. In one day the interments reached 203; indeed, on very good authority, they are said to have greatly out-numbered even this, but we refrain from supplying figures in the absence of authentic information. To add to the terror, it was made known that the most requisite medicines were not in the island, or rather that the supply had become suddenly exhausted. Immediately on representation of this being made to Commodore Bennett, at Port Royal, he dispatched a vessel of war, the Alban steamer, to St. Jago de Cuba, and in less than 60 hours she had returned with the supply. The Board of Health were doing the utmost in their power to administer to the relief of the afflicted, and to provide for the destitute, whose poverty rendered them a ready prey for the plague. Hitherto the mortality was confined to the districts of the poorer classes, but during the few days prior to the packet's leaving, its spread appeared more general, although, perhaps, in some cases in a milder form. In addition to the exertions of the Board of Health, which is composed principally of the clergy and the faculty, the merchants of Kingston had formed a benevolent society, and were most actively engaged in doing all that humanity could dictate in so trying an emergency. On the morning of the packet's departure things hardly looked so gloomy, and it is said that during the previous day and night the dead-carts had had a rest.

Wretched and desponding as is the description we have given above of Kingston, our accounts represent the state of St. Catherine's, the seat of Government, as far more distressing. There, although the actual number of deaths did not amount to those in Kingston, still, in proportion, the

mortality had been greater. Entire families had been seized with death by the merciless destroyer. At one period, it is said, on the best authority, there were lying at the burial places 70 bodies confined without a person to inter them, and but for the judicious conduct of Sir Charles Grey, the Governor, it is hard to state what might have been the consequence. His Excellency ordered the painful work to be performed by 20 of the convicts of the district prison, who did it on condition of their sentences being commuted. On another occasion his Excellency, it is reported, caused some of the troops to perform a similar task.

In the country parishes, too, the scourge was extending itself rapidly. Our information is derived from the very best private sources. Metcalfe, of all the out-parishes, may be said to have been the most distressed, and in the entire parish there was but one doctor. In other parishes there were no medical men. St. Andrew's parish, which joins immediately on Kingston, had lost large numbers of its inhabitants. In Metcalfe, as in many other parishes, the greatest difficulty was felt in procuring people to bury the dead. On the north side every precautionary step was being taken. One case, not fatal, had appeared in Trelawny. Wednesday, the 13th of November, was observed in Kingston, St. Andrew's, St. Catherine's, and Port Royal, as another day of prayer and humiliation, in consequence of the day appointed by the Governor (Nov. 1) being a most unpropitious one.

As might well be expected, business was entirely "at a standstill"—every department was most unsettled. In Spanish Town, at one period, most of the places of business were closed, being without servants and attendants, who had either died or had deaths within their families.

In Kingston the heat during the day was oppressive—the mornings and evenings were more pleasant; the rains appeared to have ceased entirely. Almost daily, heavy black clouds overhung the town, but up to the departure of the steamer no rain had fallen for some days.

Private letters state that it is impossible to convey an idea of the excitement and alarm which prevail, or of the gloomy appearance of Kingston—indeed, of the whole island. In the few parishes where the scourge had not made its appearance, prompt sanitary regulations were being made. There were a few other deaths besides those from cholera.

The intelligence by the last mail from Jamaica, which extends to the 28th of November, is quite as gloomy as that above given. The cholera had abated in Kingston

very much, and might be said entirely to have disappeared from Port Royal and Spanish Town, but it had appeared in a most malignant form in almost every other part of the island. In the mountain districts the mortality was fearful, but there existed no possibility of ascertaining accurately the number of victims. In consequence of the spread of the epidemic the House of Assembly had suspended business until the 10th of December: they reassembled on the 19th of November with barely a quorum present.

THE PROSPECTS OF PHARMACY. RETURN OF MR. JACOB BELL FOR ST. ALBAN'S.

It is with satisfaction we announce that Mr. Jacob Bell, well known as the Editor of the *Pharmaceutical Journal*, has been returned member for Saint Alban's, by a majority of 129. We think that the introduction of this gentleman into the House of Commons will be beneficial to the interests, not only of those whose rights he has specially and ably advocated—namely, Pharmaceutical practitioners, but of the medical profession in general. We may anticipate from the exertions of the honourable member some good results in reference to a Medical Reform Measure, and the proposed separation of drug-dealing from medical practice: also a Restriction of Sale Poisons Bill, and other subjects of which the very large proportion of lawyers, merchants, and magistrates, who now occupy the Benches of the House, have no practical knowledge.

ROYAL COLLEGE OF SURGEONS.

The following gentlemen having undergone the necessary examinations for the diploma, were admitted members of the College at the meeting of the Court of Examiners on the 20th inst.:—A. Carson; R. Wall; R. Sproule; T. J. E. Brown; R. Roberts; T. J. Jones; J. Gibson; N. W. Jones; J. Meane; S. Hudson; and T. B. Brindloss.

BOOKS & PERIODICALS RECEIVED FOR REVIEW

DURING THE LAST THREE WEEKS.

University Reform: Letters to the Right Hon. the Earl of Aberdeen. By A. Kilgour, M.D.

The Boston Medical and Surgical Journal. October and November, 1850.

Lectures on Clinical Medicine. By J. H. Bennett, M.D. F.R.S.E. &c.

New York Journal of Medicine. Nov.

The New York Register of Medicine and Pharmacy. Nos. 4 and 5, November and December 1850.

Journal de Chimie Médicale. Decembre. Comptes Rendus. Nos. 21 and 22, 18th and 25th November, 1850.

BIRTHS & DEATHS IN THE METROPOLIS

During the Week ending Saturday, Dec. 21.

BIRTHS.		DEATHS.	
Males....	720	Males....	564
Females..	748	Females..	602
1477		1166	

CAUSES OF DEATH.

ALL CAUSES	1166
SPECIFIED CAUSES	1166
1. Zymotic (or Epidemic, Endemic, Contagious) Diseases....	231
<i>Sporadic Diseases, viz.—</i>	
1. Dropsy, Cancer, &c.	48
2. Brain, Spinal Marrow, Nerves, and Senses	149
4. Heart and Bloodvessels.....	31
5. Lungs and organs of Respiration	274
6. Stomach, Liver, &c.	50
7. Diseases of the Kidneys, &c.	13
8. Childbirth, Diseases of Uterus, &c.	11
9. Rheumatism, Diseases of Bones, Joints, &c.	6
10. Skin.....	0
11. Premature Birth.....	30
12. Old Age.....	9
13. Sudden Deaths.....	60
14. Violence, Privation, Cold, &c....	17

The following is a selection of the numbers of Deaths from the most important special causes:

Small-pox.....	25	Convulsions.....	55
Measles.....	21	Bronchitis.....	102
Scarlatina.....	30	Pneumonia.....	117
Whooping-cough.....	52	Phthisis.....	112
Diarrhoea.....	16	Lungs.....	11
Cholera.....	1	Teething.....	10
Typhus.....	48	Stomach.....	6
Dropsy.....	14	Liver.....	12
Hydrocephalus.....	25		
Apoplexy.....	34	Childbirth.....	8
Paralysis.....	19	Uterus.....	1

REMARKS.—The total number of deaths was 20 above the average mortality of the 51st week of ten previous years.

METEOROLOGICAL SUMMARY.

Mean Height of the Barometer 29.41
 Thermometer " 28.6
 Self-registering do. " Max. 0.0 Min. 9°
 From 12 observations daily. Sun.

RAIN, in inches, .71.—Sum of the daily observations taken at 9 o'clock.

METEOROLOGICAL.—The mean temperature of the week was 1° below the mean of the month.

NOTICES TO CORRESPONDENTS.

The Vichy Waters.—"A." We do not know.—"B." Apply to Baillière, 219, Regent Street.
 Dr. T. G. Melhuish's interesting remarks on Human Gestation will be inserted in an early number.

The communications of Dr. Fleming and Dr. O'B. Bellingham have safely reached us. The papers shall, if possible, be inserted in the first number of the new volume.

Mr. Herbert Williams.—We will take an early opportunity of publishing the Bill, or such parts of it as require publication. Our next number is already filled according to engagements previously entered into.

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